
EX LIBRIS
G Brown Goode
SIBI
ET AMICIS
AIX 1634 BARBADOES NEW ENGLAND
NEW JERSEY VIRGINIA OHIO
Ffor hym was lefere to have at hese bedis hed
Twenty bokis I-clad in blak or red
Of Aristotle & his philoso-phie
Than robis ryche or fedele or gay sautrie
S. I.
U.S. National Museum
S. I.

Nature

A WEEKLY

ILLUSTRATED JOURNAL OF SCIENCE

VOLUME XXXIV

MAY 1886 to OCTOBER 1886

" *To the solid ground*
Of Nature trusts the mind which builds for aye."—WORDSWORTH

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NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

" To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

THURSDAY, MAY 6, 1886

HOMER'S SENSE OF COLOUR

Le Sens des Couleurs chez Homère. By Dr. Alb. de Keersmaecker. Part I., xii. + 152 pp. (London: Trübner, 1885.)

THIS appears to be Part I. of a monograph on the colour-sense developed in ancient times, although chiefly based on the language of the Homeric poems. It is to a large extent a criticism of the essays of Mr. Gladstone and Dr. Magnus (of Breslau) on this subject. It is generally admitted that the colour-nomenclature of the Homeric poems is far less copious and less precise than that of modern times. Various theories have been proposed about this. The author represents (p. 22, &c.) Mr. Gladstone's view to be that Homer's *perception* of colour was ill-defined, and that his so-called colour-terms are often really descriptive of luminosity rather than colour. And he describes (p. 6, &c.) that of Dr. Magnus similarly, with the addition that the human eye was in those days less perfect in colour-perception than now, and has gradually improved to its present state.

After a lengthy criticism of these and other theories, the author's conclusions are briefly that there is no evidence of any improvement in the human eye itself during these ages, and that the progress that had taken place is solely one of human knowledge of colour: also that Homer's colour-terms are probably often vague, but not more so than is admissible in poetry.

As to evolution, however, the author goes much further: he lays down broadly (p. 32) that no change has taken place in any organ in any species, and most certainly not in man. It is strange that he also expresses himself as a follower of Darwin. After this it will not seem strange that the general argument is not particularly convincing: the mode of argument, too, is not pleasant; in fact the author pleads guilty to a certain sharpness of manner (*acescence de la forme*) in his criticism of persons.

Waiving however the form, there is much in the matter that is interesting. A short account (9 pages) is first given of what is known of Homer's life, and it is argued that Homer—as being an illegitimate child—was constitutionally liable to the infirmity of blindness traditionally ascribed to him: it is fairly urged by some that this blindness, coupled perhaps with *colour-blindness*, may itself be responsible for some of the uncertainty attached to Homer's colour-terms; but the conclusion is that there is no evidence of colour-blindness.

As to the misuse (?) of colour-names sometimes ascribed to Homer, the author points out (with numerous quotations) that the usage of modern French and English poets is often, to say the least, inexact, so that it is absurd to expect exactitude of application in ancient poetry.

Among the detailed criticisms on Mr. Gladstone's essays may be noticed the following:—Exception had been taken to the use of the word φοῖνιξ (usually translated *red*) as descriptive of a horse; hereon it has been urged (by Mr. Prior in NATURE) that this word should here be translated *Phœnician;* but, if this be really a colour-term in this place, why not translate it as a *bay* or *chestnut* (horse) if the term *red* jars on the English ear? Again, surely a poet may describe a (mythical) serpent as δαφοινός (red ?), and the (mythical) ambrosia as ῥοδόεις (rosy ?) without being called to account. In one case (Odys. B. VI. v. 163), where the use of φοῖνιξ as a colour-term had been objected to, the author explains that its other meaning, *palm-tree*, makes sense of the passage. Special exception is taken to Mr. Gladstone's interpretation of αἴθοψ, which the author considers to be not a colour-term but a word descriptive of combustion, so covering a wide range of meanings, *e.g.* fiery, glowing, smoky, golden, &c.

The Homeric expression οἴνοψ applied to the sea—hitherto far from clear—receives a new explanation from a traveller in the Ægean, viz. that that sea has at times a blood-red appearance with a red horizon all round. In commenting on the word χλωρός, which seems to mean both *green* and *fresh* or vigorous, the author endeavours to connect the syllable ι with the meaning of vigour, *e.g. harit* (Sanskr.), *sairit* (Zend), ἶφι (Greek), *vi* (Lat.), (to which may surely be added the English *might*), but the connection seems very slight; in fact the syllable ι recurs in many terms expressing weakness or smallness, *e.g.* μικρός, *minimus*, *slight*, *schlimm*, *weich*, *faible*.

The remarkable fact is brought to notice that the term *sky-blue* is almost unknown in the ancient writings of any Asiatic people, *e.g.* in the Vedic hymns, in the Zend-Avesta, in the Old Testament, in Hebrew writings generally, and in Homer and Hesiod; the epithets applied to the sky being expressive of its vastness, depth, purity, brilliancy, &c., but not of its colour. A similar want of a precise colour-term is shown to exist in many modern barbarous languages. But it does not seem warrantable to conclude that sky-blue was a colour unknown to these peoples; indeed sky-blue pigments have been found (p. 37) at both Memphis and Thebes.

A part of Dr. Magnus's theory of the evolution of the colour-sense is that the eye acquired the power of recognising different colours in the order of their luminosity; but the order which he seems to assign (p. 71), viz. red, yellow, &c., is certainly not that of their luminosity. The physiological and emotional effects of colours on men and animals are noticed in this connection. Thus red is known to excite bulls and turkeys: the experiments of M. Paul Bert on the small crustacean Daphnia are quoted; when placed in a solar spectrum they congregate most thickly in the orange to green region, which is also the most luminous region. Goethe's speculations on the effects of colour on the emotions of mankind are noticed at length. A curious "colour-treatment" (chromo-photothérapie) proposed for the insane is also mentioned, which consists in placing the patients amidst surroundings of a tint supposed to be capable of exciting healthful effects: thus red is said to excite, blue and violet to sadden, green to soothe. The results of this treatment do not seem to have been very definite (pp. 78, 79).

The comparative philology of colour-terms takes up—as might be expected—much of the work; the author has spared no pains in endeavouring to trace out the meanings of Homer's colour-terms by the help of the related words in other languages. As to the uncertainties of this process, take the words related to *blue* as an instance. Mr. L. Geiger's opinion is quoted (p. 50) that the modern European words *blue*, *blae*, *blau*, *blå*, *bleu*, &c. (English, Scotch, German, Danish, French), now meaning *blue*, meant *black* in early Europe, whilst another (p. 101) connects them with words conveying the idea of brightness, *e.g. briller*, *blanc*, *blink*, *bleach*, *blank*.

The author promises a further instalment of this essay, in which the evidence from the fine arts, pottery, and dyers' work, and that from morphology and physiology are to be set forth; also a full statement of conclusions.

ALLAN CUNNINGHAM (Major, R.E.)

OUR BOOK SHELF

The Journal of the Engineering Society of the Lehigh University, March, 1886.)

THE practice of forming engineering societies in universities where engineering is taught is an exceedingly good one, and should receive every encouragement and help from the authorities. In fact every college should have its society. The meetings give the students an opportunity of discussing interesting engineering works, and give them a greater interest in the subject-matter taught in the class-room. These junior engineering societies, if I may so call them, ought not to be only found in colleges, but all large engineering works should have a society of their own, the members of which should include those of the pupils, apprentices, and men who are anxious to improve themselves by the reading and discussing of papers prepared in rotation by the members themselves. Visits to other works might also be arranged. No doubt the formation of such societies may seem very hard to accomplish, but in most works there will be found men willing and anxious to form such societies and to keep them going until their utility is recognised.

The *Journal* before us contains several articles of an interesting nature, the first being by Prof. Merriman on "The Internal Work and the Deflection of Beams"; the second article gives an account of "Boring the Big Aqueduct" for the New York water-supply from Croton Lake. We next have a short notice on technical education in Mexico, followed by a very good account dealing with "The Requisites of a Successful Engineer.'

After notices on "Mine Water Formations" and "The Foundations of the Washington Monument," the *Journal* concludes with a condensed report dealing with the measurements necessary to ascertain "the velocity and discharge of the Lehigh River about Bethlehem."

Taken as a whole the contents of this *Journal* are disappointing from a professional point of view, Prof. Merriman's article on the deflection of beams being excepted. The descriptions are much too general and popular; the subjects are not treated with that accuracy demanded by an engineering article, and are written in a style more fitted for the columns of a daily paper than a journal published by an engineering society.

N. J. L.

Fresenius's Quantitative Analysis. Parts I. and II. Vol. II Translated by C. E. Groves, F.R.S. From New Edition of Fresenius commencing in 1877. (No date)

IT is a great pity these books cannot be pushed forward much faster. The plan adopted by many German authors of sending out books in "Lieferungen" has some advantages, but generally these are more than balanced by the time allowed to elapse between each part. This slowness on the part of authors makes it somewhat unpleasant for a translator, who must of necessity be still somewhat later. In this particular instance, however, the translator has improved on the time by introducing or referring to methods not in the original, but it might have been carried further. The original does not contain anything about Victor Meyer's methods of vapour-density determination, and the translator has also refrained from noticing these methods. There may be some reason for this, but we think at least the methods might have been mentioned, as they are simpler to perform than any other, and do not fall behind any in accuracy.

The whole of Part I. and a small portion of Part II. is taken up with analysis of organic bodies; the remainder of Part II. is on the analysis of potable and spring waters. &c. If an index or table of contents had been added, it would have rendered the English edition more practical.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Protective Influence of Black Colour from Light and Heat

IN NATURE, vol. xxxiii. p. 559, a correspondent refers to the effect of blackening the skin round the eyes as a protection against the glare of strong sunlight. Probably the practice has good scientific grounds.

The shape of the orbit at once suggests the reflecting cone of a thermopile, with the eyeball centred on its axis in the position of the pile, but of course much less deeply placed. The cone is, in this case, oblique, the maximum slant side being internal, *i.e.* towards the nose, and the minimum slant side external. A plane through the outer orbital angle, and perpendicular to the axis, will be pretty nearly coincident with the tangential plane of the eyeball at the anterior end of its antero-posterior diameter, and there will be a considerable part of the nasal surface of the cone in front of that plane. This part will act as a reflecting surface, and concentrate the rays upon the eyeball. Probably variations of complexion will not much affect the reflecting power of this surface, seeing that the difference in the skin of black and of white races is mainly a difference in the amount of pigment in the rete mucosum, and not in the superficial parts of the epidermis.

It is evident that rays reflected from the ground, and from objects of no great altitude, are the rays which will have the greatest chance of striking the eye after reflection from the sides of the orbital cone. The direct rays of the sun in tropical countries will, during the hottest part of the day, be too nearly vertical to take this course. Now it would seem that it is in the case of intense light reflected from rocks, snow, &c., that the blackening has been found useful.

Whether to any appreciable extent the amount of light entering the eye is increased by the shape and projection of the orbit is a different question. For here it is not enough that the rays should be concentrated upon the eyeball. They must enter the pupil. Nevertheless, it would seem from observations made for another purpose upon the pupil-reflex, that the diameter of the aperture is increased by blackening the skin round the orbit, say by means of a piece of black cloth with an elliptical hole in it for the eye, the light of course being kept of constant intensity.

April 19 G. N. S.

On the Form of Mole-Hills Thrown up under Snow

MOLES must have an opportunity of getting to the surface here and there to dispose of the results of their excavations. When they meet with a deep-laid hard road they come out and cross it. When frost has bound the soil into an impenetrable cake they sometimes come out of the ground, and, travelling away to seek a place more suitable for their operations, are unable to find their way back or to burrow into the frozen soil in another place,

and so they get killed in considerable numbers. When there is a little snow on the ground, protecting it from the frost, the moles come to the surface as usual, and throw up mounds of earth under or through the snow. But, when deep-drifted snow has covered the ground, the mole-hills under it are found to be arranged in more or less symmetrical ridges of uniform height and breadth, as represented in the sketch. It would appear that the moles in these circumstances make galleries about the size of their own bodies on the surface of the turf in the bottom of the snow, into which they push the earth to be disposed of, finding it easier to make these small tunnels than to raise the usual mound of earth under the superincumbent snow-drift. The severe winter just over has caused the snow-drifts to lie long in the north of England, where examples of this peculiar form of mole-hill may be commonly seen on the Fells.

Cambridge THOS. MCKENNY HUGHES

Protective Imitation

I HAVE been watching for hours with great interest what I believe to be a very curious instance of protective imitation. A large old thrush has been, all that time, trying to make itself look like a serpent, and succeeding remarkably well. The object appears to be to frighten away a smaller and more active thrush—no doubt younger and with sharper ears—which seems to be getting all the worms. It appears afraid to attack its young rival, but runs towards it as if it meant to do so, and when the young one turns round and faces it, the old one crouches down so that nothing of it is seen but a crest-like back, two glaring eyes, the spotted throat, and a dark line formed by the front view of the beak and the lines at the corners of the mouth, which look very much like a serpent's mouth. If I saw the creature protruding from a bush or from the grass, I should certainly take it for a snake of some kind. The young bird looks alarmed and retreats, though just before it was ready to attack the other. No sooner has it recovered its courage and advanced to attack than the old one retreats, and resumes its serpent-like mask. There has been a little sparring in the air occasionally, just enough to show the nature of the feeling, but if allowed to do so the young one evidently would be content to feed quietly. The old thrush (I know it by a small white feather on one wing) is very much at home on this lawn, and seems to consider it as its own private domain, at all events as far as thrushes are concerned. A short time ago, when the ground was for a long time hard from frost and drought, this thrush moped about and seemed nearly starved, and at last fell upon two great clumps of yellow crocuses, and not only tore them to pieces, as if in a rage, but devoured them entirely, returning again and again to them, and gobbling up the yellow petals as a rabbit does a lettuce. At that time many birds that are usually too shy came down from the hills and strolled about the fields and lawns—snipes, plovers, &c. Two exquisite crested plovers (I think they are called) stalked about with graceful dignity for some days in a garden close by, and roosted in an old henhouse. The thrush touched no crocuses but the yellow ones, and no other bird did so. I should be glad to know if the resemblance to a serpent has been observed by any one else. J. M. H.

Sidmouth, April 19

P.S.—It may be thought that the crouching is only a preparation for a spring, but it does not suggest that to the eye, and it is not followed by a spring. If it really is a fact and not a fancy, the instincts of imitation and of fear in this case must be a very ancient inheritance indeed.

Iridescent Clouds

THIS evening at sunset there was here a fine instance of iridescent clouds. About 7 I drew the attention of my companion to some remarkable clouds; three long arms of stratus of peculiar texture, like pulled-out cotton-wool, and of striking colour, blue-black and silver, stretched nearly to where the sun had gone down behind a hill. At 5 minutes past 7 a detached portion of this cloud assumed lovely iridescent colours like bright mother-of-pearl. This gradually died away, but other portions assumed the same tints. At 7.30 the tints vanished. Wind, south to south-west.

Glencar, Kerry, April 26 J. G. GRENFELL

MADRAS MAGNETICAL OBSERVATIONS[1]

WE are indebted for the present volume to Mr. Pogson, the Government Astronomer at Madras, from whose introductory remarks we learn that he is not yet at the end of his editorial labours.

[1] "Magnetical Observations made at Madras in the Years 1851-1855, under the Superintendence of Mr. W. S. Jacob." Edited by Mr. N. R. Pogson, Government Astronomer. (Madras: Lawrence Asylum Press, 1884.)

Mr. Pogson intends as soon as possible to continue his work, and the greatest praise must be given to this distinguished astronomer for his persistent efforts to complete the records of his Observatory. But what can be thought of a system of administration under which observations are reduced about half a century after they are made? If this were the only instance of such a monstrous delay it would be bad enough, but we seem destined to have another instance, no less flagrant. The late Mr. John Allan Broun finished his work at the Trevandrum Observatory in 1864, and as yet only the first volume of his reductions has seen the light. Here the Observatory has been discontinued, and we do not know that any one has come forward to complete the labours of Mr. Broun, so that the publication of the remaining volumes seems to be adjourned indefinitely. Surely there is something in this system which requires putting right.

Mr. Pogson tells us in his introduction that the vertical force results were never entitled to any confidence, especially before March 1853, when, for the first time, the needle was placed nearly perpendicular to the magnetic meridian, agreeably to the directions given in the report of the Royal Society. Our readers are probably aware that at the present moment a Committee of the British Association is engaged in discussing magnetic observations, and they are anxious to bring together all reasonably good determinations of the solar-diurnal variations of the three magnetic elements for as many places as possible.

It may therefore be of interest, especially after the above remark by Mr. Pogson, to apply some sort of preliminary test to the Madras observations. I shall therefore compare them with the similar results obtained at Bombay, and discussed by Mr. C. Chambers in his recent elaborate and excellent volume.

In the following table we have in the first place a comparison of the solar-diurnal variations of declination at the Colaba Observatory, near Bombay, and at Madras. For the purpose of this comparison it is unnecessary to give the scale values or to exhibit all the months. We have therefore limited our comparisons to a mean of the three months, November, December, and January, and also of the three months, May, June, and July.

TABLE I.—*Comparison of the Solar-Diurnal Variations of Declination at Bombay and at Madras*

Bombay civil time (noon = 12) h. m.	Mean of Nov., Dec., Jan.	Mean of May, June, July	Madras civil time (noon = 12) h. m.	Mean of Nov., Dec., Jan.	Mean of May, June, July
0 12	+ 23	+ 23	0 41	+ 8	+ 16
1 ,,	+ 17	+ 37	1 ,,	+ 2	+ 25
2 ,,	+ 4	+ 44	2 ,,	− 3	+ 31
3 ,,	− 12	+ 46	3 ,,	− 12	+ 30
4 ,,	− 33	+ 51	4 ,,	− 23	+ 34
5 ,,	− 54	+ 85	5 ,,	− 36	+ 60
6 ,,	− 67	+ 206	6 ,,	− 48	+ 120
7 ,,	− 74	+ 272	7 ,,	− 40	+ 129
8 ,,	− 11	− 249	8 ,,	− 4	+ 91
9 ,,	+ 48	+ 128	9 ,,	+ 10	+ 21
10 ,,	+ 33	− 30	10 ,,	− 4	− 47
11 ,,	− 26	− 165	11 ,,	− 10	− 101
12 ,,	− 34	− 245	12 ,,	0	− 121
13 ,,	+ 8	− 236	13 ,,	+ 12	− 105
14 ,,	+ 28	− 184	14 ,,	+ 14	− 76
15 ,,	+ 31	− 105	15 ,,	+ 22	− 40
16 ,,	+ 26	− 34	16 ,,	+ 23	− 12
17 ,,	0	0	17 ,,	+ 12	0
18 ,,	+ 1	− 6	18 ,,	+ 16	− 13
19 ,,	+ 19	− 45	19 ,,	+ 19	− 22
20 ,,	+ 17	− 48	20 ,,	+ 13	− 19
21 ,,	+ 14	− 33	21 ,,	+ 9	− 9
22 ,,	+ 18	− 15	22 ,,	+ 10	0
23 ,,	+ 24	+ 5	23 ,,	+ 10	+ 8

Now it will, we think, be seen from Table I. that at both stations the type as well as the range of the solar-diurnal variation is very different for the two groups of months. It will likewise be seen that the peculiarities of the summer variation are very much alike at both stations, and that the peculiarities of the winter variation are also very much alike. Thus the comparison is favourable to the accuracy of the observations at both stations.

Let us now turn to the force components. In Table II. we have a comparison of the horizontal and vertical force variations at the two stations for the two months, June and December.

TABLE II.—*Comparison of the Solar-Diurnal Variations of the Horizontal and Vertical Force at Bombay and at Madras*

Bombay civil time (noon = 12) h. m.	Horizontal force June	Horizontal force Dec.	Vertical force June	Vertical force Dec.
0 18	− 105	− 98	+ 36	+ 11
1 ,,	− 96	− 88	+ 37	+ 9
2 ,,	− 97	− 84	+ 36	+ 9
3 ,,	− 94	− 77	+ 30	+ 5
4 ,,	− 92	64	+ 36	+ 7
5 ,,	− 90	− 50	+ 59	+ 5
6 ,,	− 47	− 25	+ 95	+ 3
7 ,,	+ 21	+ 28	+ 74	+ 8
8 ,,	+ 101	+ 93	+ 10	+ 28
9 ,,	+ 189	+ 161	− 80	+ 1
10 ,,	+ 287	+ 232	− 153	− 57
11 ,,	+ 302	+ 242	− 162	− 61
12 ,,	+ 278	+ 204	− 116	− 19
13 ,,	+ 214	+ 134	65	+ 6
14 ,,	+ 120	+ 66	− 17	+ 9
15 ,,	+ 21	+ 17	+ 19	+ 8
16 ,,	− 56	− 27	+ 31	+ 7
17 ,,	− 105	− 65	+ 22	− 8
18 ,,	− 132	− 7·	+ 4	+ 6
19 ,,	− 124	− 86	+ 3	+ 8
20 ,,	− 124	− 105	+ 18	+ 6
21 ,,	− 121	− 114	+ 21	+ 4
22 ,,	− 119	− 111	+ 31	+ 9
23 ,,	− 111	− 109	+ 34	+ 10
Madras civil time (noon = 12) h. m.	**Horizontal force June**	**Horizontal force Dec.**	**Vertical force June**	**Vertical force Dec.**
0 41	− 309	− 284	+ 923	+ 666
1 ,,	− 299	− 245	+ 543	+ 697
2 ,,	− 268	− 226	+ 757	+ 650
3 ,,	− 258	− 218	+ 515	+ 529
4 ,,	− 243	− 197	+ 558	+ 711
5 ,,	− 236	− 159	+ 617	+ 976
6 ,,	− 128	− 80	+ 300	+ 879
7 ,,	+ 58	+ 88	+ 259	+ 593
8 ,,	+ 365	+ 317	− 533	− 217
9 ,,	+ 658	+ 573	− 1329	− 938
10 ,,	+ 873	+ 740	− 1688	− 1247
11 ,,	+ 876	+ 730	− 1926	− 1043
12 ,,	+ 716	+ 580	− 1631	− 1246
13 ,,	+ 483	+ 354	− 1090	− 1304
14 ,,	+ 194	+ 160	− 389	− 872
15 ,,	− 66	+ 15	+ 60	337
16 ,,	− 211	− 91	+ 241	− 289
17 ,,	− 281	− 185	+ 30	+ 23
18 ,,	− 319	− 256	+ 426	+ 75
19 ,,	− 338	− 326	+ 555	+ 152
20 ,,	− 323	− 369	+ 596	+ 280
21 ,,	− 336	− 374	+ 640	+ 376
22 ,,	319	− 341	+ 779	+ 585
23 ,,	− 311	− 314	+ 991	+ 747

It will be seen from this table that at both stations and for both components the type for June is nearly the same as that for December, the chief difference being in range. Also that the type at the one station is very similar to that at the other. The most marked difference between the two stations is for the vertical force, the range of this element in December bearing a smaller proportion to its

range in June at Bombay than at Madras. To investigate this point it will be desirable to give the comparative ranges for the various elements for the various months at the two stations. This is done in the following table.

TABLE III.—*Ranges for the Various Months of the Diurnal Variations of the Three Elements at the Two Stations*

	Bombay			Madras		
Month	Declination	Hor. force	Vert. force	Declination	Hor. force	Vert. force
January ...	162	389	167	91	1318	3414
February ...	117	507	120	64	1585	3778
March ...	263	571	175	104	1785	4847
April	392	576	216	218	1851	6273
May	486	473	265	243	1522	4198
June	480	434	257	260	1214	2917
July	468	439	263	249	1218	2445
August ...	545	423	301	273	1101	3203
September...	550	407	365	282	1331	6401
October ...	258	437	213	110	1595	5163
November...	103	414	91	73	1362	3633
December...	136	356	89	86	1114	2280

From this table it will be seen that for both stations there is a smaller maximum of declination range about May or June, and a larger maximum in September, while the most decided minima are in November and February for both stations. Again, there is a maximum of horizontal force range for both stations in April, and also in October, while the minima are at Bombay in September and December, and at Madras in August and December.

Finally, at Bombay there is a smaller maximum of vertical force range in May and a larger in September, while at Madras these occur in April and September. The most pronounced minimum of vertical force is in December for both stations.

It would thus appear that there is a very striking likeness between the variations of the three elements at the two stations, and that, notwithstanding Mr. Pogson's remark about the vertical force instrument, its results do not appear to be without value in a comparison of the above nature.

BALFOUR STEWART

PLANTS AND THEIR DEFENCES

A CONSTANT struggle for existence, the consequence of the enormous increase in the numbers of the individuals of almost every species, is the fate of nearly every organism, both animal and vegetable. Some have to sustain the attacks of others which are directly antagonistic to them, and which regard them as prey; in the case of others the struggle is rather one to live in the face of adverse conditions or peculiarities of environment, so that the different organisms are not directly hostile, but each affects its neighbour injuriously by adapting itself more readily to the changing surroundings, and so diminishing the other's power of obtaining nutriment, sunlight, or whatever other condition may be the object of their competition. Thus have been developed in the different competitors different features of their constitution—many perfecting powers of active assault, others facilities for active or passive defence. The last-named is particularly the feature found in the vegetable kingdom. The want of locomotion prevents any aggressive movement of the individual, and hence success in the struggle can only be secured by more complete adaptation to environment than its competitors can show, or by protective mechanisms guarding the individual from the assaults of organisms inclined to prey upon it. These mechanisms exhibit very great variety, and their object often seems obscure till they are looked at in the light of the environment of the plant, the conditions of its life, and the enemies against which it has to contend. The specially-exposed points of attack are three: the succulent leaves and shoots or the attractive fruits are assailed by animals in search of food; the honey secreted by the flower to allure to it the particular insect adapted to bring about properly the process of fertilisation attracts also other insects whose presence is useless for such purpose, and which therefore are only robbers; while the fertilising pollen is itself the object of desire on the part of others which are equally unable to apply it to its legitimate purpose.

The protective mechanisms of plants, therefore, so far as they are directed against aggressive animals, are to be looked for mainly in the neighbourhood of the young growing parts or the reproductive organs. Not exclusively, however, but generally the older vegetative parts are defended by their own inherent qualities, such as their hardness or wiriness, which keep them from being suitable for the food of their assailants. Such young growing parts in many plants, particularly those growing in exposed regions, are plentifully supplied with thorns, spines, or prickles, rendering them in many cases extremely formidable. The thorns or prickles may be produced on almost all the vegetative organs, and may be merely epidermal structures, or much stronger in composition, containing considerable developments of woody tissue. These thorny plants are most noteworthy in desert countries, some that are met with there, notably the so-called "wait-a-bit" thorn of Africa, having spines of immense length, and being quite impenetrable by man or beast. Cases are not of infrequent occurrence where even the lion himself is a considerable sufferer by coming into collision with this plant. So great is the development of the thorny character in this region that Grisebach connects it particularly with desert exposure and scarcity of vegetation. Nor are thorny plants by any means confined to such regions—on our own heaths the gorse is a familiar plant, and one sufficiently formidable to passers-by; while other spiny Leguminosæ, as the wrest-harrow (*Ononis spinosa*), are not infrequent by the wayside. A further peculiarity may be noted in connection with these plants: often the thorns do not occur above the point which is assailable by the animal in its search for food; while, when the shoot has outlived its period of succulent condition, and its tissues have become hard and dry, the thorns do not persist, being much more numerous when the part is young.

Nor is this spiny habit confined to shrubs or trees. The cactuses, which are so remarkable a feature of the vegetation of America, are equally well protected. Their surfaces show great variety of development in this particular: some have small groups of thick rigid spines, others long flexible needles of intense sharpness, penetrating easily the skin of the assailant, and almost impossible to extract.

More formidable defences even than thorns or prickles are found in the varieties of stinging hairs borne so plentifully on the leaves of many plants. These are represented in England by the two species of stinging nettle, which are, as every one knows, capable of producing considerable discomfort to the unwary person who handles them. These are, however, not worth mentioning by the side of many of their tropical relations. The structure of the hair in all these is similar: a mass of cells forms a kind of swollen cushion below; on this is seated the long tapering hair, which ends in a somewhat recurved point or hook. The walls of the upper part of the hair are very strongly silicified, and are, consequently, easily ruptured. Lower down there is but little silica. When touched or rubbed by the hand, the pressure drives the hair downward; at the same time the brittle hook penetrates the skin and breaks off. The downward pressure forces out from the broken hair a fluid of intensely acrid nature, which, on entering the wound made by the point, sets up more or less severe inflammation. This fluid is generally conjectured to be formic acid—a

view based on the fact that this acid can be obtained from the nettle plant by suitable means.

While the English representatives of this group of plants are sufficiently formidable to careless intruders, some of their connections in other parts of the globe are distinctly dangerous. A traveller in Australia describes a specimen of *Urtica gigas* in the following terms:—"A specimen seen by Sir W. McArthur, still in full vigour, rises from its base by a series of buttresses of singularly regular outline, gradually tapering, without a branch, to a height of 120 to 140 feet. The trunk then divides into a regularly-formed, wide-spreading head, which excites admiration from its extraordinary size. But the ordinary elevation of this tree is 25 to 50 feet, with a circumference of 12 to 20 feet. The leaves, when young and in vigorous growth, attain a breadth of 12 to 15 inches, and are of a beautiful dark-green colour. As may be expected, the poisonous fluid secreted from the foliage is very powerful, particularly in the younger leaves, and their sting is exceedingly virulent, producing great suffering, not unattended with danger. It is found in the northern part of New South Wales, and is a great impediment to the traveller." An Indian species (*Urtica* or *Laportea crenulata*) is equally obnoxious. It has rather large leaves, round which numerous small stinging hairs are placed. At certain seasons it emits when bruised so irritating an aroma as to cause a copious flow of saliva and mucus from the nose and eyes for many hours, while violent fevers have been caused by the fluid poured out from its ruptured hairs. *Urtica urentissima*, a Timor species, which is known to the natives by the significant appellation of "devil's leaf," has been known to produce effects so violent as to last twelve months, and has in some cases even caused death. *Malpighia urens* bears on its leaves hairs $1\frac{1}{2}$ inch long, which are pressed flat along the surface. These act very similarly to those of Urtica.

The Loaseæ, or Chili nettles, exhibit similar defences, their power of stinging being very severe.

Other plants are protected also by hairs, which play rather a mechanical than a chemical part. Such are various species of Deutzia, particularly *D. scabra*, which bears on its leaves numerous star-shaped hairs whose walls are permeated with silica.

Besides these defences, which are chiefly mechanical, though in the case of the nettle a secretion acting chemically plays an important part in their behaviour, many plants are protected by chemical means alone. This is seen chiefly, though by no means exclusively, in the case of flowers and fruit. The plant secretes in different parts, or it may be throughout its system, a juice which may be poisonous, or acrid, or harmless in effect, but very unpleasant to its assailant. Thus very many of the Solanaceous plants have poisonous fruit, as *Atropa Belladonna*, and some species of Solanum. The whole plant is charged with juice of great pungency in many of the Ranunculaceæ, *R. sceleratus* causing sores if allowed to come into contact with a delicate mucous membrane such as that of the mouth. Parts of the Aconite (*A. Napellus*) are intensely poisonous, while the seeds of *Strychnos Nux-vomica* yield the well-known drug strychnine. Others have a latex or juice which is intensely bitter and unpleasant to the taste, as the different species of spurge (Euphorbia), the dandelion, the wild lettuce, different species of poppy, and many others. An acrid juice is to be met with in many Cruciferæ, as the mustard and the radish. The aromatic Umbelliferæ, also, are protected in this way from many of their enemies, the peculiar flavour which they possess being very unpalatable to many birds which are attracted by their fruits. Other plants pour out resinous and other sticky secretions which serve the same purpose. Some others are protected by the possession of a very fœtid odour, much resembling putrefying animal matter, though this has probably been developed to attract the carrion-loving flies which secure cross-fertilisation of the plants. Such are *Arum Dracunculus* and Stapelia, a genus of Asclepiadaceæ.

A very different kind of defence against intruders is found in a Sumatran parasite, *Hydriophytum formicarum*. This plant, instead of developing special weapons of its own, attracts to itself a colony of ants whose sting is very severe. These resent very effectually the attacks of animals inimical to the plant. It is described as parasitic on trees in the form of a large irregular tuber, fastening itself to them by fibrous roots, and throwing out several branches above. The tuber is generally inhabited by ants, and is hollowed out by them into numerous winding passages, which frequently extend a good way along the branches also, giving them the appearance of being fistular. A similar arrangement is found in *Acacia sphærocephala*, but a more elaborate one, as the plant not only serves as a habitation for the ants, but develops certain organs to attract them to it. The stem and branches are furnished with very large thorns, which are set along them in pairs. The thorns are enormously swollen at their bases, which are hollow, and in these swellings the nests of the ants are found, the magnitude of the enlargement being no doubt caused by the irritation of the insects. At the base of each pair of thorns, about midway between the two, is found a large nectar-secreting gland, which is very active. The leaves of the plant are pinnate, and on the leaflets are numerous small pear-shaped glands, consisting of delicate masses of cells containing an oily secretion. *Cecropia* is also protected in the same way; its stem is hollow, and contains the nests of the ants. As in the case of the Acacia, glandular structures are present, which attract the ants and afford them food. Schomburgk describes a plant belonging to the order Polygonaceæ (*Triplaris Schomburgkiana*), a native of Guiana, as having its trunk and branches hollow between the nodes, and serving as the habitation of venomous ants. He also mentions an orchis (*Schomburgkia tibicinis*), which, he says, has pseudo-bulbs arising from creeping root-stocks. These have a small hole at their base, and ants and other insects construct their nests therein.

Turning more especially to the reproductive organs of plants, we find them attractive to intruders, not only on account of their own palatability or succulence, but as providing two especial delicacies much sought after by the insect world—honey or nectar, and pollen. The object of the secretion of the former is to secure the due transference of the latter from the stamen of one flower to the pistil of another, and this is effected in most cases by some particular insect. The invasion of others would hence lead to loss of honey or pollen, or both, without securing the end aimed at. It is natural, therefore, to expect to find many contrivances to secure the secretion to the appropriate insect, and an almost infinite variety is found, some mechanical, others chemical, others partaking of the nature of both. The enemies most guarded against are those insects which we have seen in some other plants especially courted—ants. In assailing the plant they must usually ascend the stem from the ground, and many and various are the pitfalls placed in their way. In the teasle, the leaves, arranged in pairs along the stem, have their bases attached to it and to one another, forming deep cups, which are filled with water, thus presenting an obstacle to their ascent. The leaves of the pine-apple are arranged to bring about the same result. Some plants are surrounded in their growth by water, as many of the Polygonaceæ. In *P. amphibium*, which grows sometimes in water, and sometimes on land, and has two characteristic forms accordingly, the land form has developed round the flower-stalks a number of sticky glands, while the water form has nothing of the sort. The two forms are protected from the ants, but by different means. *Silene*, the catchfly, and *Circea*, the enchanter's nightshade, also are examples of plants furnished with sticky glands. *Lactuca*, the wild lettuce, emits

a milky juice on being assailed by them. Other plants, as some varieties of the willow, have very slippery flower-stalks, which the ants cannot pass along. The forms of the flower, too, lend themselves to protective purposes: thus *Antirrhinum* and *Linaria* have a close-shutting corolla, which they cannot enter; *Cobœa* is furnished with free hairs growing on the corolla, which block the way to the nectar, and which are insurmountable by the insects Where such means are not found, in some cases a counter-attraction is provided to draw the unwelcome visitors to parts where their attentions will be harmless: thus *Impatiens* has honey-glands on the leaves which are said to stop the ants on their way to the flower.

Other insects than ants are also to be guarded against. Many flowers are capable of fertilisation by more than one species of insect, but others are especially adapted only to one kind. In these the form of the flower, while affording facilities for the proper insect to receive its pollen upon the proper region of its body, also presents obstacles to others which would be useless. The peculiar construction of the corolla in such cases serves as a protection to both nectar and pollen. This may be carried still further, access to the honey by other than the appropriate channel being hindered by chemical means. An instance of this is seen in the Alpine varieties of the Aconite, which are adapted for fertilisation by bees. Instead of the insect inserting its proboscis into the flower from the front, so as to make it pass the stamens and pistil, one bee (*Bombus mastrucatus*) bites a hole in the back of the hood formed by the sepals, and abstracts the honey. The white variety of the flower is unprotected against the theft, but the other, blue in colour, has a nauseous, bitter taste, and so is let alone.

Besides meeting the attacks of animals in these different ways, plants have to cope with other dangers, and require for these another system of defences, which are more associated with peculiarities of environment. They are assailed continually by varying conditions of climate and temperature, and have in many cases very curious modifications of structure and habit to correspond with these. A danger that threatens most plants, except in a few regions of the world, is that of having their pollen injured by rain. To meet this many varieties of form of corolla have been developed. Many have a long narrow tubular shape, the claws of the petals cohering together, while the free limbs can curve outwards in fine weather, but arch over the tube when wet. Others have a campanulate form, with the base of the bell upwards, so that rain falling on the flower cannot get near the stamens, but is shot off as by a roof. In others the stamens are covered over by development of another part of the flower, as in the Iris; the filament of the stamen, too, may be broad, and bear the anther on its under surface, as in the Naiadaceæ. It is rather curious that flowers that produce large quantities of pollen have not such defences against this danger as those which form but little, while the most complete adaptations are found in the cases of plants that inhabit damp climates.

Many flowers are defended by habit rather than structure. In wet weather they do not open their corollas at all, and not a few, even in fine weather, keep open for a very little while, only a few hours in many cases.

Besides rain, other meteorological conditions are fraught with danger. One of the most commonly occurring is frost; and allied to this is the loss of heat by radiation during the night. The power of resistance to these conditions varies very much, but in many whose constitution makes them peculiarly susceptible to damage thereby there has been developed the so-called power of sleep. The term is no doubt a misnomer, but it has been adopted and associated with certain well-defined movements which the leaves of the plants perform at the close and at the beginning of day. The movements differ very greatly with different plants, but they bring about such a position of the leaves as will protect the upper surface from radiation. Some of them are of a very complex nature, particularly those shown by certain of the Leguminosæ, which have pinnate leaves. It is in this natural order that the property of sleep is most prevalent, certain of the Oxalidaceæ and their allies coming next to them.

A similar mechanism protects very many plants from excess of sunlight, which is injurious to the chlorophyll. In bright sunshine the leaves assume a position which has been called "diurnal sleep." In it they present their edges and not their faces to the light. In other leaves the chlorophyll corpuscles themselves move, taking up a position on the lateral walls of the cells rather than on the front ones, or so placing themselves that their profile and not their surface is exposed to the sun. In some of the Algæ, as *Mesocarpus* and *Vaucheria*, this sensitiveness is seen.

Other protective devices may be seen by studying the adaptations of plants to their conditions of life. Thus the leaves of submerged plants are preserved from being broken by the currents of water by being minutely subdivided, so that they adapt themselves easily to the motion, and do not oppose a resistance. Desert plants are protected from drought by the development of a succulent habit. Aërial parts of plants, again, are protected in many cases from becoming moistened by water by a deposition in the cuticular layers of the epidermis of varying amounts of wax or resin.

THE ORIGIN OF OUR POTATO

THE year 1886, by its tercentenary associations, brings once before us the subject of the introduction of the potato into our islands, but brings it still with most of the connected questions unsolved.

How, and when, and whence it was brought was considered by Banks in 1808, and it was by him attention was drawn to a manuscript statement in 1693 by Dr. Southwold Smith, F.R.S., that his grandfather received it from Sir Walter Ralegh, and sent it to Ireland.

It was considered by Sabine in 1822, when he concluded a paper before the Royal Horticultural Society with the remark, "The introduction of the potato *into* Virginia is still involved in obscurity."

It has been considered by De Candolle in his "Géogr. Bot. Raisonée" in 1855, and more recently in his "Origin of Cultivated Plants" in 1882. It has also been considered by others. While of the old unanswered questions some are now regarded as of mere antiquarian interest, there are others to which greater importance is attached than there ever has been before.

Among the latter a fresh interest has been given by Mr. Baker's paper before the Linnean Society in January, 1884, to the old question, was it *S. tuberosum* that was introduced from Virginia? The suggestion he, in conjunction with Earl Cathcart, has thrown out, that to strengthen our cultivated potato against disease we should cross with some other species of tuber-bearing Solanum, makes it important we should clearly know what is the species we have been for 300 years cultivating. There are many other questions surrounding the consideration, some of which border on that fundamental question, What constitutes a species?

That simple but highly practical method of approaching the question, "What is our species?" the method of introducing supposed distinct wild species, and watching their changes from year to year in cultivation, has not yet been followed sufficiently long, nor with a sufficient number of such species to effect much more than establish well-founded hopes that by it there is much we may learn. At present the twenty (?) years' cultivation of *S. maglia* is the only experiment on which we can rely. What conclusions such experiments may eventually lead s to it is impossible to predict, but this is certain, that

proceeding by such a method on fact, and untrammelled by tradition, the results will be sure. Hitherto we have relied over much upon traditions and mis-called history. It has been assumed that our species is a Virginian species, and beyond that the question, till recently, has not been pushed.

It would be a fitting observance of the third centenary of the date that may be most reasonably fixed for the introduction from Virginia, if we could celebrate it, not by speeches and after-dinner toasts to the memory of Drake or of Ralegh, but by clearly laying down our lines of inquiry, for they have been very ill-defined.

It may be one useful part of the work to reconsider the traditions and inferred history of our potato—for there is no doubt that botanists, if not perhaps actually led astray, have at least been hampered and puzzled by them.

One of the commonest traditions repeated over and over again in histories, dictionaries, works of gardening and agriculture, is that Sir Walter Ralegh brought the potato from Virginia. The great error in this is that Ralegh never was in or near Virginia.

His patent for founding an English colony in the New World was granted March 25, 1585, and he parted with it on March 7, 1589. We have records of the various expeditions sent out at his cost to endeavour to establish and maintain a colony, with the dates of sailing and returning, the names of the captains, and other details. Ralegh's life all through the period is known, and his time is so fully accounted for that he could not have gone out even *incognito*. The traditions, therefore, that he brought both the potato and tobacco from Virginia, may be for ever laid at rest. Whether some of his returning colonists, or one of the returning ships that had been sent out with supplies, brought it, is another question. There is not even tradition to that effect, far less any statement in the contemporary history of any of the expeditions.

Gerard, however, in his "Herbal," 1597, at p. 781, describing the "Potatoes of Virginia," says: "I have received rootes hereof from Virginia, otherwise called Novembeya, which grow and prosper in my garden, as in their owne native countrie." The value of Gerard's picture and letterpress will be presently discussed, but the point here to notice is that he makes the statement that he did receive "rootes" (by which, of course, he means tubers) from Virginia. One of the names he mentions for the potato is "papus." The name "papus" also occurs in the first catalogue of plants growing in his garden in 1596, so that the "rootes" he had he received not later than early in that year. The exact date is perhaps unimportant, as there is no record of any expedition to Virginia after 1590 till 1606. The land named Virginia was first visited in 1584. The introduction is therefore limited to some time between 1584 and 1590. At a period when the study of plants was confined almost wholly to apothecaries, and when sea captains thought more of fighting a Spanish or Portuguese ship than of observing the natural products of a newly-discovered land, it was not expected that the account of a voyage should refer to roots brought home. The sea-lion that roared its presage of Sir Humphry Gilbert's death is of course carefully described as a marvel, but a root is too ordinary a thing for notice. Can we by any consistent inferences account for the introduction between 1584 and 1590?

That learned mathematician, Thomas Heriot, who went out in the expedition of 1585 and returned in 1586, wrote a report on the "commodities" of the then known area of Virginia. The Island of Roanoak contained the head-quarters, and we know from Lane's report that exploring expeditions had been sent to the south for 80 miles, to the north for 130 miles, and also to the north-west for 130 miles. But that was all that was known of Virginia till the time of James I. The second part of Heriot's report is "of such commodities as Virginia is knowen to yeeld for victuall and sustenance of man's life usually fed upon by the naturall inhabitants as well also as by us during the time of our abode; and first such as are sowed and husbanded." Under the sub heading "of roots" he says:—"Openauk are a kinde of root of round forme, some of the bignesse of walnuts, some farre bigger, which are found in *moist* and *marshy* grounds growing many together one by another in ropes as though fastened with a string. Being boiled or sodden, they are very good meat." In the third edition is added, "Monardes calleth these roots beads or paternostri of St. Helena" ("Monardes," parte 2, lib. 1, cap. 4). This report is dated February, 1587, seven months after his return to England. How far it was written from memory we have no means of knowing. But this should be noticed—that Lane says that when, after much discussion, the colonists decided on returning to England, their departure was so hurried that there were "left or thrown over, cards, books, and writings." Heriot nowhere speaks of writing or making notes on the spot.

It has been generally supposed that the root here described under the name "openauk" is the potato. It should not escape notice, however, that Gerard does not in any way allude to the name "openauk," and it is nowhere said that openauk was brought to England. The only mentioned habitat, "moist and marshy grounds," seems strange, but the usual answer (in conversation at least) to the objection is, if the openauk is not the potato, what is it? and Gerard's statement that he received potatoes from Virginia is taken to strengthen the supposition. The suggestion, however, has been made that it was the Jerusalem artichoke.[1] All that can be said is, there stands Heriot's description, and there stands Gerard's statement. To link the two together may be a fair assumption, but it remains a mere assumption. The omission by Gerard of any reference to the name "openauk" is against the supposition he received roots from Heriot personally. Gerard's use of the word "papus" calls for notice, but there is one point that should be referred to before quitting the openauk.

Heriot, who is said to have been Ralegh's mathematical tutor, describes himself in his report as "servant to Sir Walter Ralegh, a member of the colony, and then employed in discovery a full twelvemonths." If he brought potatoes with him, it would be by courtesy said Sir W. Ralegh introduced them. All the expeditions were his. But there is another tradition that Sir Francis Drake brought them. Different writers give different dates for this, which are evidently wrong. He could not have brought them in 1580 from the west coast of South America, because he arrived in November, after coming round by India and the Cape, and they would have sprouted on the voyage. That was the return from his famous circumnavigation. It could not have been 1585, because he left England, after four years ashore, in that year, and did not return till July 1586. If Heriot had anything to do with the introduction of the openauk, it is almost certain Drake brought it in 1586, for the circumstances of his return then were these. His knighthood, conferred upon him after months of deliberation for his great voyage round the world, firmly established his position, and he was intrusted with the command of a fleet to the Gulf of Mexico to harass the Spaniards. His instructions were to visit Ralegh's colony at Virginia on his way home. He called there on June 8, 1586, and found the colonists much distressed that the ship from England that it had been promised should be sent with supplies in the spring had not arrived. He stayed there many days, granted their request for a ship to be left with them, but, as many unexpected troubles arose, which are described by Lane,

[1] Asa Gray and Trumbull. *Amer. Journ. Sci. and Art*, xiii., May, 1877, p. 351.

they asked to be taken home, and this was done. Although at the last their departure was so hurried that writings, &c., were not embarked, it does not follow that there had not been opportunity during previous days to embark roots among other provisions. As openauk was among the products "husbanded," Heriot may have had a supply of unplanted roots ready to send home. If this were so, then two traditions would be reconciled. It would be Drake's ships, but Ralegh's colonists, that brought the potato, assuming the openauk to be the potato. This, however, is mere assumption. For the fact that Drake brought home the people there is abundant evidence, but respecting the roots there is not a word. If we wish, however, to account at all for Gerard's receiving potatoes from Virginia, this seems the only likely way in which he could have received them. The overdue relief ship that arrived a few days subsequent to the departure of the colony, and returned after a brief search, may possibly have brought them. All the other expeditions were later in the season than even Drake's return, while of the 349 colonists who went out in 1587 nothing was ever known after they were landed, though a relief expedition made search for them. Gerard distinctly says it was the "rootes" he received, and these could not, like seeds, be available at any time of the year.

It is commonly supposed that the introduction of the potato from Virginia is a duly authenticated historic fact. What forgotten manuscript records or letters there may be it is impossible to say, but at present our sole authority that it was brought thence is Gerard, while the linking of two traditions as here suggested is only assumption.

It has been already mentioned that while Gerard does not use the word openauk, he does give the name papus. Papus is not mentioned by Heriot as a word in use in Virginia; *how then did Gerard come to use it?*

From the travels of Pedro Cieza de Leon [1532–1550] we know that papas was the general name in Peru for an edible root in his time. The root was cultivated, and it was eaten boiled, or else dried in the sun and preserved, when it was called chuña. Acosta, whose travels in the same regions were later [1570–1587], gives almost identically the same information, as also does the native-born Garcilasso. They none of them, however, give any description of papas by which it is possible to identify the plant known by that name.

The two oldest known Continental botanists that give the name papas *in conjunction with a description* of the plant, are Clusius and Bauhin. In addition to descriptions, both give figures.

In his Φυτοπίναξ (1596) Bauhin describes a plant to which he gives the name *Solanum tuberosum*, but without any figure [Lib. v. Sec. 1, No. xix.]. In his "Matthiolus" (1598) he refers to it with a figure. Here he adds, "Vulgo Pappar Hispanorum vel Indorum dicitur." Clusius, in his "Rariorum Plant. Hist." (1601), describes a plant clearly the same, with a figure, under the name *Papas peruvanorum*. He says there is no doubt this was the plant Cieza de Leon refers to. The expression, "there is no doubt," is, however, somewhat removed from certainty. In 1620, Bauhin again, in his Πρόδρομος, in describing *Solanum tuberosum*, to which he here adds "esculentum," refers to Cieza; and again, in 1623, in his Πίναξ, mentions that this is the plant from which Acosta says chuña is made. Both Bauhin and Clusius give their descriptions as from growing plants.

It might be readily surmised that with such continuous traffic as there was between Spain and the domains she had conquered in South America, the roots so highly prized by the Indians should be carried home. To strengthen this surmise there is the tradition that gives the name of the first to introduce them, a "doctor" named Hieronymus Cardan. What is the history of the introduction into Spain is beside the present question. It is not improbable that with the sustained and frequent intercommunication between Spain and America it was repeatedly introduced. The case is by no means parallel to the question of the introduction into England from Virginia, in Gerard's time, when out of the six expeditions sent out only one made any explorations inland. The opportunities of introduction from Virginia were few. From South America to Spain they were numerous. It seems sufficiently established, both by Bauhin and Clusius, that a plant called papas was introduced and grown in botanical gardens, if not as a food; and that it came to be known as the papas of the Peruvians, of the Indians, and of the Spaniards, for Peruvanorum, Indorum, and Hispanorum seem indiscriminately used. That Clusius suggested its identity with the Arachnida of Theophrastus and other Greek writers is now of little interest. Bauhin was the first to recognise the plant as a Solanum, and his *tuberosum* occurs as No. XIX. in his list of Solanums, in his Φυτοπίναξ.

Though Cieza, Acosta, and Garcilasso drew what appears to have been a consistent distinction between papas (potato) and battatas (sweet potato), that distinction was not always maintained by later European writers. In a way it seems hopeless to endeavour to trace, the Portuguese and Spaniards now use different words for the potato: the former call it batata, and the latter papa. The confusion is more bewildering when the two names were used as synonyms. In botanical nomenclature we have lost papas, but retained battatas. The identity or not of *Battatas edulis* with the battata of the three Spanish travellers is wide of the present consideration. So also would be the question why the Quichan word *ascu* was not used by them. This, however, appears a safe rule—that when papas is mentioned by sixteenth-century writers it may be read as = Solanum (but not necessarily *tuberosum*); when battatas is mentioned it is requisite to see whether it is wrongly used as a synonym or intentionally used for a distinct plant. To the present day chuña is made in Peru from "papas," but apparently not from "battata."

Assuming the rule is a safe one that papas cannot be taken to mean battatas, but battatas may and often does mean papas, then such chronological data as the following are of interest as some indication of the spread of the plant among botanists in Europe. There may be others, but these are all the writer has been able to collect.

Dr. Scholtz had papas growing in his garden at Breslau (Vratislavia), 1587; Clusius received two tubers at Vienna from Hannonia, 1588; Bauhin, in his Πρόδρομος, mentions "iconem suis coloribus delineatam," 1590; Dr. Scholtz's "*Papas hispanorum*" is mentioned in a "Carmen" (pub. at Vratislavia), 1592; Bauhin refers to a "*Pappar hispanorum*" growing in his garden, of which he gives a description, 1596.

It was in this year (1596) that Gerard published the catalogue of plants growing in his garden in Holborn. There occur in it the two names *Papus orbiculatus* and *Papus hispanorum*. In this 1596 catalogue these names, as all the rest, occur without any English equivalent or any description or note. The catalogue is simply a list of names. The word batata does not occur, but Sisarum does. Another catalogue, commonly called a second edition, was published in 1599. The "Herbal" had been published in the meantime (1597). In this 1599 catalogue English names are added to the Latin. These occur: *Papus orbiculatus*, bastard potatoes; *Papus hispanorum*, Spanish potatoes. Batata does not occur. Sisarum does, but without any adjective (we cannot call these second names "specific," while the first were in no sense of the word "generic"), and the English name with this is skyrrits.

Although it would be a natural supposition that with the aid of the figures and descriptions in the "Herbal" it would be easy to identify the plants named in the cata-

logues, it is, on the contrary, a most perplexing puzzle. There are names introduced into the "Herbal" which do not occur in the catalogues, and names in the catalogues which do not occur in the "Herbal." That the "Herbal" of 1597 should not exactly agree with the catalogue of 1596, hardly excites surprise, but that the catalogue of 1599 should so differ from the "Herbal" is more than surprising, it is perplexing. If the explanation given by Mr. Daydon Jackson in his annotations to the catalogues is correct, then the *Papus hispanorum* of Gerard's garden was not the *Papus hispanorum* of Clusius and Bauhin; but this requires very close attention. It involves not only the question whether the *Papus hispanorum* of Dr. Scholtz was Solanum or Batatas, but also whether Bauhin is to be trusted as a cautious incorporator of statements. However highly Bauhin is to be esteemed as a botanist, he may have had a Pliny-like weakness for accepting anything he was told.

Mr. Daydon Jackson's explanation is this:—

"Herbal" of 1597		Catalogue of 1599
Battata virginiana and *pappus*—Potatoes of Virginia (p. 781) ...	=	*Papus orbiculatus*—Bastard potatoes.
Sisarum peruvianorum, sive *Batata hispanorum*—Potatus or Potatoes (p. 780)	=	*Papus hispanorum*—Spanish potatoes.
Sisarum (p. 871)	=	*Sisarum*—Skyrrits.

Supposing this to be the correct explanation, what are we to think of Gerard allowing his second catalogue to appear so like his first and so unlike his "Herbal"? One point is clear—he uses Papus, Batata, and Sisarum with such want of discrimination that no importance can be attached to his names. But it is strange he should, in both his catalogues, use Papus twice and Batata not at all, while in his "Herbal" he has both *Batata virginiana* and *Batata hispanorum*. According to accounts that have been handed down to us, the "Herbal" was based on Dr. Priest's translation of the Pemptades of Dodonæus, and the plates, with the exception of sixteen, were those that had been used to illustrate works by Jacobus Theodorus ("Tabernæmontanus") and L'Obel. It is said that Gerard so little understood his work that he put cuts in the wrong places, and made so many mistakes that Norton, the publisher and proprietor of the work, engaged L'Obel, who was then living in England, to correct the errors. Gerard resented this, and a quarrel with L'Obel followed. To what extent L'Obel's corrections went we have no record. He would at any rate, we may assume, prevent wrong names and cuts being printed with the letterpress. In the particular case of the three names under consideration, he was already well acquainted with the Sisarum or Batata (p. 780), as he had described it in his "Stirpium adversaria nova," written in conjunction with Pena, and published in London in 1570. He there gives the name Battades, Ignames—Anglicé, Potades. The cut in illustration used in the "Herbal" is that on p. 482 of Tabernæmontanus, where the name used is Sisarum. So that we can account for the names used in the "Herbal" thus:—Sisarum because it occurs in Tabernæmontanus; Peruvanorum is perhaps not to be accounted for. Batata because L'Obel had used it, and Hispanorum because it was first made known to Europe by the Spaniards, who brought it (most probably) originally from the West Indian Islands. Potatus, or potatoes, because that was the Anglicised form of Batata. It is possible that Gerard may have wished to introduce the word Papus, and that L'Obel cut it out.

With regard to the "potatoes of Virginia," Gerard would perhaps have his own way. He thought so much of his having grown some received from Virginia, that in his portrait he has a branch of them in his hand. With regard to the cut used in illustration, we know at present nothing. It is not taken from any other source, and it does not occur anywhere but in this 1597 edition. In the 1633 edition by Johnson the cut from Clusius is used, while Parkinson, in 1640, uses the cut copied from Bauhin. It is one of the sixteen new cuts, but where it was made we do not know, still less do we know whether it was made from a plant growing in his garden.

This last consideration, where the plant grew which is here figured, is closely connected with the question, How did he come by the name papus? In the text Gerard says, under "The Place":—"It groweth naturally in America, where it was first discovered, as reporteth C. Clusius, since which time I have received rootes hereof from Virginia." And then, under "The Names," he says:—"The Indians do call this root papus (meaning the rootes), by which name also the common potatoes are called in those Indian countries."

Although there is no known publication of Clusius so early as this from which Gerard could be quoting, yet, as he had been thrice in England, there is the probability that Gerard and he were acquainted. It is easy to see then that he might easily have had, indeed most likely would have, the South American name papus direct from Clusius.

But did he have anything else from him—a figure, a full description, a dried specimen, or even a tuber? Clusius had two as early as 1588, eight years before Gerard's first catalogue.

We have seen—

(*a*) That Cieza, Acosta, and Garcilasso speak of papas as a common name in the north-west portions of South America.

(β) That Clusius and Bauhin speak of the "papas of the Spaniards" growing in Europe (which Bauhin recognised to be a Solanum) as the same plant the three mention.

(γ) That it was known in several botanic gardens in Europe before the time of Gerard's first catalogue.

(δ) That Gerard in some way received information from or through Clusius that the plant was first discovered in America. America here evidently means South America.

With Clusius's information we can hardly doubt Gerard would also get the name papus. There is no trace of papus being a name used in North America. Fernandez de Soto, who travelled in Florida [Evora, 1557], mentions Batata, but not papas Benzoni, 1572.

It has been a puzzle to some botanists that papas should have such a wide geographical distribution as from Virginia to South America. The puzzle has partly arisen on the assumption that papus was a Virginian name. As there is not a fragment of evidence it ever was, and as we have seen a way in which Gerard might have had it, that part of the puzzle may perhaps be regarded as entirely withdrawn. There are a sufficient number left in connection with the potato to tax ingenuity.

Can we as easily dispose of the cut in the "Herbal"? Are we on the strength of that cut to continue to believe that *S. tuberosum* was wild within the area known as Virginia? For, though we get rid of the name papas we do not get rid of the wide distribution of *tuberosum* if the plant itself grew wild in Peru and in Virginia? Possibly experts in wood-cutting or collectors of old cuts may be able to say whether the cut is English or Dutch. Sequier says the cuts are brass ["Bibl. Bot.," 1740, pp. 72, 73]. Haller says: "In 'Bib. Bodl.' icones dicuntur æneæ esse: sed ligneæ sunt undique" [1771, tome i. p. 389]. Such a point as this could probably be cleared up definitely.

It seems anomalous that we should base our belief that *S. tuberosum* is a native of Virginia, on a single cut about which we know nothing more than this: that it appears in conjunction with the name potatoes of Virginia; that it was placed there by the direction of a man against whom the charge of deliberate misstatement in his so-called scientific work has never been cleared up; that for some reason it does not appear in the second edition

of the work. If it is charitably supposed that in this case Gerard did not intentionally mislead, still, if his reputation for being a muddler of other people's work is as well founded as it appears to be, he may have made some blunder. It is by no means a far-fetched assumption that his figure was from a Continental source, but that he thought it near enough to represent his Virginian "rootes." Apart from all other considerations it is difficult in at least one particular to reconcile the figure and the text. He speaks of "the temperature and vertues" of the potatoes, and says they are the same as of the common potatoes (*i.e.* his Sisarum). Unless this is a pure invention, many must have been eaten for this conclusion to have been arrived at. The size of the tubers is not greater than of fair-sized peas, and it would take the produce of half a hundred plants to furnish a single dish.

It is perhaps worth consideration whether an explanation of the catalogues different from that given by Mr. Daydon Jackson is possible. Is there any insuperable objection to their being read thus?—*Papus hispanorum* (the *P. h.* of Clusius, &c.), received from the Continent. *Papus orbiculatus* (for orbiculatus is a name of his own) received from Virginia. Sisarum—the "Skyrrits of Peru" (p. 780 "Herbal"), and that the common skyrrits were not mentioned in the catalogue. When he mentions papus in his "Herbal" he does not add either hispanorum or orbiculatus, and it might be he included both under papus there.

The important point however is whether that cut truly represents what he received from Virginia.

In close connection with this it cannot be overlooked that Bauhin gives openauk as a synonym. He also says, "Ex insula Verginea primum allata in Angliam, inde in Galliam aliasque regiones.' He had probably seen De Bry's edition of Heriot, and so obtained the name openauk. But his authority for the remainder of the sentence is not clear. Moreover it does not harmonise with his reference to Peru.

The question of the introduction of the potato is a very complex one, involving many other considerations besides those here referred to. The foregoing notes may, however, clear up the traditions about Ralegh and Drake, remove the difficulty about Gerard's use of the word papus, and perhaps lead to something more certain being known about that cut of Gerard's on which so much hangs.

The origin and change in the use of the *word* potato are subjects which, for their satisfactory elucidation, involve considerations that fall within the provinces of the philologist, the traveller, the bibliographer, the historian, the botanist, and, using the word in its wide sense, the geographer.

Potato is but the English way of pronouncing Batata.

But what is the word Batata? To what language does it belong? The first European knowledge of it appears to be traceable to Cuba, San Domingo, or some of the neighbouring isles at the time they were discovered by Columbus, 1492, &c. But then the sixteenth century writers on Peru also use it as if it were a common word there, and, if it were, it is at least interesting, if not strange, to find a word thus widely spread over and across districts where, it has been said, languages so vary with tribes that one cannot even understand another, though neighbouring, tribe. But first we have to consider is there any *contemporary* evidence that the West Indian natives did make use of a word which, when written by the Spaniards, appeared as *batata?* It would involve a special search among such materials as Navarette had at his disposal to decide that. Compilations are not to be trusted, and English versions are of no avail. What the actual word was, written by Columbus or his companions, is what is wanted. Then, if it were a true West Indian word, and introduced and known with some plant in Spain and Portugal in the early part of the sixteenth century, what is the probability that, at the middle of it, writers on Peru used it as a name that would be understood at home, even though not used by the South American natives. With regard to papas, it is distinctly stated by Acosta it was a native name in South America, but the writer does not know of any passage in which batata is said to be. It has been pointed out above how the mistake arose that papas has been considered a Virginian name, and it is possible batata may prove to be not a South American name at all. There is a Quichau word, Ascu, equivalent, apparently, to Papas, to which only Mr. Clements Markham among English writers seems to have drawn attention. At present, in English translations of travels in Peru, papas and batata appear often confounded.

Then in regard to our own use of the word batata, did we have it with roots through the Spaniards, or direct from the West Indies? The earliest use of the word does not yet seem to have been fully searched for. It may, however, be found earlier than in the list of literary quotations usually given. For example, it occurs in the account of Sir J. Hawkins's voyage, 1565: "Hennes, potatoes, and pines." The earliest description the writer has been able to trace of what the potato was is in the botanical work of 1570, published in London, Lobel's "Stirpium adversaria nova." A figure is given of the root of the Batata, and at the heading is "Anglice Potades."

But we might have had the word half a century before that through Spain, and the fact that Lobel introduces such a curiously-spelled form as the usual English one would imply it had been for some time in use among the common people. The mention of potatoes in the Hawkins voyage without any reference to what they were like would also imply that they were then as familiarly known as pines or hens.

The change of sounds from Batatas to Potades is curious. Why should the flat labial be changed to the sharp, and the sharp linguo dental be changed to the flat, in the same word? Again—the question is not so undignified as may at first appear—when was the form "taters" introduced? It has no doubt been a gradual change, but as a fact country people of the Victorian era no more think of using the form potatoes than those of the Elizabethan era did of using batata. In 1596 the form potaton is met with. In 1627 and 1676 potadoes, and in 1655 pottato. Batata itself, by the Spaniards, seems to have been spelled indifferently batata or battata.

Then there is another curious point. How has it come to pass that for the same plant the Spaniards of to-day retain papas, while the Portuguese use batata, for the plant we *now* call the potato.

In speaking of questions in connection with our having changed the use of the word potato from one plant to another it is an advantage for preventing confusion to refer to the two plants by their present botanical names, the *Batatas edulis*, which belongs to the convolvulus "order," and the *Solanum tuberosum* (perhaps including the supposed different species, *Maglia*), our common potato, which belongs to the nightshade "order." Of the two it was *Batatas edulis*, called then, long before Linnæus's binomial system, simply Battata, that seems to have been first known in Europe.

The first European knowledge of the plant *Solanum tuberosum* (or *Maglia*) was under the name papas, by which it was known till Caspar Bauhin recognised that it was a Solanum in 1596. The date 1596, if not exactly that of his knowledge, is the date of his first publishing it in his "Φυτοπινοξ."

Then as to dates of introduction.

As already said, the first European knowledge of Battata was in 1494 or 1495, that is, assuming that it was among the valuable products of the West Indies Columbus sent home to his patron sovereigns to demonstrate the value of his discoveries. It is mentioned he sent home vegetable products as well as gold. He sent spices,

dye-woods, fruits, and herbs, or intended to. In the history "Primer viage de Colon" (Navarette, cap. 1) is the passage,[1] "And besides there are trees of a thousand species, each having its particular fruit and all of marvellous flavour, so that I am in the greatest trouble in the world not to know them, for I am very certain they are each of great value. I shall bring some home *as specimens*, and also *some of the herbs*." Taking Washington Irving's inspection of Navarette's materials as reliable, Columbus knew the potato—the battata.

Then it is also probable, for here we have to deal with probability only, that the Solanum [under the name papas] was known in Spain soon after the conquest by Pizzaro [1527], when Cieza de Leon wrote [1532-50].

Both of these are at present but assumptions in respect to dates. The exact dates may perhaps be known in Spain. Possibly some people in England may know what is known, but the writer has been unable to trace anything more through the published second-hand statements.

We in England somehow knew the battata, pronounced and spelled potade or potate or potato, before the time of Hawkins's voyage, and before Shakespeare wrote his "Merry Wives of Windsor," where he uses the word. That Shakespeare's potato was the batata is clear from Gerard's reference to the confectioners using the battata as a basis for their sugar work (p. 781 of his "Herbal").

It was Gerard who called the papus (papus, as he chose to spell it, instead of papas) the Virginian potato, or bastard potato.

There in his work we have the word "batata," or patata, or potato, transferred to the papas, to Bauhin's *Solanum tuberosum esculentum*. Though Gerard does not use the word Solanum, his figure and description are sufficient identification. Somehow, though it does not seem possible to trace how, the word "potato" or "taters" has, as an English word, stuck to the *Solanum*. The "battata" has now dropped out of cultivation as an English root, and this no doubt has been the main cause of the transference of the word "battata" from the original battata to the "bastard" potato of Gerard—the Solanum.

The establishment of batata as a botanical name, its recognised description, and its admission into generic nomenclature have a curious history, but that is somewhat wide of the points more immediately under consideration.

The whole question is by no means yet worked out, but the above suggestions may draw attention to the subject.

W. S. M.

[1] Quoted second-hand through W. Irving's "Life of Columbus."

THE COLONIAL AND INDIAN EXHIBITION

THIS Exhibition was opened on Tuesday by Her Majesty in state. Science in one form or another will be prominent in nearly all of the sections. The Exhibition as a whole will be a geographical education in its widest sense. Not many can follow the example of Mr. Froude and Baron Hübner, and spend the best part of a year in visiting our scattered Empire. At South Kensington, in the course of a few days, however, we may learn even more of the products and people and geographical aspects of our colonies than we might do by an expensive voyage. Of course the main purpose of the Exhibition is to draw attention to the economical and commercial aspects of the colonies and India; but in doing so, necessarily the introduction of a considerable amount of science is involved. In nearly all the sections, for example, we find excellent large maps of the various colonies on the walls, besides the gigantic map of the world in hemispheres beside the gateway of Old London. Again, several of the colonies have sent specimens of their natives, and from India especially there is a considerable number of individuals of all ages representing the various races which form the heterogeneous population of that vast territory. So, from South Africa, we find Kaffirs, Hottentots, Zulus, and Bechuanas; Singhalese from Ceylon, and Malays from the Straits Settlements. In several of the sections, also, notably in India, do we find life-size models of natives; some of the finest of them are in the British Guiana Court, prepared by Mr. Im Thurn. Several of the colonies, again, have had large reliefs either of the whole or part of their territory prepared. Among the exhibits of the Indian Survey is a relief-map of the Peninsula from the Tibetan table-land to Cape Comorin, on the scale of thirty-four miles to an inch. One of the finest of these models is that of New Zealand by Dr. Julius von Haast, under whose care this Court is markedly scientific. He has brought over with him the skeletons of three large moas; numerous specimens of flora, fauna, and geology, and the exquisitely beautiful skeleton of a ribbon-fish prepared after the method of Prof. Parker of Dunedin. Maori ethnology is also amply illustrated, though we believe no actual live specimen has been imported. One of the finest conservatories of native plants in the Exhibition will be that attached to the New Zealand Court. But such conservatories will be a marked characteristic of this Exhibition, and will be found attached to the Courts of the Cape of Good Hope, Queensland, Natal, and other colonies. India, of course, has much to show of interest to science, besides its numerous groups of life-size models of natives taken from actual casts. Under the care of Dr. Watt the botany is very fully illustrated. The Geological Survey has sent a fine exhibit; while the Topographical Survey will have a Court to itself. In all the Australian colonies geology is a prominent feature, at least in its economic aspects, and so we may say of botany, at least so far as timber-trees are concerned. In the Australian and several other colonies, moreover, large collections of natural history have been arranged in cases, while of course the numerous game-trophies will interest the naturalist. The trophy of trophies, however, will be the great jungle scene prepared by Mr. Rowland Ward, into which it has been attempted to compress the whole of the fauna of India. It is a triumph of arrangement; and we may refer to it in detail in a future article. An almost equally striking scene is the landscape in the South Australian Court, representing an actual piece of country near Lake Alexandrina. Of course, as in the jungle scene, we have *multum-in-parvo*,—features which in reality are spread over a wide area compressed into a few square yards. But everything is on the scale of nature, and nothing introduced that is not actually met with. We have natives at various occupations, including a woman and child under a rude shelter of branches; kangaroos, wallabies, eagles, and other animals deftly posed; characteristic vegetation and rocks, with mountains away in the background. The model of Hong Kong and the neighbouring coast may also be mentioned. The West Indian Court contains much of interest. The woods of Honduras are conspicuous; many curious land and water products from Trinidad; and a fine collection of Columbian pictures and relics, and several fine paintings and photographs of West Indian scenery. Indeed, in all the sections, pictures, and especially photographs, are among the most conspicuous exhibits, and have much geographical value.

Of course this Exhibition is one of many-sided interest, and we have mentioned here only a few of the points that will attract those interested in science. Its educational value is evident, and we hope that teachers will take advantage of so exceptional an opportunity of giving their pupils a practical lesson in physical geography and its economical and "political" developments. Most of the colonies will publish special hand-books, and in several of them we are glad to know that science will hold a prominent place.

NOTES

We refer elsewhere to the opening of the Colonial and Indian Exhibition on Tuesday. It argues ill for the spirit in which this Show is to be conducted that the representatives of British science, on which the progress of England beyond the seas has so largely depended in the past and must depend in the future, were so conspicuous by their absence at the opening ceremony. Not even the President of the Royal Society was invited to be present, though tickets were liberally distributed to a large number whose prior claims we do not care to discuss.

Science was well represented by the President of the Royal Society at the Royal Academy dinner on Saturday. Prof. Stokes showed how in several ways science is capable of rendering service to art. The rules of perspective, he pointed out, involved clear geometrical conceptions; while a knowledge of chemistry and physics would keep the artist often from violating nature. Prof. Stokes illustrated the point by referring to the inverted rainbow picture, adduced as an example for a similar purpose in these pages some years ago. At the same time he admitted with justice that art was not without its uses to science. Especially useful was it, he pointed out, as a refreshing and invigorating change for the mind of the scientific student, apt to get clogged and dulled by too eager direction to one particular subject.

The Fifty-sixth Annual Meeting of the British Association will commence at Birmingham on Wednesday, September 1, 1886. The President-elect is Sir William Dawson, C.M.G., F.R.S., Principal of McGill College, Montreal, Canada. Vice-Presidents: The Right Hon. the Earl of Bradford, Lord-Lieutenant of Shropshire, the Right Hon. Lord Leigh, Lord-Lieutenant of Warwickshire, the Right Hon. Lord Norton, K.C.M.G., the Right Hon. Lord Wrottesley, Lord-Lieutenant of Staffordshire, the Right Rev. the Lord Bishop of Worcester, Thomas Martineau, Mayor of Birmingham, Prof. G. G. Stokes, Pres.R.S. (nominated by the Council), Prof. W. A. Tilden, F.R.S., Rev. A. R. Vardy, Rev. H. W. Watson, F.R.S. General Treasurer: Prof. A. W. Williamson, F.R.S., V.P.C.S., University College, London, W.C. General Secretaries: Capt. Douglas Galton, C.B., F.R.S., A. G. Vernon Harcourt, F.R.S. Secretary: Arthur T. Atchison. Local Secretaries for the Meeting at Birmingham: J. Barham Karslake, Rev. H. W. Crosskey, Charles J. Hart, Council House, Birmingham. Local Treasurer for the Meeting at Birmingham: J. D. Goodman. The Sections are the following:—A. Mathematical and Physical Science—President: Prof. G. H. Darwin, F.R.S.; Vice-Presidents: Donald MacAlister, M.D.; Rev. H. W. Watson, F.R.S.; Secretaries: R. E. Baynes (Recorder), R. T. Glazebrook, F.R.S., Prof. J. H. Poynting, W. N. Shaw. B. Chemical Science—President: William Crookes, F.R.S.; Vice-Presidents: Prof. Carnelly, W. H. Perkin, F.R.S.; Secretaries: Prof. P. Phillips Bedson (Recorder), H. B. Dixon, F.C.S., H. Forster Morley, D.Sc., F.C.S., W. W. J. Nicol, Ph.D., C. J. Woodward, B.Sc. C. Geology—President: Prof. T. G. Bonney, F.R.S.; Vice-Presidents: Prof. C. Lapworth, F.G.S., H. Woodward, LL.D., F.R.S., F.G.S.; Secretaries: W. Jerome Harrison, F.G.S., J. J. H. Teall, F.G.S., W. Topley, F.G.S. (Recorder), W. W. Watts, F.G.S. D. Biology—President: William Carruthers, F.R.S., F.L.S.; Vice-Presidents: Prof. E. A. Schäfer, F.R.S., M.R.C.S., P. L. Sclater, F.R.S., F.L.S., Sec.Z.S.; Secretaries: Prof. T. W. Bridge, Walter Heape (Recorder), Prof. W. Hillhouse, W. L. Sclater, F.Z.S., H. Marshall Ward. E. Geography—President: Major-General Sir F. J. Goldsmid, K.C.S.I., C.B., F.R.G.S.; Vice-Presidents: Major-General Sir Lewis Pelly, K.C.B., K.C.S.I., M.P., F.R.G.S., Capt. W. J. L. Wharton, R.N., F.R.G.S.; Secretaries: F. T. S. Houghton, J. S. Keltie, F.R.G.S., J. S. O'Halloran, F.R.G.S., E. G. Ravenstein, F.R.G.S. (Recorder). F. Economic Science and Statistics—President: John Biddulph Martin, F.S.S.; Vice-Presidents: G. W. Hastings, M.P., F.S.S., Sir R. Temple, Bart., G.C.S.I., M.P., F.R.G.S., F.S.S.; Secretaries: E. F. Barham, Rev. W. Cunningham (Recorder), Prof. Foxwell, F.S.S., J. F. Moss, F.R.G.S. G. Mechanical Science—President: Sir James N. Douglass, M.Inst.C.E.; Vice-Presidents: W. Anderson, M.Inst.C.E.; W. P. Marshall, M.Inst.C.E.; Secretaries: Conrad W. Cooke, J. Kenward, Assoc.Inst.C.E., E. Rigg (Recorder). H. Anthropology—President: Sir George Campbell, K.C.S.I., M.P.; Vice-Presidents: Prof. W. Boyd Dawkins, F.R.S., Lieut.-Col. H. H. Godwin-Austen, F.R.S.; Secretaries: G. W. Bloxam, F.L.S. (Recorder); J. G. Garson, M.D., M.A.I., Walter Hurst, B.Sc., R. Saundby, M.D. The first General Meeting will be held on Wednesday, September 1, at 8 p.m. precisely, when the Right Hon. Sir Lyon Playfair, K.C.B., M.P., F.R.SS.L. and E., will resign the chair, and Principal Sir William Dawson, C.M.G., F.R.S., President-elect, will assume the Presidency, and deliver an address. On Thursday evening, September 2, at 8 p.m., a *soirée*; on Friday evening, September 3, at 8.30 p.m., a discourse on "The Sense of Hearing," by Prof. William Rutherford, F.R.S.; on Monday evening, September 6, at 8.30 p.m., a discourse on "Soap Bubbles," by A. W. Rücker, F.R.S.; on Tuesday evening, September 7, at 8 p.m., a *soirée*; on Wednesday, September 8, the concluding General Meeting will be held at 2.30 p.m.

The first general meeting of the Congress of French scientific societies took place in the large hall of the Sorbonne on April 27 at noon. M. Bertrand, Director of the Archæological Museum of St. Germain was in the chair. For the first time a special section has been created for geography, of which M. Bouquet de la Grye is chairman. The section of sciences was presided over by M. Faye, and divided into several sub-sections. M. Lhoste presented a pointed cask, with the assistance of which he hopes to keep a balloon floating in the air for several days over the sea. M. Certes, President of the Zoological Society of France, explained the use of colouring matters for the histological and physiological exhibition of living animalcules. The meetings were concluded on Saturday, May 1, by an address by M. Goblet, the Minister of Public Instruction, in which he advocated the establishment of a secondary education from which Greek and Latin should be excluded, their place being filled by modern languages. A number of decorations were awarded to members of learned societies and academies. M. Berthelot was created "Grand Officier" of the Légion d'Honneur.

The Department of Modern Ethnography in the British Museum being now arranged, the work of arranging the prehistoric section is being taken in hand by Mr. Franks. The three rooms immediately at the head of the western staircase, near the entrance, are devoted to this purpose. The collection will contain the Christy and Museum collections, which will be incorporated with each other, and also the Greenwell collection from British Barrows. The central room of the three will contain palæolithic objects from England and the rest of the world. The finds in the caves of the Dordogne will form an important and interesting part of these. These caves were excavated by the late Mr. Christy at his own expense, and the results added to his collection. The pictures were, at his wish, sent to France. The room on the left of the entrance contains Neolithic objects, arranged under the Stone and Bronze Ages, the objects from the various countries being arranged within the periods. Here Canon Greenwell's remarkable collection from the British Barrows (which will be maintained intact) will find a place. The special value of this collection is that the place and manner of finding of each individual object is known and recorded, and every cir-

cumstance connected with the discovery of each is known. The Barrows, from which the collection was made, are found mostly in Yorkshire, although other places are also represented. In the same room will also be placed the implements used in working flint quarries in prehistoric times, and other objects found there; there will also be some curious implements from countries where the Stone Age still exists, as it does, in a certain measure, in Madeira, Syria, and Iceland. The room on the right will be given up to iron objects, and those of an age which may be called semi-prehistoric, such as the Roman times in Britain.

A SUPERFICIAL examination of the ethnographical galleries in the British Museum shows that the American section is overcrowded. On the left are the American antiquities, which are of the greatest interest, but which do not seem to belong properly to ethnography at all, while the objects on the left, belonging to modern America, and which are certainly ethnographical, are crowded into a space which is quite insufficient. As much has been done as possible to arrange the objects, and there is no confusion, but it is quite impossible to examine the cases properly when they contain so much. Ancient Mexico, Peru, and New Granada crowd modern South, North-West, and Arctic America into a very small space. It is obvious that an attempt should be made to remove the American antiquities to some more suitable place, and to give up the whole of the gallery to American ethnography proper.

AFRICA does not seem so well represented in the Collection as it might be for a country which has sent its missionaries and travellers into every corner of the land. Two or three small South Sea Islands occupy about as much space as the continent of Africa. The only district well represented is that of the Upper Nile, the collection of Lupton Bey being specially noteworthy, as giving one a fair idea of the manufacturing industries of the people of these parts. South Africa is moderately well represented, and in a less degree northern West Africa. East Africa, Central Africa, and southern West Africa are all relegated to a small case and very poorly represented. A spear from one tribe lies beside a pipe or a dagger from another tribe a thousand miles distant. And yet in this enormous region there are tribes singularly expert as blacksmiths, potters, &c. No African tribe produces more beautiful spears than the Wa-Vira, more horridly barbed than those of Nyassa, or more remarkable than those of Maryema or of Masai-land; and yet good collections of all these are undoubtedly in the country. Owing to this meagre display the collection is not of much value for purposes of comparison or to illustrate the relative advancement of the various tribes in arts and manufactures, and yet in this respect there is as much difference between the most degraded of the tribes and the most civilised as there is between the latter and ourselves. The arrangement also leaves much to be desired. Articles manufactured by tribes totally distinct in race, degree of civilisation, and religion are thrown indiscriminately together. Take, for instance, northern West Africa. There one finds the fetishes, idols, and rudely-worked articles of the degraded and barbarous tribes of the Lower Niger figuring amongst the artistic and advanced productions of the Mohammedan and polished tribes of the Central Sudan, and nothing to indicate that they are not the work of one people. In the East African section, again, you find Somali weapons beside those of the Bantu tribes further south, such as the Wa-gogo. Some objects do not appear to be correctly named. Thus the backbone of a shield divested of the hide which it was intended to support now figures as a bow, a string having been stretched from point to point. The map to illustrate Africa is scarcely worthy of the British Museum. The Congo Basin is strikingly shown by an utter blank.

FROM the Royal Gardens, Kew, we have received a cheap, carefully arranged, and highly useful guide to Museum No. III. at that establishment, which is devoted chiefly to specimens of timber and other large articles unsuited for exhibition in the glazed cases of the other museum. Another extremely useful publication is a Route Map of the Royal Botanic Garden and Arboretum on a scale quite large enough to enable any visitor to find his way. The various sections of the gardens are clearly laid down, and on the back is an index to the various entrances, museums, houses, the arboretum, &c., corresponding to the sections in the map indicated by figures and letters.

A MICROSCOPICAL SOCIETY has been started in Glasgow with Dr. Dallinger as first President; over fifty members have been enrolled.

THE volcano of Smeru in Java is stated to be in eruption.

WE have received the first number of *The Indian Engineer*, published by Messrs. Newman and Co., Limited, Calcutta. This is a new publication, the object being to provide a representative organ for all branches of the Indian engineering profession, and to make it a creditable representative of the great engineering and scientific services of the country. The leading article appropriately gives a history of Indian engineering journalism. We are told that the first publication of the kind was made by the Corps of the Madras Engineers in the form of a series of papers, to provide a record of the experience of their members for future reference. Messrs. Newman and Co., twenty-eight years ago, followed this first attempt by publishing a paper called *The Engineers' Journal and Railway and Public Works Chronicle*. Since then several different papers have been issued with varying success. *The Indian Engineer* is nicely got up and well printed, and, to judge by the first number, will prove to be an interesting journal, containing as it does many very good articles on general Indian engineering, civil and mechanical. We trust it will receive general support, and in time become an acknowledged organ of the profession in India.

WE are pleased to see from the current number of the *Agricultural Students' Gazette* that the authorities of the Royal Agricultural College, Cirencester, have provided greater facilities for teaching the increasingly important branch of agriculture, dairy farming. A new working dairy has been erected and fitted with appliances of the most improved kinds. We notice also that further substantial accommodation has been made for out-students. The *Gazette* contains a description of the new buildings and an account of the College live stock; the dairy herd contains specimens of nine breeds, and the specimen flock of fifteen different breeds. An article by Mr. J. M. Muir-McKenzie, on cultivation in the Western Ghâts, gives a description of the prevalent method of cultivation in this part of the Bombay Presidency, by means of wood ash and *râb*; this style of native agriculture entails the destruction of much jungle and denudation of the hills to the detriment of the low lands; it raises various difficulties between the natives and the forest and other officials, and any attempt to grasp its scientific and economic bearings is worth careful attention.

UNDER the title of "Malvern Field Hand-book and Naturalist's Calendar," Mr. G. E. Mackie, Assistant Master in Malvern College, has published a little volume that will be useful both to residents and visitors. The Hand-book was originally begun for the use of the boys of the Malvern College Field Club, but has been much enlarged.

MR. THOMAS WARDLE, of Leek, has been to India to examine the cultivation of the silkworm (*Bombyx mori*) there, and the methods still in use of reeling the silk. Although the reputation of Bengal silk has gone down greatly during the last twenty-five years, yet microscopical examination satisfied him that the fibre of the Indian silk was quite equal to that of Italian, and that improvement in the machinery and method of

reeling was all that was required. The length of thread, however, in each cocoon was very different, the Indian worm only spinning 150 metres, while the more highly-tended and selected Italian worm produced 650 metres. It is suggested that the Government should rear a limited quantity of cocoons, from which a careful selection of "seed" only shall be made, since much of the present inferior quality is traceable to want of discretion in the choice of breeding stock. A loss to the growers of 60 per cent. of their grubs through hot winds can be prevented by the use of mud huts instead of matted walls only. The profitableness of the business is shown by the fact that the zemindars have been able to exact the highest of all agricultural rents for land where the mulberry is grown for this purpose: more than twelve times the amount paid for land adjoining planted with rice. But they do not realise that such high rents are not practicable now silk is at only half its ordinary price.

Mr. Fortescue, the Superintendent of the Reading Room in the British Museum, has just produced a catalogue which is new, as far as the Museum is concerned, in plan, and which will prove of the utmost benefit to all students, men of science included. It is a catalogue of all the works acquired during the years 1880–85 in all modern languages except Oriental, Hungarian, and Sclavonic, arranged according to subjects. At present the alphabetical system is that employed in the Museum Catalogue, and therefore, unless the student knows, or can ascertain, the name of his author, the Library and its Catalogue are of no use to him. With Mr. Fortescue's Catalogue one can tell at a glance what books have been published during the past five years in any given subject, or branch of a subject, in Europe, America, or the British Colonies. The work contains about 1000 pages, with from 50,000 to 60,000 entries. An analysis of one or two headings will best show the value of the Catalogue. To take "Chemistry," under the sub-head "General" we find, first, all important text-books, then elementary works, both grouped under the different languages; then follow Agricultural, Analytical, Arithmetical, Bibliography, Examination Papers, Inorganic, Medical (with cross-references to Materia Medica and Pharmacy), and, finally, Organic, with about 400 entries in all. This, of course, does not exhaust the subject, for under such heads as Acids, Alkalies, Alkaloids, and so on, throughout the book, we have also the titles of chemical publications. The subject Electricity is a remarkable one for the number of entries under it. They fill ten pages in double columns, and about half refer to the electric light. It is curious to notice, too, that fifty telegraph codes were published in the five years included in the Catalogue; these do not, of course, include the innumerable private and cypher codes.

A United States digest of the Report of the British Commissioners on Technical Education by an eminent pioneer in the work has been issued as a Circular of Information by the Bureau of Education. In the writer's earlier days "apprenticeship was rapidly disappearing and home manufactures were giving place to large mills and factories, and yet the schools in which the young were to be specially fitted for their career in the new order of industries were in a large measure limited to the old in methods and principles"—and far too little has there been any alteration since! The British Commissioners' Report is reprinted and added on to the text of this Circular, but the latter is chiefly an account of the French, German, and Russian technical schools, to the latter of which the writer gives the palm of excellence. In these schools, however, a great deal more than teaching is done. In St. Petersburg material is handled in the most wholesale style, and in Moscow orders for specially difficult work are taken and executed. Valuable, however, as such trained ability may be where trained ability is scarce, it is not a solution of the problem before England and America, where the object is to teach every youth the principles which underlie his work. The average age of youths who enter such institutions is over seventeen, and the course extends over five or six years. The result of much of such training in the advanced manufacturing countries must naturally be, as in Germany already, an overflow of highly-trained polytechnic students seeking something above an intelligent mechanic's work. A specially complete set of schools for teaching the various trades at Chemnitz is described. In France the work of such schools in providing a substitute for the extinct apprenticeship system is so efficient that, it is said, "the effort to avoid teaching trades will not be very successful," and they are found already to revive drooping industries and to make new ones. A most important observation, if generally borne out, is that much of this technical work can be added to, not substituted for, ordinary school work.

We learn from *Naturen* that a committee has been formed at Christiania to promote the long-projected establishment of a zoological garden in the Norwegian capital. The plan suggested by the promoters of the scheme is wisely adapted to the special collection of North European and Arctic animals, such as the Polar bear, reindeer, elk, and the numerous other members of the Cervus family to be found in high latitudes, while no attempt will be made to introduce animal forms belonging to tropical faunas, whose susceptibility to cold makes it difficult to maintain them in health even in zoological stations lying far south of Norway.

We are sorry to learn that bad weather greatly interfered with the success of Herr Stejneger's explorations of the Behring Straits fauna and flora during his last summer's boating voyage. At the extremity of Komandor Bay he believes that he has identified the exact spot at which Behring and his unfortunate comrades were shipwrecked, and where he perished from the effects of exposure in the winter of 1741. Here Herr Stejneger found buried beneath the soil various relics of this memorable expedition, including a thin brass plate stamped with the Russian double eagle. The search for plants and insects was specially unsatisfactory, for the damp mildewed the few specimens collected, and ruined all the cases and herbaria, while it so thoroughly rusted every fragment of steel and iron that all the instruments intended for meteorological and other observations were made useless.

The additions to the Zoological Society's Gardens during the past week include two Military Macaws (*Ara militaris*), a Red and Yellow Macaw (*Ara chloroptera*) from South America, presented by Mr. C. Clifton, F.Z.S.; two Ring Doves (*Columba palumbus*), British, presented by Lord Arthur Russell, M.P., F.Z.S.; a Jay (*Garrulus glandarius*), British, presented by Mr. R. Humphries; two Spanish Terrapins (*Clemmys leprosa*) from Spain, a Spotted Salamander (*Salamandra maculosa*), a Fire-bellied Toad (*Bombinator igneus*), six Axolotls (*Siredon mexicanus*) from Mexico, a Green Lizard (*Lacerta viridis*), European, presented by Mr. Alban Doran, F.R.C.S.; twenty Palmated Newts (*Molge palmata*) from Epping Forest, presented by Mr. G. A. Boulenger, F.Z.S.; a Collection of Sea Anemones, from British Seas, presented by Mr. W. L. Sclater, F.Z.S.; two Ring-tailed Lemurs (*Lemur catta*) from Madagascar, an Asiatic Wild Ass (*Equus onager* ♂) from India, deposited; a Ludio Monkey (*Cercopithecus ludio*) from West Africa, three Red-crested Finches (*Coryphospingus cristatus*) from South America, two Rosy-faced Love-Birds (*Agapornis roseicollis*) from South Africa, a Shining Parrakeet (*Pyrrhulopsis splendens*) from Fiji Islands, a Vinaceous Amazon (*Chrysotis vinacea*), a —— Conure (*Conurus* ——) from Brazil, two Short-eared Owls (*Asio brachyotus*), a Magellanic Eagle Owl (*Bubo magellanica*), a Pudu Deer (*Pudu humilis* ♀) from Chili, purchased; a Hairy-eared

Rhinoceros (*Rhinoceros lasiotus* ♂) from India, two Punjaub Wild Sheep (*Ovis cycloceros*) from North-West India, received in exchange.

OUR ASTRONOMICAL COLUMN

THE INFLUENCE OF PHASE ON THE BRIGHTNESS OF THE MINOR PLANETS.—Dr. G. Muller gives an interesting discussion in the *Astronomische Nachrichten*, Nos. 2724-2725, of the variations in brightness of seven of the minor planets. The determinations of the magnitudes of these objects were made by means of a photometer, on Zollner's principle, attached either to the Steinheil telescope of the Potsdam Observatory, of aperture 135 mm. aperture, or to the Grubb equatorial of 207 mm. aperture. The result of these observations seems to show that there is a real connection between the phase of these planets and their apparent brightness, and that Lambert's law of phase brightness does not apply to them. Dr. Müller further divides the planets he has observed into two classes. In the first class, which embraces Vesta, Iris, Massilia, and Amphitrite, the changes in brightness are only perceptible as the planet approaches opposition; in the second, which contains Ceres, Pallas, and Irene, the changes in brightness seem to be co-extensive with the changes of phase. The planets of the first group thus correspond in their behaviour to the planet Mars, and Dr. Muller thinks we may fairly infer therefrom a similarity in their physical condition to that of the ruddy planet. The planets of the second class would appear, on the other hand, to give a light curve similar to that given by our moon, or rather perhaps by Mercury; it is therefore not improbable that they bear more resemblance in their physical constitution to that body.

COMET FABRY.—The following ephemeris by Dr. S. Oppenheim is taken from the *Astronomische Nachrichten*, No. 2722:—

For Berlin Midnight

1886	R.A. h. m. s.	Decl. ° ′	Log *r*	Log Δ	Brightness
May 3	5 1 16	7 33·1 S.	9·9351	9·2358	381·4
5	6 16 8	22 59·4	9·9617	9·4446	195·2
11	7 3 53	30 30·4	9·9877	9·5698	97·3
15	7 34 43	34 18·8	0·0130	9·6758	53·2
19	7 55 56	36 29 6	0·0373	9·7632	31·8
23	8 11 35	37 53·6	0·0606	9·8364	20·4
27	8 23 52	38 53·2	0·0828	9·8992	13·8
31	8 34 0	39 39·2 S.	0·1041	9·9528	9·8

The brightness on 1885 December 1 is taken as unity.

BARNARD'S COMET.—The following ephemeris by Dr. H. Oppenheim (*Astr. Nachr.*, No. 2714) is in continuation of that given in NATURE for April 1, p. 518:—

Ephemeris for Berlin Midnight

1886	R.A. h. m. s.	Decl. ° ′	Log *r*	Log Δ	Brightness
May 6	1 41 34	39 23·5 N.	9·6858	9·8894	155
10	1 50 59	36 42·5	9·7087	9·8125	199
14	2 8 29	31 42·6	9·7429	9·7266	253
18	2 35 41	23 16·9	9·7828	9·6374	318
22	3 13 3	10 16·3 N.	9·8242	9·5619	371
26	3 58 59	6 32·5 S.	9·8648	9·5291	359

The brightness on 1885 December 5 is taken as unity.

THE APPLICATION OF PHOTOGRAPHY TO ASTRONOMY.—In Appendix III. to the "Washington Observations for 1882," Prof. Harkness, U.S.N., commenting on the difficulty of preventing the solar rays from disturbing the adjustments of a meridian instrument employed in observing the sun, points out that photography seems to afford an escape from the difficulty. He suggests that a transit-circle might be so constructed that its eye-piece could be readily removed, and a sensitive photographic plate inserted just behind its wire system. Then with the eye-piece in position stars can be observed, and the instrumental constants determined in the usual way; while at noon a photographic plate can be inserted, and an instantaneous exposure will suffice to give an image of the sun with the transit and declination wires of the instrument imprinted upon it. The position of the sun's centre relatively to these wires having been measured, this, together with the instrumental constants, the circle-reading and the sidereal time of exposure will give an exact determination of the sun's right ascension and declination. As the instruments will be exposed to the sun's rays only for a few thousandths of a second, no disturbance of its constants can, Prof. Harkness thinks, arise from that cause; and the results, in his opinion, would probably be superior in accuracy to any hitherto obtained by the usual methods.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *MAY* 9-15

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on May 9

Sun rises, 4h. 20m.; souths, 11h. 56m. 16·3s.; sets, 19h. 33m.; decl. on meridian, 17° 25′ N.: Sidereal Time at Sunset, 10h. 43m.

Moon (at First Quarter on May 11) rises, 9h. 12m.; souths. 16h. 58m.; sets, 0h. 36m.*; decl. on meridian, 16° 37′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	3 46	10 19	16 52	5 51 N.
Venus	3 0	9 4	15 8	0 0
Mars	12 40	19 34	2 28*	9 52 N.
Jupiter	14 21	20 39	2 57*	2 50 N.
Saturn	7 1	15 13	23 25	22 50 N.

* Indicates that the setting is that of the following morning

Occultation of Star by the Moon (visible at Greenwich)

May	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
15	θ Virginis	4½	2 42	3 36	93 309

May	h.	
13	16	Jupiter in conjunction with and 0° 25′ north of the Moon.

Variable Stars

Star	R.A. h. m.	Decl. ° ′		h m.
ζ Geminorum	6 57·4	20 44 N.	May 9,	21 30 *m*
			,, 14,	21 40 *M*
S Cancri	8 37·4	19 27 N.	,, 12,	22 56 *m*
R Ursæ Majoris	10 36·6	69 22 N.	,, 12,	*M*
δ Libræ	14 54·9	8 4 S.	,, 9,	2 30 *m*
U Coronæ	15 13·6	32 4 N.	,, 13,	21 17 *m*
R Draconis	16 32·4	67 3 N.	,, 10,	*m*
U Ophiuchi	17 10·8	1 20 N.	,, 10,	11 58 *m*
		and at intervals of		20 8
X Sagittarii	17 40·4	27 47 S.	May 12,	2 20 *m*
			,, 15,	0 0 *M*
W Sagittarii	17 57·8	29 35 S.	,, 11,	2 30 *m*
R Lyræ	18 51·9	43 48 N.	,, 13,	*M*
T Aquarii	20 43·9	5 34 S.	,, 15,	*m*
δ Cephei	22 24·9	57 50 N.	,, 10,	21 30 *m*

M signifies maximum; *m* minimum.

Meteor Showers

Amongst the secondary radiants active at this time are the following:—From Lynx, R.A. 123°, Decl. 40° N.; near δ Libræ, R.A. 223°, Decl. 10° S.; from Delphinus, R.A. 304°, Decl. 7° N.; near ζ Cygni, R.A. 320°, Decl. 18° N.; near κ Andromedæ, R.A. 354°, Decl. 41° N.

BIOLOGICAL NOTES

THE HYMENOPTERA OF THE HAWAIIAN ISLANDS.—In the *Proceedings* of the Literary and Scientific Society of Manchester (vol. xxv. pp. 123-183) is a valuable contribution on the Hymenopterous insect-fauna of the Hawaiian Islands, by the Rev. T. Blackburn, B.A., who resided there for many years, with a short introduction and annotations by Mr. P. Cameron. Eighty-four species are catalogued or described, but Mr. Blackburn says he has taken over 100. The greater part of the species appear to be strictly autochthonous. Of the *Anthophila* (or bees) there are 14 species (excluding the introduced honey-bee), and it is curious that 10 of these belong to 1 genus—*Prosopis*. Of the *Fossores* there are 35 species, and here again there is a paucity of genera, for 19 are included in *Odynerus* and 11 in *Crabro*. Of *Heterogyna* (ants) are only 10 species; and about 25 species of the various parasitic and hyper-parasitic groups. No indication of any of the phytophagous forms occurs in the paper. Before Mr. Blackburn went to the Hawaiian Islands the insect-fauna was almost unknown, so far as what may be termed the

more occult (and therefore the chief) portion of it. Most of what had hitherto been discovered resulted from the casual visits of entomologists (not always trained to the subject). In Coleoptera alone he discovered about 430 species, of which nearly four-fifths appear to be strictly endemic, which is certainly noteworthy in considering the fauna of an insular group of volcanic origin. The minority of more recent "introductions" look largely in the direction of Western North America, with a sprinkling of Polynesian or Australian forms. The Rev. Mr. Blackburn's Hawaiian discoveries in entomology have an important bearing on the selection of naturalists to accompany exploring and other expeditions. A trained observer knows where and how to look, even if in doubt as to what he may find, and is always rewarded by new discoveries. An untrained hand scampers over the country, and, with every desire to distinguish himself, comes back and complains of the barrenness of the land.

VEGETABLE PARASITES OF CODFISH.—Some years ago Prof. Farlow called attention to the presence of a red fungus which was destructive to the dried codfish of the American fisheries (NATURE, vol. xxiii. p. 543). Since then Dr. E. Bertherand has given an account of poisoning which had occurred among the French troops at Algiers, caused, it was believed, by eating dried codfish, which had a vermilion hue owing to the presence of a fungus described by M. Mégnin in the *Revue Mycologique* (vol. vi. p. 114) as *Coniothecium bertherandi*. Specimens of fish with the same colour were also met with at Bordeaux and Dieppe, these latter presumably from Newfoundland. It would appear probable that Mégnin's fungus is the same as that originally described by Farlow as *Clathrocystis roseo-persicina*, Cohn.[1] In addition to this species, however, Farlow has described another parasitic form on the cod, *Sarcina morrhuæ*, which name had to yield in priority to *S. litoralis* of Poulsen, found on mud near Copenhagen, and which has lately been recognised by Saccardo and Berlese as occurring on codfish from Algiers. These botanists seem to think the *Coniothecium bertherandi* identical with *Sarcina litoralis*, and this latter to be but a condition of *Beggiatoa roseo-persicina*; but although they are found in company Farlow sees no good reason to think they belong to the same species. It is curious the form should occur in regions so far apart as New England, Algiers, and salt-marsh mud in Denmark, and it suggests the idea that salt may be the means by which the disaster is spread. Still another species, called *Oidium morrhuæ* by Farlow, by forming small brown spots on the surface of the dried codfish injures its sale, and has been found not only in New England, but also at Algiers.—(W. G. Farlow, *Bull.* U.S. Fish Commission, i. p. 1, February 8, 1886.)

SUPERIMPOSED STAMENS.—Mr. Thomas Meehan suggests a new interpretation for the appearance of superimposed stamens. Stamens are by most, if not by all botanists, regarded as exogenous lateral outgrowths from a caulome, in which latter there has normally been an arrest in its axial development. Stamens, however, occasionally will spring from the inner base of petals, and Mr. Meehan would account for this by taking the petal as the analogue of a leaf on an elongated branch, and the stamen as the development of an axial bud to the petal. "Branching and articulated stamens are frequent in those families that have these organs springing as it were from an axial bud at the base of the petal, as in a diminution or suppressed secondary branch we might expect them to do." In illustration of this idea Mr. Meehan refers to the flowers of *Mahernia verticillata*, Cav., a well-known Byttneriaceous plant from the Cape of Good Hope. The genus is separated from Hermannia chiefly by a cup-shaped gland at the middle of the stamen. A comparison with the axial development of the inflorescence shows the stamen to be formed on precisely the same plan, Mr. Meehan thinks, as the biflowered peduncle. This latter is simply a diminutive branchlet; after forming one node the longitudinal development becomes nearly arrested, and there is a short pediceled flower, then the bud in the axil of the bracteolate leaflet pushes up and over this, giving rise to the longer-stalked flower. So in the development of the stamen, a bud arises in the axil of a petal, the common peduncle is represented by the filament, and the cup-like gland at the middle stands for the bracteole of the bipedicels. Here one of the flower-buds wholly disappears, the innermost becomes the upper part of the filament, the real node may be at the connective, and then the theoretical floral leaves proceed to form the anther. The incised bract is reduced to the fringed cup-like gland from which the stamen proper springs, and he concludes from a survey of the whole subject that in many cases superimposed stamens are the development of theoretical axial buds at the base of the petals, and not the result of an interposition of an extra whorl of leaves for which there seems no warrant in phyllotaxy. It will be seen that even on this explanation the true stamen is phyllomic; the fact that foliage leaves often have stipules ought not, in a consideration of this interesting subject, to be overlooked. Mr. Meehan's observations may throw some light on the herotaxy of the floral organs.—(*Proc. Acad. Nat. Sci.*, Philadelphia, 1886, p. 9).

[1] *Bacterium rubescens*, Lank.

STRUCTURE OF LINGULA PYRAMIDATA.—From a very important memoir on the structure of this species by Dr. H. G. Beyer, we condense the following. In 1870, when Mr. Dall was studying the species of Lingula, he separated those species which he found provided with raised fulcra for the attachment of certain muscles, forming a median septum or one or two divaricating septa on the other valve, and formed for them the genus Glottidia. All of the known species (four to six in number) are exclusively to be found in American waters, while not a single species of Lingula has been found to occur in America. While the true Lingulas are almost always attached to a fixed rock or stone, Glottidia attaches itself, if at all, only when adult, and usually to a very small pebble or bit of shell. As to the structure of the shell, the author confirms in great measure the observations of Gratiolet, but describes the cuticle as a thin homogeneous layer, and immediately beneath it, sometimes aggregated in clusters, sometimes arranged in linear series, and at other times again irregularly scattered, he found a series of little round bodies, staining with hæmatoxylin, homogeneous, and without nuclei; these are regarded as homologous if not analagous to the bodies occurring within the organic septa in the shell of the Testicardine Brachiopods. Immediately adjacent to the cuticle and this layer of bodies comes a broad layer of horny substance and internally a thin calcareous layer, and these horny and calcareous layers alternate with each other in a number varying with the age of the animal. Towards the periphery the cuticle and horny layer alone are found, and these join the supporting layer of the mantle margin. A very intimate structural relationship exists between the body-wall, the mantle, and the peduncle. It seems doubtful whether the structures described by Vogt, Owen, Hancock, and others as muscles are in reality muscular in character. All the true muscles are smooth muscle-fibres, but other so-called muscles seem to be rather mesenchymatous supporting substance, lacking contractility, but perhaps possessing elasticity. The author's observations on the vascular system confirm rather the views of Shipley, Schulgin, and Morse than those of Hancock, and no central propelling organ over the posterior slope of the stomach was on transverse sections found. The number and division of the nervous ganglia indicated by Hancock for Waldheimia seem to be the same in Lingula, though Hancock's views have lately been criticised by Van Bemmelen. Hancock's details as to the reproductive organs are in great measure confirmed. Three excellent plates of anatomical details accompany this memoir.—("Studies from the Biological Laboratory, Johns Hopkins University," vol. iii. No. 5, March 1886.)

THE CUCKOO.—In the note on the cuckoo in the Biological Notes of April 1 (p. 519, line 6 from bottom), *January* was inadvertently printed for *June*.

NOTE ON EARTHQUAKES IN CHINA[1]

I HAD prepared for presentation to the Seismological Society of Japan a tabulated account of earthquakes that have been recorded in Chinese annals for the past thirty-seven centuries, but it was destroyed by fire during a riot last winter, and with the paper were destroyed also the works from which the seismic facts were derived. Perhaps, however, some general remarks which those records suggest may not be devoid of value.

Nothing can be inferred anent the relative frequency and destructiveness of earthquakes in ancient and modern times from Chinese history; from the earliest recorded earthquakes of Mount Tai in Shantung 1831 B.C. to the commencement of the

[1] Communicated to the Seismological Society of Japan by D. J. Macgowan, M.D.

Han dynasty 200 B.C., only twelve are recorded; tradition and written archives noting those only that presented extraordinary features; a bald list merely mentioning a disturbance of the rivers of the I. and Lo Hanan, 1808 B.C.; Wei, Chin, and So in Shensi, 778 B.C.; the formation of long chasms in the loess, 345 and 206 B.C. From the Han period, notices of the phenomena of course increase, accompanied betimes with a few details relating mainly to loss of life, and the succour afforded to survivors. Geographically considered, earthquakes in China may be grouped as insular, littoral, and inland.

On the island of Formosa earthquakes are hardly less frequent than in Japan, while on Hainan they are comparatively of rare occurrence. These islands form a portion of the great volcanic chain that girdles the coast of Eastern Asia; the Chinese portion rises from the submarine plateau that overlooks the profound abyss of the Pacific Ocean.

Insular earthquakes affect the mainland but seldom, and to a slight extent, which is noteworthy from the proximity of Japan, the least stable portion of the earth's surface, which seemed inexplicable until Prof. Milne's statistics showed that a large majority of earthquakes in Japan originated beneath the Pacific.

The absence from Chinese and Korean annals of notices of earthquakes in that peninsula long inclined me to regard Korea as comparatively exempt from seismic action, and recently, I addressed Consul E. H. Parker, of H.B.M.'s service in that country, for information, who obtained from the prefect of Chemulpo a communication on the subject, the purport of which is, that earthquakes are so infrequent and harmless that records are not made of their occurrence. It is more than ten years since an earthquake was experienced in that kingdom, and on that occasion no one was injured, nor were buildings thrown down. No information is obtainable on the subject from Manchuria, where presumably earthquakes are uncommon: there is, however, a record of a volcanic eruption having occurred about a century ago in that portion of the empire.[1]

The only existing volcanic action on islands of this coast is on the north of Formosa, near Keelung, where three solfataras are in ceaseless ebullition, affording large supplies of sulphur, and emitting during earthquakes so much hydro-sulphuric gas as to occasion a degree of *malaise* to the residents, and to discolour the white paint of ships.[2]

Facts respecting Formosan earthquakes are so scanty that the following from a Chinese writer is worth citing. It relates to an earthquake that occurred in Northern Formosa in the fifth month of 1693. "During that month the earth shook without cessation. A tract of country in which three villages were situated caved in; the inhabitants, however, had time to escape." Three years after that submergence, the narrator, a mandarin, who was on his way to procure sulphur from the solfatara "could see in a lakelet, where the water was shallow, tops of bamboos and other trees of those villages. While near the solfatara he heard for a day and night noises that resembled a cataract precipitated from a lofty cliff; the sound seemed to be near and all about, but no evidence of the cause of the noise was discoverable When, however, he arrived at the solfatara the mystery was explained, he there heard the same sounds like a rushing of subterranean waters."

Another active volcano is named in a Chinese account of Formosa. It is in Têngshan district in the southern portion of the island at Red Hill, near the Tanshin Creek, on a plateau. Probably it has not been in open action since Formosa was opened to trade, as it does not appear to be known to foreigners.

Formosan seismic action occasionally causes tremors to be felt on the mainland, which is due to the ordinary direction of earthquakes on that island, which are generally from south to north or the reverse. The Liuchiuan group is the centre of seismic force that does not appear to extend beyond those islands.

Submarine disturbances not unfrequently attend the insular earthquakes; the sea sometimes rises on the Formosan coast sixteen feet above the usual height. Independently of the terrene commotions of Formosa, its adjacent waters appear to be subject to submarine agitations occasioning what records of the mainland style "third" or supplementary tides; but these are of rare occurrence. The "tide-rips" that have attracted the attention of hydrographers are notable phenomena, but the following, from a local gazetteer, seems to indicate the existence of phenomena that cannot be referred to tidal action:—"Peculiar noises of the sea are sometimes heard which are commonly regarded as indicative of change of weather, sounds from the north foreboding rain, those from the south being followed by wind. Hissing noises are heard, at times they are low, at others loud; when low, they resemble the beating of a drum or the dropping of beans on that instrument: now, the sounds are near; anon, they are distant; stopping suddenly or continuing for hours. When the noise is loud, it is more noisy than the voices of a hundred thousand men, and the sea bubbles up: in very protracted cases the noises continue day and night for half a month; and when of short continuance the sound lasts three or four days. Coast landers err in supposing that these noises have connection with the weather. They are absent during rains and in drought, in winds and in calms. . . During the sounds, the sea is agitated by fearful billows and furious waves." If that extraordinary seething and roaring of the ocean were synchronous with earthquakes, the fact could not have escaped observation: indubitably that graphic description applies to submarine volcanic action; to which the submarine plateau of eastern Asia is subject, and to which also I attribute the supplementary tides of the adjacent coast. Some thirty years ago an island was thrown up by a submarine volcano on the south of Formosa; the pumice which is cast on the northern shores of that island is evidently a submarine production.[1]

As proximity to the belt of volcanic islands seldom disturbs the mainland of the northern littoral, so the adjacent coast of Southern China and Annam enjoy like exemption from insular throes: Chehkiang and Fuhkien are sometimes slightly visited by Formosan shocks, and even the Canton coast slightly, but Philippine earthquakes never affect Annam.

Earthquakes on the coast of China are frequent, but slight and harmless. Their harmlessness is evinced by the tall slender pagodas that adorn the hills and valleys, and they are generally very limited in area, with great diversity of direction, but a majority being from south-west to north-east.

The southern provinces of China, and yet more Indo-China, appear to be comparatively exempt from earth throes, which, however, may be due to lack of information from those regions, but there is evidently no seismic zone in tropical or sub-tropical eastern Asia such as exists in our mid-latitudes.

The tremors that are experienced in Chehkiang, Kiangsu, and coterminous regions to the west, are sometimes followed by the appearance on the ground of substances that in Chinese books are styled "white hairs." When I first called attention to records of that kind that are found in local gazetteers, I suggested that they might be crystals precipitated by gaseous emissions, such as were once reported as occurring after an earthquake in the south-west of the United States; from later descriptions of these "horsetail-like" substances I incline to the opinion that they are organic, perhaps mycillium.

In the summer of 1878 the vernacular press gave an account of the occurrence of the phenomena at Wusoh, a city on the grand canal, thirty miles north of Suchau. "At noon, June 12th of that year, shocks of an earthquake were experienced, which lasted several minutes (*Sin.* 'for the space of time taken in swallowing half a bowl of rice'); the motion was so great that sitting or standing was difficult, but no harm was done. Two days later at night there was a severer shock, after which, within and without the walls of the city, white hairs resembling a silvery beard, about three inches in length, were found, which boys pulled out of the ground, gathering handfulls in a short space of time." My list of Chinese earthquakes for the past two thousand years having been destroyed by fire I am unable to indicate the regions in which earthquakes were followed by the emission of "hairs," but my impression is that all, or nearly all, are alluvial valleys.

The chief foci of inland earthquakes are Yunnan, Szechuan, Shensi and Kansuh—and less frequently Shansi, Chihli, Shantung, and the central provinces, where they are more violent than in other portions of the empire, and frequently present continuous or protracted action, for example:—

A series of earthquakes occurred at Taiyuan, the capital of Shansi, in 1882, followed by shocks at brief intervals for a year. An earlier series occurred in the province of Chihli; the district

[1] Perhaps the following may be explained as a result of volcanic action far distant from Peking. In the month of June, 1465, during a gust of wind at the capital a sound was heard as of hail falling on the ground, when pellets the size of cherries were picked up. On breaking them open they emitted a sulphurous odour. The writer says he could not have regarded such a phenomenon as credible had he not himself witnessed it.

[2] "Head-dizziness" is said to be an occasional accompaniment of earthquakes on the mainland Slight shocks that occurred at Weichang-November 3, 1885, are described in the *Shen-pau* as exhibiting that phenomenon.

[1] For accounts of the volcanic region of Northern Formosa see Taintor's "Imperial Maritime Customs Report, 1865," and Hancock, 1881.

city Chiichow suffered most, not a house remained standing, many lives were destroyed; frequent shocks occurred for a year after. The province of Szechuan is also liable to continuance of seismic throes, one of these commenced in the fourth month, 1462, and continued eleven months—there were in all 375 shocks.

In the loess formation of Northern China (discovered and described by Baron Richthofen) the land is not unfrequently riven by earthquakes forming long narrow chasms of unknown depth that gradually disappear on account of the vertical cleavage and unstratified nature of loess.

In the first decade of the fourth month, 1828, an earthquake caused a fissure over three miles in length, twenty to thirty feet broad, from which a vapour issued that proved fatal to many: people, animals, houses, and tombs were ingulfed. About two months later, during heavy rain, the chasm gradually filled up.

The chief earthquake region of China lies in a great seismic zone, which extends from near the gulf of Chihli to the shores of the Caspian—including Turkestan and the Aralo-Caspian depression. In Eastern Turkestan they present a periodic character (five per annum with remarkable regularity). Yet there are few portions of the world so far removed from active volcanoes. Recent Russian exploration has discovered that the supposed Tienshan volcano is merely a solfatara, or an ignited coal-field.

Observations of officers appointed by the Emperor Chienlung to examine the newly subjugated territory in reference to these "firefields," are several. They say: "Three days travel to the east of Okishu and to the south of the hill at Palikeh there are several firefields. The ground is of a red colour, and a number of variegated stones are piled upon each other in the neighbourhood; from the middle of which flames upward of a foot in height are emitted: they are alternately extinguished and lighted up, while the smell is so strong as to render a near approach to the place impossible. For a distance of about 100 *li* not a blade of grass, not an inch of wood, nor a drop of water can be seen. From the peculiar smell of the fire thus raised, it is imagined that the soil must be strongly impregnated with sulphur."

The same work represents earthquakes as so common in Eastern Turkestan and the desert, that to the inhabitants "they are not considered strong; four or five occur every year; even when violent, they merely cause the doors and windows to rattle, but on account of the firm and adherent character of the soil, and thick walls and light roofs in common use, the houses are never thrown down."

A recent English traveller[1] makes a similar statement respecting Mid-Asian earthquakes generally. At Tashkend they generally average five in a year, but so slight, as not to be noticed by anybody. In that part of the world earthquakes appear to be most frequent at the close season. In the western portion of the seismic zone, they are of greatest frequency and violence in mountain regions.

Anent the opinion of M. Perrey, that a maximum of earthquakes is coincident with the moon's perigee, I submit the following statistical fragment that escaped the loss referred to: it is partially confirmatory of Prof. Milne's observations, that cold weather furnishes the maximum of frequency.

Lists of 738 continental shocks:—

1st month	65	5th month	46	9th month	56
2nd ,,	82	6th ,,	63	10th ,,	43
3rd ,,	72	7th ,,	70	11th ,,	65
4th ,,	49	8th ,,	70	12th ,,	88

(The first day of the first month occurs about February 6th, or at the new moon which falls nearest to the point when the sun is in the fifteenth degree of Aquarius.) In their seismic records the Chinese seldom designate the day of the month (moon) when earthquakes occur. Yet a considerable number may be found. Seventy-two cases show twice as many in the first and second as in the third and fourth quarters of the moon's phrases: forty-eight in the former period, and twenty-four in the latter; of that number fifteen occurred at the syzygies. The 6th day shows the largest number, twelve. None took place on the 2nd, 5th, 13th and 14th; one occurred on each of the following, 4th, 7th, 17th, 20th, 22nd, 23rd, 24th, 28th, 29th. Hours are rarely given; so far as they go, they show that a large majority are nocturnal.

[1] Lansdell's "Russian Central Asia," 1885.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The following list of lectures and classes in Natural Science has been arranged for the summer term:—

Physics.—In the Clarendon Laboratory Prof. Clifton lectures on Instruments and Methods employed in the Study of Optics. Practical instruction in Physics is given by the Professor and by Messrs. J. Walker and A. L. Selby. At Christ Church, Mr. Baynes lectures on Electro-Kinematics and Dynamics, and has a class for practical instruction in Electric and Magnetic Measurements. At Balliol Mr. Dixon lectures on Elementary Electricity and Magnetism. At Trinity the new Millard Laboratory will be opened for instruction in Mechanical and Electrical Engineering under Mr. Frederick Smith.

Chemistry.—In the Chemical Department of the University Museum Dr. Odling lectures on Some Special Points in Organic Chemistry. Mr. Fisher and Dr. Watts continue their courses on Inorganic and Organic Chemistry respectively. Mr. W. R. Dunstan lectures on Organic and Pharmaceutical Chemistry. Practical instruction is given in the laboratories by Messrs. Fisher, Watts, Baker, and Marsh. At Christ Church Mr. Vernon Harcourt has a class for Quantitative Analysis, and Mr. Dixon for Gas Analysis.

Animal Morphology.—In the Morphological Department Prof. Westwood lectures on the Haustellated Orders of Winged Arthropodous Animals. Prof. Moseley lectures on the Mammalia, Mr. Baldwin Spencer on Embryology, and Mr. J. B. Thompson on the Osteology, Distribution, and Odontography of Birds and Mammals. Practical instruction is given by Prof. Moseley and by Messrs. Robertson and Spencer. In Human Anatomy Mr. A. Thomson lectures on the Vascular and Respiratory Systems, and gives demonstrations on Topographical Anatomy. Daily instruction is also given in Practical Anatomy.

Physiology.—In the Physiological Department Prof. Burdon Sanderson lectures on the Chemical Processes of the Animal Body, and on Elementary Physiology. Mr. Dixey lectures on Histological Methods. Practical instruction is given daily.

Botany.—At the Botanic Garden Prof. Balfour lectures and gives practical instruction in Vegetable Morphology and Physiology. Prof. Gilbert gives four lectures on Rural Economy.

Anthropology.—Dr. Tylor lectures on the Origins of Civilisation.

Geology.—Prof. Prestwich lectures on the Secondary and Tertiary Series as illustrated by the geology of the neighbourhood of Oxford. Each lecture is followed by a geological excursion.

CAMBRIDGE.—The Special Board for Biology and Geology have recommended the following grants from the Worts Fund:

(1) 50*l.* to Mr. W. Bateson, of St. John's College, to assist him in investigations into the fauna of lakes in the neighbourhood of the Sea of Aral in 1886, and an additional 50*l.* if he continues his investigations into the summer of 1887. Mr. Bateson's investigations into the development of Balanoglossus in the Southern United States have, it is well known, been of great value.

(2) 60*l.* to Mr. A. C. Seward, of St. John's College, to assist him in studying and collecting fossil plants in Belgium and France.

(3) 35*l.* to Mr. Hans Gadow, of King's College, to assist him in exploring the ossiferous caves of Portugal, which he has already partly explored during two former visits. Prof. Boyd Dawkins strongly recommends the continuance of these explorations.

(4) 25*l.* to Mr. C. Potter, of Peterhouse, to assist him in elucidating the life-history of the alga parasitic on the water-tortoise in Portugal.

In the list of lectures issued by the Board of Physics and Chemistry for the present term we note that Dr. Ruhemann, assistant to the Jacksonian Professor, will lecture on Gas Analysis, and also on the Aromatic Bodies. The other chemical courses repeat the usual advanced and elementary courses.

In Advanced Mathematics Mr. Forsyth lectures on Thermodynamics, Mr. Glaisher on Theory of Errors, Mr. Webb on Dynamics of a System. The latter course will be continued during the Long Vacation, when also Prof. Darwin will lecture on the Theory of Attractions, Potential, and Figure of the Earth.

In Geology Prof. Hughes lectures on Stratigraphy and Cam-

bridge Geology, Mr. Marr on Advanced Palæontology, especially the Graptolites, Mr. Harker on Microscopic Petrology.

In Botany Dr. Vines is lecturing on the Cryptogams; Mr. F. Darwin on Physiology, and Mr. Potter on Advanced Systematic Botany.

In Zoology, Mr. Sedgwick continues the courses of Elementary Biology, and the Anatomy and Embryology of the Vertebrata; Mr. Gadow gives a summary Course on the Palæontology of the Vertebrata.

In Physiology, beside Prof. Foster's Elementary Course, we have advanced lectures by Dr. Gaskell, Dr. Hill, and Mr. Langley.

Prof. Macalister lectures on the Variations in the Skeletal, Muscular, and Nervous Systems of the Races of Mankind.

The Special Board for Physics and Chemistry report to the Vice-Chancellor on the new Mechanical Science Tripos:—

In consequence, the report states, of the Grace passed March 11, 1886, confirming their report, dated December 14, 1885, the Board have drawn up regulations for the New Tripos in Engineering, Physics, and Chemistry, for which they would propose the name "Mechanical Science Tripos." They do not think it desirable that the University should examine in subjects for which the University does not or may not easily provide adequate teaching, and have therefore made the examination in Engineering mainly an Examination in Mechanical Engineering. They have included, however, in it such elementary portions of Civil Engineering as can be taught in Cambridge and such as may often be advantageously studied by those who are intending to become Mechanical Engineers. With respect to the Engineering papers in Part II. of the Examination one paper would test the ability of the candidates to indicate how a given design should be carried into execution; another would include questions on steam and the steam-engine besides other prime movers, and also on boilers and furnaces; a third would include questions on bridges, roofs, arches, abutments, elementary hydraulics, strength of materials, and elementary building construction. In the Examination in Physics in Part II. the papers would contain questions on the application of dynamics to physical phenomena; gravitation; attractions; hydrostatics and hydrodynamics; properties of matter, including elasticity, capillarity, diffusion, and viscosity; heat; kinetic theory of gases; radiation; light, including the application of the undulatory theory to the problems of geometrical optics; mineralogical physics; acoustics; meteorology; cosmical physics; electricity and magnetism; reduction of observations. The Practical Examination would extend over two days, the Examination on the first day being of such a nature as would test the knowledge of the candidates in the general methods of laboratory work; on the second day a list of experiments would be given, one or more of which each candidate would be expected to complete.

SCIENTIFIC SERIALS

Bulletins de la Société d'Anthropologie de Paris, tome 8eme, 4me fascic., 1885.—On the facial and cranial muscles of a young gorilla, by M. Chudzinski. The subject of this post-mortem examination, a young male, was 98 centimetres in height. The muscles of the head and face were the same in number as in the human species, but in form and dimensions they exhibited certain differences, being combined into a single fleshy mass, which covered most of the face.—M. Pozzi laid before the Society various anatomical characteristics with reference to the comparative constitution of the muscles of the Negro and the white races.—M. Folley drew attention to the greater anastomosis of the subcutaneous abdominal veins of the Negro, and the importance of this peculiarity in giving to the organism a greater power of resisting the action of rapid variations of atmospheric or aqueous pressures.—On the common origin of Malays and Vedahs, by M. Beauregard.—On the universal language of F. Sudre, by M. Gajewski. The basis of the system proposed fifty years ago by M. Sudre is the musical nomenclature of the vocal notes, *do*, *re*, &c., and from these he elaborated a language which claims to be equally capable of expression by means of musical instruments and the voice. The defects and impracticabilities of Sudre's proposed musical language were considered at length by MM. Kerckhoffs, Dally, and Dehoux.—Suggestions for the modification of Broca's method of determining the direct absolute cranial capacity, by M. Topinard. The points chiefly insisted on are the different results yielded by fresh, and often-used, lead, the latter being valueless after 100 cubage determinations.—On the cause and nature of the vitrification observed in tumuli, and other ancient structures, by M. Manouvrier.—Report of the recent Anthropological Exposition at Buda-Pesth, by Dr. R. Blanchard.—On the dimensions and location of the dolmens of St. Nectaire, by Dr. Verrier.—History and anthropology, by Dr. Fauvelle. The writer draws attention to the tissue of errors which works intended for the instruction of the young continue to promulgate, as exemplified in the current historical explanations of the origin and usages of earlier races.—On the Gallic habitation of Mané Gobenne, Carnac, by M. Gaillard. The finds, which consisted principally of flints and pottery, included a string of twenty-three green serpentine beads cut into various forms.—On certain unique objects shaped like fishes, found in the Mammoth Cave in Varsovia, by M. Zawisza, and supposed to have been employed as fetishes by sorcerers.—On the significance of certain strongly marked impressions on the inner surface of a skull, by M. Manouvrier. Such impressions have been regarded as an evidence of imperfection in the cerebral convolutions, and of consequent mental deficiency.—On man of the age of Palæolithic pottery in the Lozère district, by MM. Martel and L. de Launay. The local finds attest the co-existence there of man and the cave-bear, and the fabrication of pottery at the time.—On the flint implements of Croix Fringant, near Cognac, by M. Germain.—On the calcareous islets of Taled Sah, in the inner sea of the Samsans, in the Malayan peninsula, and the natives who dwell in natural caverns and are engaged in collecting edible swallow-nests, by M. Macey.—On the displacement of the brain in accordance with the different attitudes assumed by the body, by M. Bonnard.—On the form of the hand and figure of Asiatics, by Dr. Mugnier.—Anthropometric and other observations of three Australians now being exhibited in Paris, by M. Topinard.—On the development of the cranium in the gorilla, by M. Deniker. It is found that, while the frontal region is developed, like other parts of the cranium, as rapidly in the gorilla as in man from the middle of fœtal life to the eruption of the milk molars, different relations supervene after the latter period, the cranial development of the gorilla becoming much more strongly marked in the posterior and inferior than in the anterior regions. At the same time the upper maxillary rapidly acquires its characteristic prognathic form. An almost equal degree of prognathism is observable in the adult Negro, or Australian, and in the infant gorilla, but with its growth the latter acquires a facial angle which is smaller than that of any human cranium.—Ethnographic observations on the cerebral function, by M. Fauvelle.—On a case of an hermaphrodite, by M. A. de Mortillet.—Notes on the post-mortem appearances of an imbecile, by MM. Doutrebente and Manouvrier.—Report, by M. Letourneau, on the Godard Prize Essay of M. de la Calle (1885) on the earliest attempt at speech in infants. M. de la Calle attempts to draw a parallel between the first enunciation of the vowel-sounds *a*, *e*, *o* by infants, and the monosyllabic character of certain languages belonging to various peoples of the far east of Asia, which have scarcely yet entered upon the more advanced stage of lingual agglutination.

SOCIETIES AND ACADEMIES

London

Royal Society, April 15.—"Dynamo-Electric Machines." By John Hopkinson, D.Sc., F.R.S., and Edward Hopkinson, D.Sc.

Omitting the inductive effects of the current in the armature itself, all the properties of a dynamo-machine are most conveniently deduced from a statement of the relation between the magnetic field and the magnetising force required to produce that field. This relation given, it is easy to deduce what the result will be in all employments of the machine, also the result of varying the winding of the machine in armature or magnets. The magnetic field may be expressed algebraically as a function of the magnetising force, or more conveniently by a curve (*Proceedings* of the Institution of Mechanical Engineers, April 1879, p. 246). Amongst the empirical formulæ which have been proposed to express the electromotive force of dynamo-machines in terms of the currents around the magnets, we may mention that known as Fröhlich's, where $E = \frac{ac}{1 + bc}$, E being the electromotive force of the machine at a given speed, c the exciting current, and a and b constants. For some machines this hyperbola is said to express observed results fairly accurately. In our

experience it does not sufficiently approximate to a straight line in the part of the curve near the origin, and gives too high results for large values of c.

One purpose of the present investigation is to give an approximately complete construction of the characteristic curve of a dynamo of given form from the ordinary laws of electromagnetism and the known properties of iron. Let n be the number of convolutions on the magnets, c the current round the magnets, l_1 the mean length of the lines of force in the iron of the armature, A_1 the area of section of iron in the armature, l_2 the distance from iron of armature to iron of pole pieces, A_2 the area of the magnetic field in which the wires move corrected for its extension round the edge of the pole pieces, l_3 the total length of the magnet cores, A_3 the area of the magnet cores, l_4 the mean length of lines of force in the yoke connecting the magnet limbs in machines of the type on which we have principally experimented, A_4 the area of section of the yoke, l_5 the mean length of the lines of force in each pole piece, A_5 the main area of section of pole piece, I the total induction through the armature when no current passes in the armature, and νI the total induction in the magnet cores; and, finally, let the relation between the magnetic force (α) and induction (a) (*vide* Thomson, "Electrostatics and Magnetism," p. 397, and Maxwell, "Treatise on Electricity and Magnetism," vol. ii. p. 24) be represented by the equation $\alpha = f(a)$, then the characteristic curve is—

$$4\pi nc = l_1 f\left(\frac{I}{A_1}\right) + 2l_2\frac{I}{A_2} + l_3 f\left(\frac{\nu I}{A_3}\right) + l_4 f\left(\frac{\nu I}{A_4}\right) + 2l_5 f\left(\frac{I_5}{A_5}\right).$$

If the relation between α and a be given in the form of a curve, this formula indicates at once a perfectly simple graphical construction for the characteristic. Taking the curve of magnetisation determined by one of us for wrought iron, and constructing a characteristic in this way, we have obtained a theoretical curve which agrees over a long range with the actual results of observation on a dynamo-machine more closely than any empirical formula with which we are acquainted.

To determine ν, a wire was taken once round the middle of one magnet and connected to a ballistic galvanometer, a known current was then either suddenly passed round the magnets or short-circuited, the elongation of the galvanometer being noted. A similar observation was made with the same current, the galvanometer being connected to a single convolution of the armature in the plane of commutation. The ratio of the two elongations is the value of ν.

The distribution of the waste field $(\nu - 1)$I was roughly ascertained in a similar manner.

The currents in the fixed coils round the magnets are not the only magnetising forces applied in a dynamo-machine. The currents in the moving coils of the armature have also their effect upon the resultant field. In well-constructed machines the effect of the latter is reduced to a minimum, but it can be by no means neglected. This introduces a second independent variable, viz. C, the current in the armature. The effect of the current in the armature depends upon the lead given to the brushes. Denote this by λ, which we may also regard as an independent variable, as it is subject to arbitrary adjustment.

If $I = F(4\pi nc)$ be the characteristic curve when no current passes through the armature, then

$$I + \frac{\nu - 1}{\nu} 4\lambda m C \frac{A_2}{l_2} = F\left(4\pi nc - \frac{4\lambda m C}{\nu}\right),$$

where m is the number of convolutions in the armature. Here we omit the comparatively unimportant portion of the magnetic force in the core of the armature and the pole pieces. From this formula it is not difficult to deduce a geometrical construction for the characteristic surface (*vide* "Practical Applications of Electricity," lectures delivered at the Institute of Civil Engineers, 1882–83, p. 98). The equation may be thus expressed in words, if λ be such that the coils at commutation embrace the whole or nearly the whole induction. The effect of the current in the armature upon the difference of potential between the brushes of any machine, is the same as that of an addition to the resistance of the armature proportional to the lead of the brushes, and to the ratio of the waste field to the total field, combined with that of taking the main current $\frac{m\lambda}{\nu\pi}$ times round the magnets in a direction opposite to the current c. Many consequences can be deduced, of which we may notice the following:—In a series-wound dynamo C is equal to c, and if c be increased beyond a certain point, I must attain a maximum and then diminish; this has been frequently observed. We now see that it depends upon the existence of a waste field. Secondly, let the coils of the magnets be entirely disconnected, and let λ be the negative; if the armature be short-circuited through a small resistance and be run at a sufficient speed, a large current may be produced in the armature. This latter deduction we have verified by direct experiment.

The efficiency of the type of dynamo-machine upon which the experiments before indicated have been made, has been accurately determined by the device of coupling two similar machines, both mechanically and electrically, so that one should act as a generator of electricity, driving the other electrically, whilst the latter acted as a motor driving the former mechanically; the loss of power required to keep the whole combination in movement being determined by direct dynamometric measurement, and the power passing electrically from the one machine to the other being measured by ordinary electrical appliances.

The whole of the experiments were carried out at the works of Messrs. Mather and Platt, to whom we are indebted for the exceptional opportunities we have enjoyed of putting theoretical conclusions to the test of experiment on an engineering scale.

Zoological Society, April 20.—Prof. W. H. Flower, F.R.S., President, in the chair.—Mr. O. Salvin, F.R.S., exhibited a living specimen of a foreign worm (*Bipalium kewense*), found in a garden in Sussex.—The Secretary read an extract from a letter addressed by Mr. R. A. Sterndale, F.Z.S., to Sir Victor Brooke, concerning a case of hybridism between *Ovis hodgsoni* and *O. vignei*.—Mr. J. Bland Sutton, F.Z.S., read a paper in which he gave an account of some of the investigations he had made during the past twelve months into the diseases affecting the mammals living in the Society's Collection.—A communication was read from Dr. O. Finsch, C.M.Z.S., describing a new species of wild pig from New Guinea, which he proposed to call *Sus niger*.—Mr. Smith Woodward read a paper on the relations of the mandibular and hyoid arches in a Cretaceous shark (*Hybodus dubrisiensis*, Mackie).—A communication was read from Prof. R. Collett, of Christiania, C.M.Z.S., containing an account of the hybrid between the willow-grouse (*Lagopus albus*) and the black grouse (*Tetrao tetrix*), which occurs occasionally in Norway, Sweden, and Northern Russia, and of which the author had examined altogether thirteen specimens, most of them of the male sex.—Mr. G. A. Boulenger, F.Z.S., gave the description of a new Iguanoid lizard living in the Society's Gardens, for which he proposed the name of *Ctenosaura erythromelas*. The exact locality was unknown.—A second paper by Mr. Boulenger contained remarks on specimens of a scarce European frog (*Rana arvalis*) exhibited in the Society's Menagerie.

Royal Meteorological Society, April 21.—Mr. W. Ellis, F.R.A.S., President, in the chair.—Mr. L. J. Petre and Mr. G. B. Wetherall were elected Fellows of the Society.—The following papers were read:—The climate of Killarney, by the Ven. Archdeacon Wynne, M.A., F.R.Met.Soc. The climate is determined partly by its geographical position, and it has the benefit of proximity to the south-west coast, with all the modifying influence of the Gulf Stream. The temperature, however, is locally modified, and a decided difference is found to exist between that of Valencia and of Killarney. The author shows that Killarney is colder than many other places in Ireland, and this he attributes to the fact that it is in a great irregular basin surrounded by mountain ranges for about a third, and by hilly ranges elevated some hundreds of feet above the lakes on most of the remaining two-thirds of the circle.—Note on the probability of weather sequence, by Lieut.-Col. C. K. Brooke, F.R.Met.Soc.—Account of the cyclone of June 3, 1885, in the Arabian Sea, by Capt. M. T. Moss. The author, who was in command of the s.s. *Inchulva*, while on a passage to Bombay had, when near Aden, the misfortune to encounter a most furious storm on the above date. This storm, which was apparently not of very large dimensions, was exceedingly severe, and was accompanied by an immense wave which caused several fine steamers to founder.—Results of solar radiation observations in the neighbourhood of Birmingham, 1874–84, by Rupert T. Smith, F.R.Met.Soc.—Results of meteorological observations made in the Malay Native State of Selangor during 1884, by A. W. Sinclair, L.R.C.P. These observations were taken at four stations, viz. Kwala Lumpor, Klang, Kajang, and Kwala Langat. The mean temperature of the district is about 80°, and the rainfall about 90 inches.

DUBLIN

University Experimental Science Association, March 16.—The following communications were made:—Prof. J. E. Reynolds, on action of silicon tetrabromide on thiocarbamide.—Mr. H. L. Crosthwait, the Forth Bridge.—On the melting-points of minerals, by J. Joly, B.E. An account of experiments with the meldometer, in which the temperature of the platinum strip, acting as the stage of a microscope, was determined in terms of its resistance according to Siemens's formula. It was mentioned that the order of fusibility assumed in Van Kobel's scale is erroneous. The true order seems to be: (1) stibnite; (2) natrolite; (3) adularia; (4) actinolite; (5) bronzite; (6) almandine. The blowpipe being a powerful chemical agent, may evidently mask the phenomena of fusion with secondary effects. Fair comparison is impossible with it, the shape and conductivity of the specimen used affecting the result. Comparison on the meldometer is not open to these objections. It is very advisable that a scientific scale of fusibility should be adopted for the use of mineralogists. If this scale rested on the melting points of easily-prepared salts, it would then always be easy to determine by comparison the melting-point of a mineral. Approximate determinations could thus be readily effected on very minute quantities of matter. In the author's experiments the substances are reduced to a fine powder, the phenomena attending fusion being observed with a 1″ object-glass. These phenomena are often very characteristic and beautiful.

PARIS

Academy of Sciences, April 27.—M. E. Blanchard in the chair.—On the quantitative analysis of the organic carbon contained in soils which absorb free nitrogen, by M. Berthelot. The author's researches on the direct absorption of free nitrogen by various argillaceous soils through certain minute organisms have led him to seek some other measure capable of indicating the proportion of these organisms in the ground. It being apparently impossible to isolate them, some idea of their abundance may still be formed by a quantitative analysis of the carbon entering into the constitution of their tissues. Hence the present inquiry, which promises to raise some new and extremely delicate problems.—Observations relative to the proportion and quantitative analysis of the ammonia present in the ground, by MM. Berthelot and André. The experiments conducted during the last four years by the authors at Meudon on the general growth of vegetation and on the formation of nitric compounds, both in plants and in the soil, have led to certain observations here communicated on the processes employed in the quantitative analysis of the ammonia and the starchy compounds. It is inferred generally that the analysis of the ammonia present in the soil should be made without any desiccation, and that arable ground, when watered, tends continually to liberate the ammonia of the ammoniacal salts contained in it.—On the nitric substances contained in rain-water, by MM. Berthelot and André. A process is explained for determining by analysis the exact quantity of nitric substances conveyed to the earth by meteoric waters.—On the movements of meteorites in the atmosphere, by M. Faye. These remarks are made in connection with M. Daubrée's essay on "Meteorites and the Constitution of the Terrestrial Globe," recently presented to the Academy by the author.—Discourse pronounced at Montdidier on the occasion of the celebration of the Parmentier centenary, by M. Chatin.—Note on the meteorological observations made at the Montpellier School of Agriculture since last summer with the registering actinometer, by M. A. Crova. The results already obtained for the variations of solar radiation in summer require to be modified for the autumn and winter seasons. In autumn the oscillations diminish in amplitude, the two maxima of heat intensity tending continually to approach each other and gradually merge together about noon in winter.—Note on M. Lœwy's formulas for the reduction of the circumpolar stars, by M. Gruey. A process, at once simple and easily remembered, is given for establishing all M. Lœwy's formulas without any sacrifice of accuracy.—Remarks on the appearance of Fabry's comet in April 1886, by M. G. Rayet. The comet, observed at Bordeaux on April 7, 13, and 21, exhibited a very long continuous spectrum from the extreme red to the violet, corresponding with the light of the nucleus and of the three ordinary bands of cometary spectra.—Note on the equilibrium of a fluid mass in rotation, by M. H. Poincaré. Some explanations are offered in connection with M. Matthiessen's note inserted in the *Comptes rendus* for April 12.—On the magnetic rotatory power of the crystalline bodies, by M. Chauvin. Iceland spar and some other birefractive crystals, supposed by Faraday and others to be inactive, are shown to possess the property of magnetic rotation.—Action of alcoholic potassa on urea, sulpho-urea, and some substituted ureas; inverse reaction of the artificial urea prepared by Wöhler's process, by M. Alb. Haller.—Note on two properties of the urethanes of the fatty series, by M. G. Arth.—On the abnormal secretion of nitric substances in yeast and mould, by MM. U. Gayon and E. Dubourg.—Remarks on *Polystigma fulvum*, Tulasne, a new disease of the almond-tree, by M. Maxime Cornu.—Propagation of the luminous sensation to the non-excited zones of the retina, by M. Aug. Charpentier. From his optical experiments the author concludes that, in the phenomenon of successive luminous induction, the nervous action which gives rise to the sensation is really transmitted to the parts of the percipient medium lying near the excited part.—An attempt at a physiological explanation of the phenomenon of complementary colours, by the late M. Trève.—Heliophotography and the magnetic perturbation of March 30, 1886, by M. Ch. V. Zenger.—Observation of an aurora borealis at Rolleville, Seine Inférieure, coincident with the magnetic perturbation of March 30, by the Abbé Maze.

BERLIN

Physical Society, February 19.—Dr. Pernet reported on the part he had taken in the labours of the International Commission which had for their object the comparative determination of the normal metre. After recounting in a brief historical survey the undertakings carried out in Paris at the end of last century by an International Congress, which, after theoretically determining on the kilogramme and the metre as normal units, produced a normal metre and normal kilogramme of platinum, the speaker discussed the events which in 1878 led to a new international agreement, in consequence of which a new normal metre of platinum-iridium of X-form was prepared and compared with the metre of the Archives. A series of national standards was also compared with the normal metre. The speaker described in a searching manner the arrangements of the Bureau in which the comparisons were undertaken, the contrivances for securing the several comparing rooms against outward disturbances, the means adopted for insuring constant temperatures, and the methods employed in the comparisons, as also in the determination of the expansion coefficients of the rods used. Finally he gave a sketch of his own labours, which had for their object the comparison of a series of normal metre rods of different metals with the metre of the Archives, and the determination whether repeated heatings and coolings between 50° and 0° C., whether concussions, and whether time caused any perceptible changes in the lengths of the rods. As the result of these investigations it was found that the compared national standards, together with their divisions, were exact up to one-thousandth of a millimetre; that, with the exception of steel, which, on account of its changes in hardness, readily yielded modifications of volume and length in the rods made of this material, all the metals out of which the standards were made—namely, platinum-iridium, platinum, and brass—furnished material suitable for normal metre rods; and that repeated heatings and concussions induced no changes passing beyond the limits within which observation fails.—Herr C. Baur described experiments he had made with water-jets, which, issuing from a conically-pointed tube in parabolic curves, were acted upon by certain musical tones so that at some distance from the mouth of the tube they showed a rotation, and that the jet, though broken up into drops behind the apex of the parabola, contracted into a continuous jet. The thinner was the jet the higher must be the tone towards which it was sensitive; the thicker the jet the deeper the tone. Herr Baur had instituted further experiments with water-jets, which he caused to fall on plates. Under certain circumstances there thus arose quite pure tones, which continued as long as the jet hit on the plate. The experiments succeeded best with a Weissmann apparatus, when the jet issued under a pressure of 10 cm. water from a lateral opening of 4 mm. in diameter without tube. Thin window-glass plates and metal plates, which, resting on pedestals, had free movement of vibration, were best suited as receiving-plates. The tone was most certain of occurrence when the node lines of the plates were supported. In the jet itself appeared nodes and ventral segments at some distance from the opening; they were most distinct and regular at its middle; away in the direction of the plates they again became indistinct. If the metal plate

and the water acidified beforehand were connected with a galvanic cell and a telephone, then no interruptions of the current could be recognised during the time of the sounding. The contact of the water-jet with the plate must necessarily therefore be continuous. Herr Baur deemed this mode of excitation very well adapted to the purpose of studying the vibrations of plates. In the discussion following this address it was pointed out from various sides that more than twenty years ago Prof. Tyndall and after him Magnus had instituted experiments respecting the action of tones on water-jets, and that Prof. Tyndall had at the time shown his experiments to the Physical Society in Berlin.

Physiological Society, March 12.—Dr. Gad reported on the experiments he had made on the subject of hæmorrhagic dyspnœa which he had referred to in his last address. If by opening a cannula inserted into the aorta a large supply of blood were taken from an animal, dog or rabbit, then dyspnœa at once ensued, and that in the form of increased inspirations, such as showed themselves in all cases of dyspnœa induced by insufficient conduction of oxygen to the respiratory centre. These heightened inspirations proceeded side by side with a conspicuous sinking of the blood-pressure, and were denominated by the speaker "pneumatorectic" respirations. This respiration was distinguished from normal respiration by regular deep inspirations of unchanged frequency, inspirations in which the middle attitude of the thorax removed farther from the expiratory than was the case in normal respiration. The curve of respiration either then passed over into the normal, or convulsions set in, in which case the blood-pressure rose and the respiratory curve grew altogether irregular. After repeated heavy discharges of blood, the pneumatorectic passed into the "syncoptic" respiration, which was characterised by deep inspirations of very infrequent occurrence, during which the attitude of the thorax after expiration approximated ever nearer to that which it held in a dead body, till the last breath, and so the death of the animal, occurred. These two kinds of respiration, the pneumatorectic and the syncoptic, were perfectly regular and typical; the former showed itself immediately after a heavy discharge of blood, the latter before death. Between these two extreme forms there passed a series of others in an inter-current manner. Of these there was first to be mentioned a very frequent superficial respiration, which was inadequate to the necessities of the organism, and had the name "hypokinetic" applied to it. If the animal recovered out of this stage, the hypokinetic passed into the pneumatorectic and the normal respiration, otherwise it was followed by the syncoptic respiration and death. The transitional process from the hypokinetic into the pneumatorectic respiration might be experimentally brought about in a perfectly regular manner by the injection into the venous system of warm physiological solution of common salts. With the increase of the blood-pressure the alteration in the form of respiration at once asserted itself, the respiration becoming sufficient. Even at the stage of syncoptic respiration a transition into the pneumatorectic respiration might occasionally, though not always, be induced by injection of solution of common salt, and in that way the life of the animal be rescued. Another form of respiration following heavy bleeding was that which showed itself in periodical increasings of the amplitudes in respiratory movements. These and diminishings of amplitudes ran parallel to the Traube-Hering periodical oscillations of the curves of blood-pressure, though with displacement of the phases. The periodical oscillations in the amplitude of respiration referred to formed a transition to the Cheyne-Stokes phenomenon. The speaker recounted the explanations of the Cheyne-Stokes respiration, and took sides with the older theory, according to which it was to be conceived as a rhythmus of activity on the part of the central organs having periods of a higher order than had the simple rhythmus of respiration. In conclusion Dr. Gad drew from his physiological experiences a series of practical consequences having respect especially to the suitability of transfusions of common salt after heavy bleedings, particularly at the stage of hypokinetic respiration.—Prof. Zuntz spoke of the nature of the stimulations regulating the normal respiratory movements. The every-day experience that increased muscular activity produced an increased respiratory activity, dyspnœa, had suggested simultaneously to the speaker and to Dr. Geppert the idea of investigating whether the gases of the blood, which were universally assumed to be the sole stimulations of respiration, were adequate to the explanation of this dyspnœa. The experiments respecting which the speaker delivered a report were instituted in common. From the carotid artery of an animal habituated to regular work—a draught dog—were taken quantities of blood which sufficed for the purpose of analysing the gases of the blood. The quantities of blood referred to were taken on one occasion while the dog was in a state of rest, lying comfortably at his ease in his cage; or on another occasion while the dog was at work pulling a loaded car in his usual manner. By an ingenious contrivance, which the speaker described, the discharge of blood was rendered possible without the dog noticing anything of the matter. In a similar manner, by special apparatus, without molesting the dog in any way, they were enabled to measure the quantity of the air breathed in a given time, and to take away small quantities of the exhaled air to be subjected to analysis. The examination of the blood-gases showed that the arterial blood during work contained less carbonic acid and more oxygen than it did during a state of rest. During work the blood contained about 39 per cent. CO_2, and in a state of rest about 40 per cent.; the amount of oxygen, on the other hand, was about 18 per cent. during work, and about 12 per cent. in time of rest. The respiratory activity was, however, during work considerably increased. The quantity of exhaled air during work increased to threefold that exhaled in time of rest, and, corresponding with the increased respiratory activity, the air exhaled during work showed a less increase of CO_2 and a smaller loss of oxygen than in time of rest. The increased respiration during work could not now be caused by the blood-gases, seeing that the contents of the arterial blood in CO_2 were less, and in oxygen considerably more, than during a state of rest. Another stimulus must accordingly act on the central organs of respiration during work. It was possible to imagine that, along with the voluntary excitation of the muscles of the body during work, the respiratory muscles might likewise be stimulated, or that from the corporeal muscles contracting themselves during work a stimulus proceeded reflexively exciting the respiratory centres. The following experiment, however, was against both of these possible assumptions. The spinal marrow of an animal was intersected at the top of the thoracic vertebra, and the paralysed lower extremities tetanised while the anterior part of the body remained at rest. Notwithstanding, however, that all nervous connection between the working muscles and the respiratory centre was cut off, the dyspnœa of work still ensued, and disappeared when the tetanus ceased. From this fact the speaker drew the inference that in the active muscle some product or other was generated which arrived with the blood at the respiratory centre and excited it. The accuracy of this conclusion was further confirmed by the following experiment. The abdominal aorta of the animal with intersected spinal marrow was, during the tetanus of the posterior extremities, strongly compressed through the abdominal integuments. The respiration now continued unchangedly normal, nor did any dyspnœa ensue so long as the compression lasted. Dyspnœa showed itself, however, the moment the compression was removed. Even when the aorta was left free after the tetanus was ended, increased respiration still occurred. The speaker conceived therefore he had conclusively established that a substance, still unknown, forming itself during the muscular activity, proceeded with the blood to the respiratory centre and excited it. He conjectured that, in other active organs as well, such an efficient substance developed itself as respiratory stimulus, a substance which operated along with the gases of the blood even in the normal respiration. In the discussion following this address, Prof. Zuntz mentioned that Dr. Lehmann had made some experiments respecting the effect of acids on the respiratory centre, and had found that the acids excited this centre. This excitation was of course not powerful enough to justify the conclusion that the acid produced during the muscular contraction was the respiratory stimulus in the dyspnœa of work.

Meteorological Society, April 6.—Prof. von Bezold, the newly appointed Director of the Meteorological Institute in Prussia, which is to be reorganised, explained the principles in accordance with which the reorganisation in question would be undertaken. He first gave a short survey of the history of meteorological observations, setting forth how, first, the disciples of Galileo in the Academia del Cimento made use of the newly invented instruments for the observation of temperatures and atmospheric pressure; how, next, as early as the beginning of the eighteenth century, several investigators of nature had arrived at the knowledge that meteorological observations of any

comprehensiveness could be successfully instituted only through the association of a considerable number of observers; and how, more than a hundred years ago, the Societas Palatina in Mannheim had organised an extended network of stations of observation, at which observations were instituted with instruments of the same construction, according to the same plan, and at the same times, and were collected at the central office, and published in a manner which would be deemed exemplary even if issued at the present time. This work was prosecuted till the French Revolution put a termination to it. In Prussia the suggestion of a meteorological institute was made by Alexander von Humboldt, and was crowned with success only in 1847, when, on Humboldt's proposal, Mahlmann was made the first Director of the Meteorological Institute, which was connected with the Statistical Bureau. In 1849 Dove succeeded Mahlmann as Director of the Institute, and held the post till his death in 1879. Meanwhile, however, the necessity of a complete transformation of the Meteorological Institute came to be recognised. Formerly, simple average values for the different stations were calculated, and for these no special stress was laid on the single observation, in consideration that mistakes balanced one another. Now, however, when it was a question of preparing synoptic maps and of obtaining exact maps of the meteorological conditions prevailing at a determinate time over a large area, the value attached to the single observation was a much higher one, and it was of the greatest importance that all the data should be as free from error as possible. It would accordingly be the first task of the Institute to provide all stations of the second and third order with good instruments, carefully to see they are maintained in good order, and to collect the materials of observation. The network of stations of observation would have to be completed and equally distributed, and there were about 200 stations of the second and third order, besides some thousands of subordinate stations, in contemplation. The subordinate stations should be equipped with rain-gauges, and make observations on precipitation, thunderstorms, and such like. A second problem of the Institute was the exact determination of the course of the meteorological elements for the day, the month, and the year, by uninterruptedly continuous observations not only of the climatic factors—temperature, atmospheric pressure, moisture, &c.—but also of the phenomena of the earth's magnetism and electricity. This work would be done by the Observatory, which was completely separated from the Meteorological Institute. The Observatory, under a special direction, was transferred to Potsdam to the Astro-physical Observatory. Two similar Observatories of the first rank, one in Breslau, perhaps, and one in Bonn—at all events, in University towns wide apart from each other—were in contemplation. While the Observatory prosecuted its observations in the quiet of Potsdam, the Meteorological Institute should have its seat in the midst of Berlin, in the edifice of what was formerly the Building Academy, and continue in connection with the lively intercourse of the capital. Irrespective of the service for weather warnings to be introduced perhaps at a later date, which would require to be in proximity to the head telegraph office, the central position should be readily accessible to the different observers who came from the provinces to the capital. The Institute, moreover, should be easily available for all students of science and experts who were in need of meteorological data: such, for example, as agriculturists, physicians, persons engaged in hydraulic labours, &c. The Meteorological Institute should, finally, have as its main function that of being a teaching institute for the scientific training of meteorologists. Its function in this respect should not be merely confined to lectures at the University, but should especially consist of practical work done, under the guidance of assistants, by students and young observers in the Meteorological Institute, similar to what is carried on in chemical, physical, and other laboratories. With this programme in hand, the new Director hoped very soon to bring the Meteorological Institute to the degree of efficiency attained by similar institutes in neighbouring countries, and particularly by the teaching thus imparted to cultivate a new field fruitful of good results for science.—Dr. Weinstein, with reference to his paper recently read to the Society, made some further communications respecting disturbances of the earth's currents which had occurred on January 9 and March 30. On March 30 the disturbances were so great that in the course of the forenoon telegraphic communication in Germany was stopped. Even with currents of 60 Daniells no signs could be forwarded by the telegraph wires. The magnetic elements in Wilhelmshaven showed great simultaneous disturbances, and from the direction of these magnetic disturbances it was inferred that the disturbances of the earth's electricity were the primary, the oscillations of the earth's magnetism the secondary.—In connection with these observations of Dr. Weinstein, Prof. Spörer stated that from March 26 to April 4 a very remarkable and numerous group of spots had been observed on the sun. On March 30 Dr. Less had observed squalls, accompanied with remarkable oscillations of temperature and of atmospheric pressure, and Dr. Assmann read several reports on North Light phenomena which had been perceived on March 30 in Eldena, Greiffenhagen, Magdeburg, and Nordhausen.—Dr. Weinstein further communicated that Prof. Förster had entered into an arrangement for having reports of disturbances observed in the earth's current at once forwarded to the Astronomical Observatory that the state of the sun might be simultaneously examined.

BOOKS AND PAMPHLETS RECEIVED

"Journal of the Statistical Society," March (Stanford).—"Earthquakes and other Earth Movements," by John Milne (K. Paul).—"Transactions of the Institution of Engineers and Shipbuilders in Scotland," 1885–86 (Glasgow).—"The Forest Flora of South Australia," part 7, by J. E. Brown (Spiller, Adelaide).—"Jahrbuch der k.k Geologischen Reichsanstalt," Band xxxvi. Heft 1 (Hölder, Wien).—"Archives Italiennes de Biologie," tome vii. fasc. 11 (Loescher).—"Sea-Weeds, Shells, and Fossils," by Peter Gray and B. B. Woodward (Sonnenschein).—"A Treatise on Nautical Astronomy," by J. Merrifield (S. Low).—"Birds of Cumberland," by H. A. Macpherson and W. Duckworth (Thurnam, Carlisle).—"Handbuch der Palæontologie," Abtheil. 1. Band 11, Leif. 5, "Myriopoda, Arachnoidea, und Insecta," by S. H. Scudder (Druck, München).—"Handbuch der Palæontologie," Ahtheil. 11. "Palæophytologie." Leif. 4. "Coniferæ und Monocotylæ," by Dr. A. Schenk (Druck, München).—"Letters and Journal of W. Stanley Jevons" (Macmillan).—"Solid Geometry," 3rd edition, by P. Frost (Macmillan).—"Recherches sur l'Instabilité des Continents et du Niveau des Mers," by J. Girard (Leroux, Paris).—"Johann Kepler," by C. Anschutz (Prag).—"The Management of Athletics in Public Schools," by G. Fletcher (Lewis).

CONTENTS

THURSDAY, MAY 13, 1886

THE CHEMISTRY OF THE COAL-TAR COLOURS

The Chemistry of the Coal-Tar Colours. Translated from the German of Dr. R. Benedikt, and Edited, with Additions, by E. Knecht, Ph.D. (London: George Bell and Sons, 1886.)

THIS is an excellent little practical manual dealing with a subject of great scientific and industrial importance—a subject the scientific side of which has been somewhat neglected in this country, to the inevitable detriment of the industrial side. The decline of this industry in England is a tempting subject to expatiate on; but the moral has of late been pointed with such laudable iteration that we refrain from pointing it afresh. The state of affairs which prevails with regard to the literature of the subject is expressed in the opening words of the editor's preface:—

"Although England may be called the birthplace of the coal-tar-colour industry, it is a remarkable fact that the English literature on the subject is very scanty, and that which does exist is now almost obsolete owing to the rapid strides which have been made during the last ten years in the manufacture of the coal-tar colours."

There is no doubt about the want, and we think that this little work supplies it to the extent aimed at. Both author and editor are specially qualified for their task by experience in teaching the technology of the subject.

The work contains excellent introductory chapters on the optical properties of colouring matters, the methods of testing colouring matters—both spectroscopically and with regard to their tinctorial power—on the relation of the various fibres to the colouring matters, and kindred general questions, of importance both to the colour chemist and to the dyer. "Rule-of-thumb" is everywhere excluded; reasons are fully and clearly given.

The greater part of the work is necessarily devoted to the chemistry proper of the coal-tar colours—the chemical processes by which the various colouring matters are obtained and the reactions by means of which a knowledge of their chemical constitution is arrived at. Constitutional formulæ naturally play a very important part.

Our modern dynamical chemists—some of whom, by the way, appear to be censors first and investigators afterwards—are never tired of crying out for the abandonment of these constitutional formulæ on the ground that they afford only statical, not dynamical, representations of chemical phenomena. Happily, those who have built up the German coal-tar-colour industry of the last fifteen years on the basis of the benzene theory have never shared this opinion; nor is it shared by our authors, who in their little treatise faithfully reflect the methods and results of this great scientific and industrial development. Doubtless, colour chemists would prefer a dynamical formula—one which should indicate, for example, the most suitable temperature at which to perform a potash fusion, or a nitration, with a few hints thrown in as to time of heating, concentration, and so forth—and doubtless the dynamical chemists will in time supply this want; but meanwhile the colour chemist feels, taught by experience, that his humble and inexact calculus of chemical operations, the constitutional formula, is vastly better than anything that has been offered in its stead. But as yet the dynamical critic does not appear to have anything to offer in its stead: like certain *dynamitical* critics he is satisfied with destruction, and his attitude towards constitutional formulæ is not unlike that of the dynamitical critic towards Constitutions—British and other.

There is little which calls for criticism in the chemical portion of the work: the classification is good, and the results of the elaborate investigations of which almost every colouring matter of any importance has of late years been made the subject are given briefly but in a way calculated to make clear to the beginner the significance of such work. We could have wished, however, that Dr. Knecht in his editorial capacity had thought good to give some account of the researches of O. Fischer on flavaniline and chrysaniline, and of Bernthsen on methylene-blue. The problem of the constitution of these compounds has been solved in a very instructive and conclusive fashion—much more conclusively than in the case of some of the colouring matters of which the constitution is discussed in the present work.

In the introduction reference is made to the popular prejudice which exists against the so-called "aniline dyes"—the collective name by which coal-tar colours are known among non-chemists. There is an impression that the tints are crude and glaring, and that the colours lack fastness. Certainly there are coal-tar colours which sin in all these respects. But there is a survival of the fittest here as elsewhere: the vulgar shades and fugitive colours are being weeded out and replaced by better. The accusations come most frequently from persons of an æsthetic turn, and it is perhaps too much to expect that the strenuous æsthete, living laborious days in the endeavour to improve his own taste and that of his neighbours, should be aware that the beautiful and permanent Turkey-red, which he so justly admires, is now a coal-tar colour, and that even indigo may be made from coal-tar. As regards "fastness" of colours, the ideas of the general public on the subject may perhaps be gauged by a speech which we remember reading, made some years ago by a Member of Parliament in distributing the prizes at a technical school. Seeking to inculcate the duty of thoroughness in work, and desirous at the same time to employ only such illustrations as would at once come home to every technologist, he said:—"But it would not be thorough work, for example, to daub a wall with untempered mortar, or *to dye with fast colours*." Probably a life divided between politics and sport had not permitted him to realise that the fastness of colours is distinct from that of race-horses—or of youth!

Where there is so much to praise we regret to have to record a defect, but we think that hardly adequate care has been bestowed upon the proof-reading. The misprints are unnecessarily numerous, and must sometimes be very puzzling to a beginner, especially where, as is occasionally the case, they affect complicated formulæ. A list of errata is given, which, however, needs extension. Whether, for example, the chemistry of the average student of technology will be equal to the task of informing him that not sodium bisulph*ate*

but sodium bisulph*ite* is employed in the preparation of soluble alizarin-blue, or that the three formulæ given on p. 70 in a preliminary account of the products from tar, and described as those of "the three isomeric dinitrobenzenes," are in reality those of the three mononitrotoluenes—errata not corrected in the list—is open to doubt.

In conclusion, we cordially recommend the book. We trust that it will not only be made use of by students of technology as a useful introduction to the larger treatises in French and German, but that the ordinary student of organic chemistry will take the opportunity of making a closer acquaintance with a special branch of his subject, as fascinating from a scientific point of view as it is fertile in practical results. F. R. JAPP

JAPANESE HOMES

Japanese Homes and their Surroundings. By Edward S. Morse, Director of the Peabody Academy of Science. (London: Sampson Low, 1886).

ALTHOUGH Prof. Morse's connection with Japan has been comparatively short and interrupted, few men have done so much for scientific progress in that country. About ten years ago he first visited Japan in order to study certain forms of ocean life on its coasts, and, fortunately, was induced to accept the Chair of Zoology in the University of Tokio. While holding this office he did much to arouse an interest in the minds of his students for biological research, and he established a Biological Society, which is, we believe, still at work. By his discovery and thorough investigation of the shell-mounds at Omori, near Tokio, he stimulated prehistoric studies. His monograph on these mounds—although perhaps his theory as to the builders may not, on more extended examination, have proved tenable—was followed by a number of publications on the Japanese Stone Age, cave-dwellers, and the like; and in many less generally known directions his influence on the advance of science in Japan has been a beneficial and stimulating one. His first visit to Japan has been followed by two others, during which he visited all parts of the country, as well as other regions of Eastern Asia, and has collected material on a variety of matters. The present volume is a monograph on the house in Japan;—the different types of houses, their mode of construction, the uses of each part, the varieties in each from the roof to the foundation, the types and uses of household utensils, &c. The illustrations, which are beautiful, are also very numerous, being, on the average, about one to a page. Without them it would, indeed, be difficult for readers who are not well acquainted with Japanese houses to follow the descriptions. Many of these details Prof. Morse thinks it may soon be difficult, if not impossible, to obtain, and therefore like an old Japanese to whom he refers, and who "held it a solemn duty to learn any art or accomplishment that might be going out of the world, and then to describe it so fully that it might be preserved to posterity," he now describes and copies them for the benefit of future generations who may not have the opportunity of seeing these evidences of Japanese skill and sense of beauty. We do not apprehend that the Japanese will ever change so far as to substitute the jerry-builder for their own carpenters, and we do not think that their style of architecture will ever greatly alter, for the simple reason that they have now what, on the whole, is the fittest. Nevertheless we cannot but be grateful to Prof. Morse for making the Japanese house, inside and out, so familiar to English readers. His work is so clear and detailed that we see no reason why any one who feels so disposed should not be able to erect for himself a home in the Japanese style in England.

In the eighth chapter indications from the most ancient works in Japanese literature are collected together in order to catch a glimpse of what the Japanese house of a thousand years ago was like. It would be useless without a plan of the modern house before us, to refer to these beyond quoting Prof. Morse's conclusion that they are significant indications of the marked southern affinities of the Japanese, and he thinks that, from all we can gather relating to the ancient house of the Japanese, it would seem that certain important resemblances must be sought for in Annam, Cochin China, and particularly in the Malay peninsula—but not amongst the Ainos. This is another nail in the coffin of the theory of an ethnic relationship of the latter with the Japanese. On the whole, Prof. Morse's theory of the history of house development in Japan is a slow but steady progress from the rude hut of the past to the curious and artistic house of to-day—a house as thoroughly a product of Japan as is that of the Chinese, Corean, or Malay a product of these peoples, and differing from all quite as much as they differ from one another. It has just those features incorporated into it that might be expected from its physical proximity to, and historical relations with, China and Corea. The last chapter deals with the "neighbouring house"—that is, Corean, Chinese, Aino, and Loochooan houses. In this chapter the writer has fallen into a curious error in describing Hachijô Island as one of the Bonins. There is no more connection between the two than there is between Iceland and the Isle of Wight. Hachijô has from the earliest times been Japanese; it was at one time a place of exile for political offenders. The Bonins never belonged to Japan until within the last few years; as the name (*Bu* or *Mu Nin*, without people) implied, they were uninhabited, except by a few waifs and strays thrown up by the sea—Caroline Islanders, deserters from whalers and ships of war. The account of the visit to Hachijô, from which Prof. Morse quotes, was published some years ago in the *Transactions* of the Asiatic Society of Japan, and is of exceptional interest, for in this island may still be observed ancient Japanese customs which have long fallen into desuetude on the mainland. Thus the peculiar lustration ceremonies, the special parturition houses, &c., now found in Hachijô, are mentioned in ancient Japanese works as common to all Japanese. The difficulty of access to the island from the adjacent mainland on account of dangerous currents would explain the presence of this little oasis of antiquity. There is this excuse, however, for Prof. Morse's confusion of the Bonin Islands with Hachijô, that the expedition set out for the Bonins, but the writers about Hachijô went no farther than that island, and there, while awaiting the return of the steamer, collected the material for the paper in question.

ACOUSTICS, LIGHT, AND HEAT

Acoustics, Light, and Heat. By William Lees, M.A., Lecturer on Natural Philosophy, the Heriot Watt College, and Lecturer on Mathematics and Experimental Physics, Free Church Training College, Edinburgh. New and Enlarged Edition. (London and Glasgow: Wm. Collins, Sons, and Co.)

THIS is one out of many of the text-books which have been called into existence by the "May" Examinations of the Science and Art Department. Being written especially to meet the requirements of the student who wishes to pass these examinations, it is only brought up to the standard given in the directory of the Department, and may therefore for this purpose be useful. The fact that a new and enlarged edition is now appearing is certainly evidence that this is the case. To make it more serviceable, the questions of all the May Examinations in Subject VIII. from 1872 to 1885 are given.

Though the simple and numerous diagrams and the generally clear nature of the text give it a certain value as a text-book, it is by no means so free from faults and ambiguities as might be expected in a new edition.

It may be well to refer especially to a few places where alterations suggest themselves.

Figs. 19 and 20 show the contrast between a musical sound and a noise. Though it is explained that "noises are due to irregular vibration or a confused mixture of musical sounds which produce aërial waves of great complexity and wanting in periodicity," no explanation is offered of a peculiarity in the "curve of a noise" (Fig. 20), which in three places is actually made to slope backwards.

Some of the figures in optics are rather wanting in precision. Thus in Fig. 73, which shows a real image formed by a concave mirror, a pair of slightly diverging rays are made to cross between the mirror and the principal focus. Again, Fig. 77 shows a caustic on the surface of milk in a glass with its cusp reaching close to the centre. Fig. 103 shows the action of a refracting plate on a beam of light by the turning and approximation of successive wave-fronts. Those two wave-fronts which obliquely cut the surface are shown straight and partly swung round, as if they were rigid lines meeting with resistance at one end. It would surely have been better to have bent the line at the point of intersection, leaving all the wave-fronts and parts of a front outside the medium parallel to one another, and also all inside parallel to one another, but it is possible that wave-fronts, strictly speaking, are not intended.

The explanation of so important a thing as the achromatic lens can hardly be considered satisfactory. Owing to its brevity it is possible to give this in full. "This defect in a lens [the defect of chromatic aberration] is obviated by the combination of a double convex lens of *crown* glass, with a convexo-concave of *flint* glass (Fig. 122). The effect of the second lens is to re-blend the coloured rays which the first has produced, and at the same time such an amount of refraction is preserved as to bring the light to a focus." As nowhere is it directly pointed out that for the same degree of refraction flint glass produces more dispersion than crown, it is not difficult to imagine that a student might fail to form any very clear idea of the principle of the achromatic lens, nor is he likely to be materially helped by the figure (122), which certainly does not represent either the section or any other view of any achromatic lens that was ever made. If it were not for the section lines it would be a good perspective drawing of a short cylinder; the ellipse which appears to be the end of such a cylinder is really meant to show the crown lens in section, and the figure of uniform thickness by its side, as thick everywhere as the ellipse is in the middle, which seems to be the side of the cylinder, is meant for the section of the flint lens. Simplicity in a diagram is a thing to be desired, but there is more than simplicity here.

Very little is said about spectrum analysis; and its application to the measurement of the motion of the heavenly bodies in the line of sight is not even mentioned.

The general weakness of the optical part is to a certain extent compensated for by the chapters on polarisation, which have much to recommend them. There is here, however, a paragraph which requires explanation. "Now it is found that whatever quantity of polarised light there is for any incidence *other than the polarising angle* in the reflected beam, there is always the *same* quantity in the refracted beam. At the polarising angle, however, the refracted beam exhibits traces of polarisation." What is meant by this distinction is by no means clear.

Heat is more precisely and clearly treated than light, but here the general excellence is marred by an example to illustrate expansion in which the working out of the result shows that the obvious meaning of the question is not intended. What any one would understand by the words, "Find the length of a rod of brass which would expand equally with a rod of steel 3 feet long under a change of temperature of 10° C.," is evidently —Find what length of brass will increase in length by the same amount that a 3-foot rod of steel does for a change of 10° C. But what is found in the working of the answer is the length of a piece of brass which will expand so as to be *as long as* a piece of steel 3 feet long when each is *raised* 10° C.

Sufficient has been said to show that this book is not as clear and accurate in either the text or the figures as might be expected in a new edition.

OUR BOOK SHELF

Cholera Curable. By John Chapman, M.D. (London: Churchill, 1885.)

DR. CHAPMAN has had the opportunity of testing, in the Hôpital de la Charité in Paris, his method of the so-called neuro-dynamic treatment in Asiatic cholera, and his demonstration of the success of this treatment constitutes, we take it, the cardinal motive for the production of this book, although a good many other, mostly theoretical, considerations are brought into the discussion.

The symptoms of Asiatic cholera are explained by a number of assumptions on the action of the spinal cord and the sympathetic nervous system, but as to which we look in vain for experimental proof. The theories concerning the etiology and causation of cholera are fully treated, and then Dr. Chapman promises to furnish us with a complete solution of these problems in a discovery made by him as to the cause of cholera. When, however, we come to analyse what he really has discovered,

it turns out that he himself is moving in a circle of fallacies. While denying the specific nature of the cholera virus, he explains this latter by the symptoms of the disease. Assuming, for the sake of argument, with Dr. Chapman, the particular disturbances of function in the cord and the sympathetic nerves to which the symptoms of cholera are due, how does this bring us nearer to the knowledge of what causes these particular disturbances? By saying, or even by showing, that such and such a disturbance in the function of the cord and sympathetic causes such and such a symptom of disease, we are not one iota nearer the answer to the question, Why did such and such a disturbance take place? what has caused it? The answer to this one wants to know, but this is not supplied by Dr. Chapman. It is quite true that a great many conditions are required to favour the outbreak and spread of cholera, *e.g.* conditions of temperature, water, atmospheric disturbances, soil, &c., &c., but all these conditions may be present without producing cholera, or typhoid fever, or any other similar disease. Why? Because *the actual cause* of the disease is absent. These two things, viz. secondary conditions favouring the outbreak and spread, and *the actual cause*, must be kept separate; but evidently Dr. Chapman has not arrived at this as yet.

The chapters VIII. to XV. describe the various methods of treatment of the disease, and they form the most important part of the book. E. KLEIN

Seaweeds, Shells, and Fossils. By Peter Gray, A.B.S., and B. B. Woodward. (London: Swan Sonnenschein and Co

THE object of this book is to give to the young English collector a general knowledge how to set about collecting the more common seaweeds, shells, or fossils.

In the first case the subject is dealt with generally, classifying the different seaweeds and stating where each is most likely to be found, and, when found, the best way to press them and get them ready for the cabinet, the most economical way of making or obtaining which is given.

Secondly, shells are dealt with, descriptions and diagrams being added where necessary, together with a table of the more important genera, showing the approximate number of species belonging to each genus, and their distribution.

Thirdly, and lastly, different localities are pointed out where fossils are best found, and the best mode of arranging them in the cabinet. A table of the principal fossiliferous strata arranged in chronological order, with notes on the different formations mentioned in the table, and also of the principal divisions of the animal kingdom, are added to show the order in which the fossils should be arranged. S.

The Modernised "Templeton"; or, "The Practical Mechanic's Companion." By William Templeton. Revised and Modernised by Walter S. Hutton, C.E. (London: Crosby Lockwood and Co., 1886.)

TEMPLETON'S "Mechanic's Workshop Companion" is a work familiar to most mechanics and draughtsmen, having been considered for the last quarter of a century a useful book of reference by all connected with the management of engineering workshops and kindred trades. Books of this description require revising very often, and considering the enormous development of the mechanical sciences during the last few years no one will wonder on hearing that even "Templeton" has to be modernised to keep pace with the times.

The reviser tells us that he has endeavoured to follow as far as possible the lines of the original work, at the same time bringing all the information up to date. Much new matter has been added, giving information on air, gas, water, and steam; methods of testing steam-engines and boilers; turbines and other water motors; the strength and weights of material; and miscellaneous information too numerous to give in detail.

The work has for a frontispiece an illustration of the fine compound locomotive "The Marchioness of Stafford," designed by Mr. F. W. Webb, the able locomotive superintendent of the London and North-Western Railway. On seeing this we are at once led to imagine that at last we have found a book giving recent data on locomotive engineering, and likely to fill a want seriously felt by those who study that most interesting branch of mechanical engineering. We are told to "see p. 360," to which we turn hoping to find a section devoted to locomotive work, having Mr. Webb's fine engine as an example of the latest advance. We find a third of a page giving the bare dimensions of the compound. Even the index makes matters no better, for the book contains no locomotive data whatever! Considering the thousands of mechanics engaged in this class of work, this is a great pity, and should be remedied in a future edition.

An excellent abstract is given of the results of experiments on riveted joints, with special reference to practical work, by Prof. Alexander B. W. Kennedy. This is most interesting and useful, and will well repay careful study by those connected with the manufacture of soft steel boilers and bridges.

The book contains all the usual tables, embracing every subject likely to be required by the intelligent mechanic or draughtsman, including extensive practical rules and data Instruction is also given in the rudiments of arithmetic, algebra, and trigonometry. N. J. L.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Foreign Fishery Boards

IN reference to recent statements in NATURE on this subject, it will interest your readers to know that in Italy the Government has constituted a Fishery Board, which, my friend Prof. Giglioli, of Florence, tells me, is actively employed in advising the executive and in inquiring into the grievances and difficulties of fishermen, and the suggestions for improvement of fisheries; it has also recently, at the expense of the Government, taken practical measures in the stocking of lakes with fish, and in the cultivation of sea-fish. This Commission is a branch of the "Ministry of Agriculture, Industry, and Commerce," which corresponds to our Board of Trade. *The members of the Fishery Commission, with the exception of the first three named below, are exclusively scientific men.* They are as follows:—The Permanent Under-Secretary of State for Agriculture; the Permanent Under-Secretary of State for Commerce; a distinguished lawyer; M. Minni, of Venice; Dr. Kenier, of Chioggia (representing the fishermen of this island); M. Friedlander, of Comacchio (specially acquainted with the peculiar fish-culture of this district); Prof. Giglioli, Florence: Prof. Targioni-Tozzetti, Florence; Prof. Costa, Naples; Prof. de Vincentis, Taranto; Prof. Canestrini, Padua; Prof. Pavesi, Pavia; Prof. Issel, Genoa.

The Commission meets from time to time in Rome. The questions submitted to it are brought forward and referred separately to one or two members, who are requested to draw up a report on the particular subject thus referred. The report may take several months, and involve experiment or research, or it may be a simple matter. The report when presented is discussed by the whole Commission. The conclusions and recommendations which it embodies are modified by vote of the majority, and it is obvious from the constitution of the Board that the scientific experts have the voting strength.

The members of the Board or Commission are paid travelling

expenses and 25 francs a day each during its sessions in Rome, or other fixed place of meeting.

The constitution of this Board is admirable, since it secures a fair representation of the leading scientific men of Italy. The Italian Government does not, it seems, refer such questions exclusively to one individual, but endeavours to obtain a consensus of the scientific opinion of the country.

Hôtel de la Ville, Florence E. RAY LANKESTER

Fabry's Comet and Barnard's Comet

NOT having seen any mention of the rapid apparent growth of the tail of Fabry's comet, probably some of your readers are not aware to how great a length it extended. On April 26 occurred the first fine night after a very unusual series of overcast ones, and about 14h. G.M.T. I was surprised to see the tail reaching up to, or at least to within 1° of, δ Cassiopeiæ, a distance of 38° from the place given in the ephemeris for the nucleus, which was far below the horizon; and the tail would doubtless have been visible to a greater distance but for the brightness of the Milky Way. The following night, about 10h., it reached at least up to the Cluster in Perseus, a distance also of 38° from the predicted position of the nucleus; it was very narrow both nights. The next night, which was pretty fine, I failed to find any trace of the tail.

The principal tail of Barnard's comet is also very narrow: on May 1 its length was 4½°, as seen with a pair of field-glasses. With the telescope this comet had also a faint tail *nf*, about 16′ long, making an angle of 65° or 70° with the other.

Sunderland, May 7 T. W. BACKHOUSE

"Pumice on the Cornish Coast"

STEAMER-CINDERS, similar to those referred to by Mr. Whitaker in NATURE for April 29 (p. 604), occur frequently on the Falmouth beaches; but as there seemed to me little probability of their being mistaken for pumice, I did not refer to the matter in my communication to your columns (April 15, p. 559).

Mr. Murray tells me that the pumice I found is felspathic, and that from its form and diminished buoyancy it had evidently been a long time in the water. The fragment was sent by him to Mr. Whitaker, who at once recognised its true character and its distinction from the steamer-cinders observed by him on the Suffolk coast, one of which he sent to Mr. Murray to satisfy him as to their very evident source. H. B. GUPPY

95, Albert Street, Regent's Park, N.W., May 8

THE VELOCITY OF LIGHT

I.

[A reinvestigation of this important constant has recently been published by Prof. Newcomb. Before we state his methods and results we think it well to reproduce the following admirable historical notice with which his monograph commences.—ED.]

WHEN it became clearly understood that vision was not an immediate perception of objects by the eye, but was produced by the passage of an entity called light from the object to the eye, the question of the time which might possibly be required for this passage became one of interest to physical investigators. The first proposal for an experimental investigation of this question is due to Galileo.[1] He suggested that two observers, each holding a lantern, should be stationed at a distance apart, in sight of each other. Each should be supplied with a screen, by which he could, in a moment, cover or uncover his lantern. One observer should then uncover his lantern and the other uncover the other the moment he perceived the light from the first lantern. The interval which elapsed after the first uncovered his light, until he perceived the light of the second, would be the interval required for the light to go and come, plus the time required for the second observer to perceive the light and make the required movement. This experiment was tried by the Florentine Academy, and of course resulted in a conclusion that the time required was insensible, since we now know that it was far below any interval that could have been detected by so rude a method.

It is, however, interesting to notice that, rude though this experiment was, the principle on which it was based is the same which underlies one of the most celebrated methods used in recent times for the attainment of the same object. Two very simple improvements which we might have imagined the Academicians to make in their experiments are these:—

Firstly, to dispense with the second observer, and in his place to erect a mirror, in which the first observer could see the image of his own lantern by reflection. The time required for the second observer to perceive the light and uncover his lantern would then have been eliminated from the problem. The interval sought would have been that between the moment at which the observer uncovered his lamp and the moment at which he perceived the reflection.

Secondly, to use the same screen with which he uncovered his own lamp, to cut off the returning ray from the distant mirror, and thus obviate the necessity of an uncertain estimate of the interval between his muscular effort in removing the screen and his perception of the return flash of light. If the image was perceived before he could cover his own eye with the screen removed from the lamp, it would show that the interval of passage was less than the time required to make a motion with the screen. This interval might have been reduced almost indefinitely by having both lines of sight as near together as possible.

Had these improvements been made, the Academicians would have had, in principle, Fizeau's method of measuring the velocity of light by the toothed wheel, a tooth being represented by the screens. To realise the principle more fully, the two lines of sight should have been rendered absolutely coincident by reflection through a telescope. It does not, however, appear that any effort to put the question to a severer test was made until the subject was approached from a different point of view. It was probably considered that the passage was absolutely instantaneous, or, at least, that the velocity was above all powers of measurement.

The subject was next approached from the astronomical side. In 1676 Roemer made his celebrated communication to the French Academy, claiming that observation of the eclipses of the first satellite of Jupiter did really prove that light required time to pass through the celestial spaces.[1] He found 11m. to be the time required for light to pass over a distance equal to the radius of the earth's orbit. Dominique Cassini, while admitting that the hypothesis of Roemer explained the observed inequality, contested its right to reception as an established theory, on the ground that the observed inequality might be a real one in the motion of the satellite itself.[2]

Continued observation showed that the time assigned by Roemer for the passage of light between the earth and sun, or "the light equation" as it is briefly called, was somewhat too great. In 1809 it was fixed by Delambre at 493·2s., from an immense number of observations of eclipses of Jupiter's satellites during the previous 150 years. This number has been received as a definitive result with a degree of confidence not at all warranted. In 1875, Glasenapp, then of Pulkowa, from a discussion of all available eclipses of Jupiter's first satellite between 1848 and 1870, showed that results between 496s. and 501s. could be obtained from different classes of these observations by different hypotheses.[3]

[1] Poggendorff, *Geschichte der Physik*, p. 402, where reference is made to he *Saggi* of the Florentine Academy.

[1] *Paris Memoirs*, tome i. p. 212, and tome x. p. 575.

[2] *Ibid.* tome viii. p. 47. Poggendorff (*Geschichte der Physik*, p. 656) quotes Maraldi as also contesting Roemer's explanation on the ground that a similar inequality should be found depending on the position of Jupiter in his orbit. The ground here taken was quite correct, the only fallacy being the assumption that such an inequality did not exist.

[3] This paper of Glasenapp's was published only in the Russian language as an inaugural dissertation, and in consequence has never become generally known

As not a trace of Delambre's investigation remains in print, and probably not in manuscript, it is impossible to subject it to any discussion.[1]

The discovery of aberration by Bradley afforded an independent and yet more accurate method of determining the light equation. We call to mind that the latter constant, and that of aberration, are not to be regarded as independent of each other, but only as two entirely distinct expressions of the same result. The constant of aberration gives a relation between the velocity of light and the velocity of the earth in its orbit from which, by a very simple calculation, the time required for light to pass from the sun to the earth may be deduced.

It is remarkable that the early determinations of the constant of aberration agreed with Delambre's determination of the light equation, although we now know they were both in error by an amount far exceeding what was at the time, supposed probable. Struve's value, 20''·445, determined in 1845 from observations with the prime vertical transit of Pulkowa, has been the standard up to the present time. The recent determinations by Nyrén being founded on a much longer series of observations than those made by Struve, and including determinations with several instruments, must be regarded as a standard at present. His result is :—[2]

Definitive value of the constant of aberration = 20''·492 ± 0''·006.

At the time Struve's result was published there was an apparent difference of 1 per cent. between its value and that of the light equation determined by Delambre. The question then naturally arose whether the light equation, deduced on the hypothesis that the tangent of the angle of the constant of aberration was the ratio of the velocity of the earth in its orbit to the velocity of light, might not need correction or modification. This question cannot yet be considered as definitely settled, since the modifications or corrections might arise from a variety of causes. One of these causes is connected with a very delicate question in the theory of the luminiferous medium; a question which can be most clearly understood when placed in the following form :—It is a result of optical principles that a ray falling perpendicularly upon the bounding surface of a refracting medium retains its direction unaltered. Now, if this surface is carried along by the motion of the earth, and the light comes from a star, and it is desired that this surface shall be so directed that there shall be no refraction, must it be placed perpendicular to the *true* direction of the star as freed from aberration, or to its *apparent* direction as affected by aberration? The difference of the two directions may exceed 20'', and since the index of refraction of glass exceeds 1·5, there will be a difference of more than 10'' in the direction of the refracted ray, according as we adopt one or the other hypothesis. Assuming that the standard direction would be perpendicular to the true or absolute direction of the star, it is easily shown that the constant of aberration determined in the usual way would be too large by a quantity depending on the ratio of the thickness of the objective to the focal length of the telescope. In an ordinary telescope the difference would be nearly one-hundredth of the total value of the aberration, and would, therefore, closely correspond to the discrepancy between Delambre's result from the satellites of Jupiter and the modern determinations of the constant of aberration. The question of this particular cause was set at rest by Airy's experiments with a telescope filled with water, which showed that the result was independent of the thickness of the objective, and, therefore, that the apparent direction of the star was that on which refraction depended.

If, in accordance with the undulatory theory of light, we suppose the hypothetical entity called "the luminiferous medium" to be a substance, each part of which has its own definite and fixed location in space, then we must conceive that another unknown quantity may enter into the problem, namely, the motion of the heavenly bodies through this medium. We have relative motions in the solar system, exceeding 50 kilometres per second, and possibly greater relative motions among the stars. Now it is clear that the heavenly bodies cannot all be at rest relative to the medium, but must move through it with velocities at least of the order of 50 kilometres per second, and possibly greater without limit, since it is conceivable that the whole visible universe might be moving in a common direction relative to the medium.

It is easily seen that if we suppose the velocity of the earth, through the medium, to have a small ratio, a, to the velocity of light, then the observed constant of aberration may be altered by an amount found by multiplying its value by a quantity of the order of magnitude of a. This alteration would be entirely insensible if the earth does not move through the medium with any greater velocity than it does around the sun, since the value would then be only $\frac{1}{10000}$. It is remarkable that so far as yet investigated every optical effect arising from such a motion, which could be measured on the surface of the earth, is of the order of magnitude of the square of a. Thus, no phenomenon has yet been discovered which can be traced to the motion in question.

Assuming that there is no general motion of the solar system through the ether of a higher order of magnitude than that of the relative motions of the fixed stars to each other, and that the ordinary theory of aberration is correct, there will be three constants between which a relation exists, such that when any two are found the third can be determined. These constants are :—

1. The distance of the sun in terrestrial units of measure;

2. The velocity of light in units of the same measure; and

3. The constant of aberration, or, which is supposed to be equivalent, the light equation.

Until our own time the first and third constants were used to determine the second. From the fact that light required about 500 seconds to traverse the distance from the sun to the earth, and that the distance of the sun was, as supposed, 95,000,000 of miles, it was concluded that light moved 190,000 miles per second. The hopelessness of measuring such a velocity by any means at the command of physicists was such that we find no serious attempt in this direction between the date of the futile effort of the Florentine Academy, and that of the researches of Wheatstone, Arago, Fizeau, and Foucault nearly two centuries later. One of the most curious features presented by the history of the subject is that two entirely distinct methods, resting on different principles, were investigated and put into operation almost simultaneously. The revolving mirror of Wheatstone, and its application to determine the duration of the electric spark and the velocity of electricity, come first in the order of time. But, before this ingenious instrument had been applied to the actual measurement of the velocity of light, Fizeau had invented his toothed wheel, by which the same object was attained

Fizeau's paper on the subject was presented to the Academy of Sciences on July 23, 1849.[1] We have already shown that his method and that of Galileo rest fundamentally upon the same principle. The arrangement of his apparatus was substantially as follows :—

A telescope was fixed upon a house at Surêsne pointing to the hill Montmartre. On this hill was a second fixed telescope looking directly into the first, the distance between them being about 8633 metres. In the focus of this second telescope was fixed a small reflector, so that

[1] The author could find no remains of this investigation among Delambre's papers at the Paris Observatory.

[2] *Mémoires de l'Académie Impériale des Sciences de St. Pétersbourg*, vii. série, tome xxxi. No. 9.

[1] *Comptes rendus*, vol. xxix. 1849, p. 90.

a beam of light from the first would be reflected directly back to it. By means of a transparent glass, fixed in the eye-piece at an angle of 45°, a beam of light was sent from the first telescope to the second, and, on its return through a total distance of 17 kilometres, could be seen as a star by an eye looking through the first. Alongside the eye-piece of the latter a revolving wheel, with 720 teeth cut upon its circumference, was fixed in such a way that the beam of light both in going and coming had to pass between the teeth. When the wheel was set so that the tooth was in the focus, the beam would be entirely cut off in its passage through the telescope. Changing the position of the wheel through half the space between the middles of two consecutive teeth, the light would go and come freely between the teeth. When the wheel was set in revolution a succession of flashes would be sent out. If, on the return of each flash, a tooth was interposed, it would be invisible to the eye looking through the telescope. Fizeau found that with a velocity of 12·6 turns per second each flash which went out was on its return cut off by the advancing tooth. With a velocity twice as great as this it was seen on its return through the opening next following that through which it went. With three times this velocity it was caught on the second tooth following, and so on.[1]

This experiment of Fizeau was soon followed by the application of the revolving mirror of Sir Charles Wheatstone. Shortly after measuring the duration of the electric spark this investigator called attention to the fact that the same system could be applied to determine the velocity of light, and especially to compare the velocities through air and through water. In 1838 the subject was taken up by Arago, who took pains to demonstrate that it was possible, by the use of the revolving mirror, to decide between the theory of emission and that of undulations by determining the relative velocities in air and in a refracting medium.[2]

The difficulties in the way of securing the necessary velocities of the mirror and of arranging the apparatus were such that Arago never personally succeeded in carrying out his experiments. This seems to have been done almost simultaneously by Foucault and Fizeau about the beginning of 1850. Both experimenters seem to have proceeded substantially on the same principle and to have reached the same result, namely, that the motion of light through water was slower than through air in the inverse proportion of the indices of refraction of the two media.[3]

An important and most necessary modification of Arago's plan was made by these experimenters. As originally proposed, the plan was to send an instantaneous flash of light through water and through the air, and to receive it on the revolving mirror and determine the relative deviations in the positions of the images produced by the two rays. This system would, however, be inapplicable to the measurement of the actual time of transmission, owing to the impossibility of making any comparison between the time at which the flash was transmitted, and that at which it was received on the mirror. This circumstance would, indeed, have rendered the actual realisation of Arago's project nearly impossible for the reason that the flashes of light, seen through the water, would have reached the mirror at every point of its revolution; and only an exceedingly small fraction of them could have been reflected to the eye of the observer.

This difficulty was speedily overcome by Foucault and Fizeau by a most ingenious arrangement, of equal importance with the revolving mirror itself. Instead of sending independent flashes of light to be reflected from the mirror, a continuous beam was first reflected from the revolving mirror itself to a fixed mirror, and returned from the fixed mirror back on its own path to the revolving one. A succession of flashes was thus emitted as it were from the fixed mirror, but their correspondence with a definite position of the revolving mirror was rendered perfect. Moreover, by this means, the image was rendered optically continuous, since a flash was sent through and back with every revolution of the mirror, and after the velocity of the latter exceeded 30 turns per second, the successive flashes presented themselves to the eye as a perfectly continuous image.

It was not until 1862 that this system was put into operation by Foucault for the actual measurement of the velocity of light through the atmosphere. A new interest had in the meantime been added to the problem by the discovery that the long-accepted value of the solar parallax was too small, and that the measurement of the velocity of light afforded a method of fixing the value of that constant. The central idea of the method adopted by Foucault was that already applied in comparing velocities through different media. The element sought is made to depend upon the amount by which the revolving mirror rotates while a flash of light is passing from its surface to the distant reflector, and coming back again. As the details of Foucault's method will be best apprehended by a comparison of them with those adopted in the present investigation, a complete description of his apparatus will here be passed over. It may, however, be remarked, that what he sought to observe was not the simple deviation of a slit, but the deviation of the image of a reticule. The deviation actually measured was 0·7 millimetre, and the system adopted was to determine at what distance, with a definite velocity, this amount of deviation could be obtained. His result for the velocity of light was 298,000 kilometres per second.

The next measures of the element in question were those of Cornu. The method which he adopted was not that of the revolving mirror, but Fizeau's invention of the toothed wheel. His earlier measures, made in 1870, and communicated to the French Academy in 1871, led to a result nearly the same as that of Foucault.[1] This result was, however, not so satisfactory that the author could record it as definitive. He, therefore, in 1874, repeated the determination on a much larger scale and with more perfect apparatus. The distance between the two stations was nearly 23 kilometres, and therefore much greater than any before employed. He was thus enabled to follow the successive appearances and extinctions of the reflected image to the thirtieth order; that is, to make fifteen teeth of his wheel pass before a flash returned from the distant reflector, and to have it stopped by the sixteenth tooth.

This method has a defect, the result of which is evident by an examination of Cornu's numbers. It is that the extinctions and reappearances of the light as the wheel changes its speed are not sudden phenomena, occurring at definite moments, but are so gradual that it is difficult to fix the precise moment at which they occur. Of this defect the able experimenter was fully conscious, and his discussion of the disturbing causes which come into play, and of the amount of error due both to the apparatus, the observer, and to the method of eliminating them, form altogether one of the most exhaustive discussions of a physical problem.[2] But the uncertainties are not of a kind which admit of complete investigation, and it now appears that although his result was far superior in point of accuracy to that of Foucault, it was nevertheless in error by about 0·0015 of its whole amount. It was, in

[1] It is curious that the author's account of this remarkable experiment, which forms an epoch in the history of physical science, is contained within the limits of two pages, and terminates without any definite discussion of the results. It is merely stated that the result is 70,948 leagues of 25 to the degree, but the velocity, in kilometres, which must have been that first obtained, is not given, nor is it stated what length the degree was supposed to have in the computation.

[2] *Comptes rendus*, 1838, vol. vii. p. 954; *Œuvres de François Arago*, vol. vii. p. 569.

[3] *Comptes rendus*, xxx. 1850, pp. 551 and 771.

[1] *Comptes rendus*, vol. lxxii. 1871, p. 857.

[2] Annales de l'Observatoire de Paris, *Mémoires*, tome xiii.

fact, when reduced to a vacuum, 300,400 kilometres per second, while we may now regard it as well established that the true velocity is less than 300,000.

The next determination of the velocity of light was that of Michelson,[1] whose result was 299,910 kilometres per second. His investigation being a part of the first volume of the present series need not be here discussed, but it is worth while to remark that his method seems far superior in reliability to any before applied.

An attempt has been made by Messrs. James Young and George Forbes to improve Fizeau's method, by diminishing the uncertainty arising from the gradual extinction of the visible image.[2] By the method of these experimenters the result depends, not upon the moment when the image disappears, but when two images, side by side, are equal in brightness. This is effected by employing two reflectors, at unequal distances, but nearly in the same line from the telescope, to return the ray. Each reflector then forms its own image in the field of view of the sending telescope. With a regularly increasing velocity of the toothed wheel, each image goes independently through the same periodic series of changes as when only one mirror is used; but owing to the unequal distance the period is not the same. If the speed of the mirror be carried to such a point that the difference of phase in the two images is half a period, then one image will be increasing while the other is diminishing, and the stage at which the two images are equal would appear to admit of fairly accurate determination.

The distant reflectors were separated from the observing telescope by the Firth of Clyde. The distances were respectively 16,835 feet, and 18,212 feet. A study of the printed descriptions of their experiments gives the impression that the performance of the subsidiary parts of the apparatus was not such as to do justice to the method. The resulting velocity of light was 301,382 kilometres per second, and the difference between the extreme results of twelve separate determinations was 4000 kilometres.

The most important result of the work of these gentlemen, could it be accepted, would be the establishment of a difference between the velocities of differently-coloured rays. We may regard it as quite certain, from the absence of any change in the colour of the variable star, β Persei, while it is increasing and diminishing, that the difference between the times required by red and by blue rays to reach us from that star cannot exceed a moderate fraction of one hour. It is quite improbable that its parallax is more than $0''.1$, and therefore probable that its distance is 2,000,000 or more astronomical units. The possible difference between the velocities in question can, therefore, only be a small fraction of the hundred-thousandth part of either of them. No apparatus yet devised would suffice for the measurement of a difference so minute, and we are justified in concluding that the phenomena observed by Messrs. Young and Forbes arose from some other cause than a difference between the velocities of red and blue rays.

The present determination had its origin as far back as 1867. In his "Investigation of the Distance of the Sun," published in that year, the author introduced some remarks upon Foucault's method, and pointed out the importance to the determination of the solar parallax of repeating the determination of Foucault on a much larger scale, with a fixed reflector placed at a distance of three or four kilometres.[3]

From that time forward the subject excited the attention of American physicists, several of whom formed plans, more or less definite, for executing the experiments. As, up to the year 1878, no important steps in this direction had been taken, the author, in April of that year, brought the subject before the National Academy of Sciences, with the view of eliciting from that body an expression of opinion upon the propriety of asking the Government to bear the expenses of the work. The subject was referred to a Select Committee, who, in January, 1879, made a favourable report on the subject, which was communicated to the Secretary of the Navy. On the recommendation of the Secretary, Hon. R. W. Thompson, Congress, in March following, made an appropriation of $5000 for the purpose, and the author was charged by the Department with the duty of carrying out the experiments.

In the meantime it became known that Mr. Michelson had made preparations for repeating Foucault's determination at his own expense, with the desirable improvement of placing the fixed reflector at a considerable distance. But before the reliability of Mr. Michelson's work had been established, the preparations for the present determination had been so far advanced that it was not deemed advisable to make any change in them on account of what Mr. Michelson had done. The ability shown by the latter was, however, such that, at the request of the writer, he was detailed to assist him in carrying out his own experiments, and acted in this capacity until September 1880, when he accepted the Professorship of Physics in the Case Institute, Cleveland, Ohio. After the departure of Mr. Michelson his place was taken by Ensign J. H. L. Holcombe, U.S.N., who assisted in every part of the work to the entire satisfaction of the projector until its close.

[1] "Astronomical Papers of the American Ephemeris," vol. i. part iii. Owing to an error in applying one of the corrections the result was given as 299.912 kilometres.

[2] *Philosophical Transactions* for 1882, p. 231.

[3] Washington Observations, 1865. Appendix ii.

PANCLASTITE

DR. SPRENGEL has sent us a reprint of a note sent by him to the *Chemical News* on this subject. After showing that these new explosives, so named by Mr. Turpin, are not original, he continues:—

"The 'beau idéal' of a detonating explosive is a mixture of 8 parts (88.9 per cent.) of liquid oxygen and 1 part (11.1 per cent.) of liquid hydrogen.

"In my paper of 1873 I say, p. 799:—'On referring to the foregoing table the reader will be reminded that peroxide of hydrogen is the highest oxygen compound known, while nitric anhydride is the compound which contains the largest amount of oxygen available for combustion (74 per cent.). But as this compound, as well as the next two, nitric peroxide (69.5 per cent. oxygen) and tetranitromethane (65.3 per cent. oxygen) are: at present: on account of their nature and their difficult preparation, mere chemical curiosities, my attention naturally turned to the fourth, to *nitric acid* (63.5 per cent. oxygen), which is a cheap and common article of commerce.'

"Now, when Mr. Turpin's attention turned to the second oxidiser on my list—to nitric peroxide—he found that this substance does *not corrode* metals, such as iron, copper, and tin under 356° F. (180° C.); and further, that combustible liquids, such as petroleum, carbon bisulphide, and nitro-benzene are readily soluble in nitric peroxide *without* rise of temperature. These are valuable properties, *first noticed by Mr. Turpin.*

"What was formerly a chemical curiosity is now an article of commerce. Nitric peroxide may be bought to-day at eighteenpence the pound, and I see ways and means of producing it a great deal more cheaply. Nitric peroxide is a yellowish liquid, heavier than water (sp. gr. = 1.451), and boils at 71° F. (22° C.), but may be kept like ether or similar volatile liquids. In France it is sent about in tinned-iron cans.

"Taking as a typical example a benzene-mixture—

$$\left.\begin{array}{l} C_6H_6 = 18.4 = \begin{array}{r} 17.0\ C \\ 1.4\ H \end{array} \\ 7\tfrac{1}{2}(NO_2) = \underset{\overline{100.0}}{81.6} = \begin{array}{r} 56.8\ O \\ 24.8\ N \end{array} \end{array}\right\} = \begin{array}{l} 62.6\ CO_2 \\ 12.6\ H_2O \end{array}$$

we see, that the 18·4 parts of benzene require 56·8 parts of oxygen for the oxidation of their carbon and hydrogen to carbonic acid and water. This oxidation or combustion takes place at the moment of explosion at the expense of the 56·8 parts of oxygen, contained in the rest of the mixture—the 81·6 parts of nitric peroxide. No other explosive now in use (including blasting gelatin) contains weight for weight a greater amount of combustible matter, and as an explosion of *these* bodies is simply a sudden combustion, I again beg to draw attention to the fact that the oxygen available for combustion in gun-cotton is most probably not more than 32·3 per cent. and in nitroglycerin 42·3 per cent.,[1] while in this case we have without a doubt 56·8 per cent. Hence no other explosive now in use can rival this and similar mixtures in power, as I published in 1873. They still remain *the most powerful* explosives known.

"It hardly need be said that an explosive of this nature consists of two parts—an oxidising and a combustible agent—and that Mr. Turpin with the same *naïveté* lays claim not only to the first, but also to the latter half of the subject.

"None of my *safety*-explosives are licensed in England, though many of them, when mixed, are much less sensitive to concussion than common gunpowder.

"In April 1884 the French military authorities were busy near Rochefort with shells of the 'système Turpin.' These shells, so my informant said, were made of such a size, and possessed such a prodigious power, that a ship struck by one of them would inevitably be sent to the bottom of the sea, even were she the strongest ironclad afloat. It is devoutly to be hoped that those whose office it is to provide for the defence of the British Navy *will be ready* in the hour of need to serve out shells, filled with an explosive of equal force or better still with something superior, approaching more closely the 'beau idéal.'"

MR. VERBEEK ON THE KRAKATÃO DUST-GLOWS

As it appears from the letter of Mr. Douglas Archibald in NATURE of April 29 (p. 604) that some doubt exists as to the quantity of volcanic dust ejected during the Krakatão eruption in 1883, it may not be inopportune to give an abstract of what Mr. Verbeek—the best authority on the subject—says in the second part of his book. The mistake in the number of cubic kilometres—which Dr. Riggenbach or his critic magnified from 18 into 150—may possibly have arisen from the comparison Mr. Verbeek draws between the quantity of volcanic substances ejected by the Tambora in 1815 and that ejected by Krakatão.

Junghuhn estimated the quantity of ashes ejected by the Tambora in Sumbawa at 318 cubic kilometres, but Mr. Verbeek reduces it by calculation to about 150, though he adds that the data are insufficient to form a really correct estimate. It is certain, however, that the quantity was considerably larger than that ejected by Krakatão. To calculate this quantity Mr. Verbeek made observations everywhere on the islands and along the coasts of the Straits of Sunda; while the thickness of the ashes which fell into the sea was computed according to the difference in the depths of the sea before and after the eruption, a difference which greatly varies, and amounts in some places to 40 metres, if not more. Wherever some doubt exists for want of previous accurate deep-sea soundings, Mr. Verbeek gives the lowest figures. These observations are all illustrated by maps. Mr. Verbeek estimates the quantity of ejected material which fell round the volcano at 18 cubic kilometres at least. The possible outside margin would, however, not exceed 3 cubic kilometres. Of this quantity, two-thirds, or 12 cubic kilometres, lies within a circle with a radius of 15 kilometres drawn round Krakatão, one-third, or 6 kilometres, outside it. Of the finer ashes a large quantity were already, during the first three days, blown into the sea, as appeared from observations made on ships; and Mr. Verbeek assumes that considerably less than 1 cubic kilometre remained floating in the upper regions of the atmosphere. This quantity would correspond to a layer of 0·002 millimetre thickness divided over the whole surface of the earth, or of 0·004 millimetre over the temperate zones only.

Such an infinitesimally thin layer could hardly have been the principal cause of the atmospheric phenomena. They must be accounted for in a great measure by the large volume of aqueous vapour ejected by Krakatão, the amount of which lies, unfortunately, beyond all calculation. We have to deal with two distinct phenomena, as Prof. Michie Smith also has shown by the two different spectra, and these phenomena had different causes: thus, the blue and green tints of sun and moon, which were specially observed during the first month after the eruption, and only in places close to the equator, must be principally ascribed to the *solid* particles in the volcanic ash-cloud, as various observations have shown that these are the main cause of the special absorption of the rays of light by which the sun appeared blue and green; the aqueous vapour may have increased the phenomenon, for it is known that the sun can look bluish through mist. It cannot be said to be a proof to the contrary that Mr. Lockyer saw the sun green through the steam which escaped from the funnel of a steamer, for probably a quantity of ash and soot-particles escaped from the funnel at the same time, and it is possible that the sun appeared green from that very fact. The steam was thus in the identical condition of our volcanic cloud. It was only in the beginning after the eruption, before the ashes had spread very far, and when, therefore, their density was greater, that they were able within a limited space to give green tints to the sun. This phenomenon ceased when the ashes were dispersed further round the globe—in the northern hemisphere by the south-west, in the southern hemisphere by the north-west winds—and when probably also a portion of them fell gradually on the earth.

The crimson after-glows which soon followed the eruption were observed *at the same time* over a much larger area than that within which the blue and green sun was seen at successive periods, and they are believed by Mr. Verbeek to have been caused mainly by the masses of aqueous vapour thrown out by Krakatão, and which formed the greater part of the volcanic cloud. This vapour, after condensing and freezing in the higher and colder regions of the atmosphere, produced the remarkably beautiful sunsets, while the ashes may have intensified the phenomenon, besides serving as a centre of condensation for the vapour. The real cause of the crimson glows was therefore probably the same as that of the evening red, their intensity being a consequence of the extraordinary quantity of vapour in the upper regions emitted by Krakatão.

THE PARIETAL EYE OF HATTERIA

Some little time ago, whilst engaged in work upon *Hatteria punctata*, I found a curious sense-organ embedded in the substance occupying the parietal foramen, but was unable at the time to examine the specimen further; Prof. Moseley has kindly directed my attention to a short paper published in the *Zoologischer Anzeiger*

[1] Of these, by the bye, only 38·8 per cent. can be utilised for want of fuel, as pointed out by me in my patent of 1871, and verified four years later by the force of Nobel's blasting gelatin, in which the excess of 3·52 per cent. oxygen is utilised by the dissolved gun-cotton, an explosive too rich in carbon. See Abbot's table, p. 17, in "The Hell-Gate Explosion near New York and so-called 'Rackarock,' with a few words on so-called 'Panclastite,'" by H. Sprengel. London: E. and F. N. Spon, 1886.

for March 29, 1886, by Von Henri W. de Graaf, "Zur Anatomie und Entwickelung der Epiphyse bie Amphibien und Reptilien," wherein are described briefly (1) the development of the epiphysis, and (2) the structure of this part in the adult animal in certain amphibia and reptiles. An examination by means of sections at once revealed the fact that in Hatteria the epiphysis becomes modified in a manner more interesting than that found by Von Graaf to obtain in *Anguis fragilis*—the most modified form described by him.

The epiphysis apparently arises as a hollow outgrowth from the roof of the third ventricle (region of thalamencephalon), and in both amphibia and reptilia becomes divided into two parts—a proximal one remaining in connection with the brain, and a distal bladder-shaped structure—the two becoming in most cases completely separated from each other. In *Anguis fragilis* Von Graaf finds that the distal part loses all connection with the brain, and develops into a structure resembling a highly organised invertebrate eye with, however, the important and curious exception that no nerve is present.

In Hatteria a still more interesting modification takes place, the distal portion being, as in Anguis, modified to form an eye; but this, unlike that in the latter, is provided with a well-marked nerve.

Fig. 1 shows the structure of the eye. The whole is enclosed in a capsule of connective tissue (C); anteriorly a lens (L) is present, composed of cells whose nuclei are very distinct. The lens forms the anterior boundary of a vesicle, the walls of which are formed

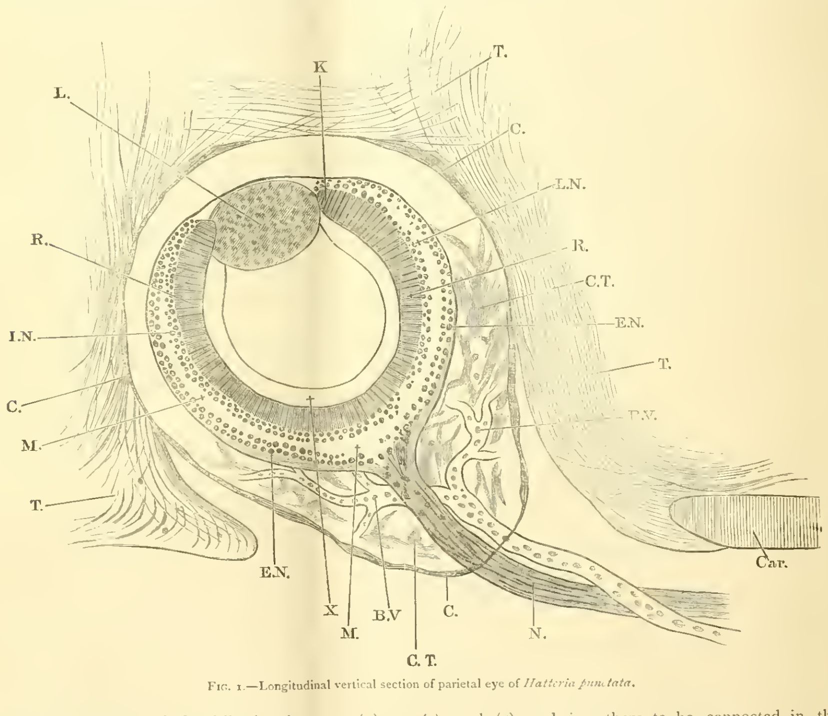

FIG. 1.—Longitudinal vertical section of parietal eye of *Hatteria punctata*.

from within outwards of the following layers:—(1) a layer which is not well marked (X), and which may possibly be due to the shrinkage and clinging to the walls of the contents of the vesicle, fluid in life; (2) a layer of rods (R) embedded in dark brown pigment, the pigment being specially developed anteriorly at the part indicated by the letter K; (3) a double or even triple row (I N) of nuclei; (4) a clear layer (M) which scarcely takes stain, and may be called the molecular; and (5) an outer layer (E N) of nuclei two or three rows deep.

This structure will, so far, be seen to correspond closely with that of Anguis.

Posteriorly a nerve enters the eye, the fibres spreading round behind the vesicle; the rods may be observed giving off processes from their external ends, which in some cases appear to pass right through the layers (3), (4), and (5), and in others to be connected in their passage with the nuclei of these layers.

However, I hope in a very short time to publish a detailed account of the histological structure of the organ.

The capsule containing the eye is filled posteriorly with connective tissue (C T), in which breaks up and ramifies a blood-vessel which enters along with the nerve (B V).

Fig. 2 represents somewhat diagrammatically a section transverse to the parietal foramen, showing that the eye is single and lies exactly in the median line. A depression of the skin of the head occurs immediately over the parietal foramen, but does not lead down into this, which is filled up by a plug of connective tissue (Fig. 1, T, Fig. 2, P T), specially dense (D T) around the eye capsule. The nerve

is single, and leads downwards and backwards in the median line, being enveloped in the tissue passing from the foramen directly to the roof of the thalamencephalon.

I have not yet actually traced the nerve itself into the brain, but it is difficult to imagine that it can possibly arise as a branch from a cranial nerve, being *single* and *medianly placed*, and, as just said, enveloped in the material running directly to the roof of the brain from the foramen.

There can be little doubt that it represents the stalk connecting the distal with the proximal outgrowth from the roof of the thalamencephalon, this part having apparently disappeared in other reptiles and amphibia (so far as is yet known).

This being the case it is extremely interesting to observe that another instance will be added to that of the optic nerves in which an, at first, hollow outgrowth from the brain becomes solid and transformed into a nerve, and further that the latter, as in the former case, is connected with an organ of vision.

Though it is difficult to imagine what can be the use of the organ in its present state, seeing it is deeply embedded in connective tissue—so deeply as almost to

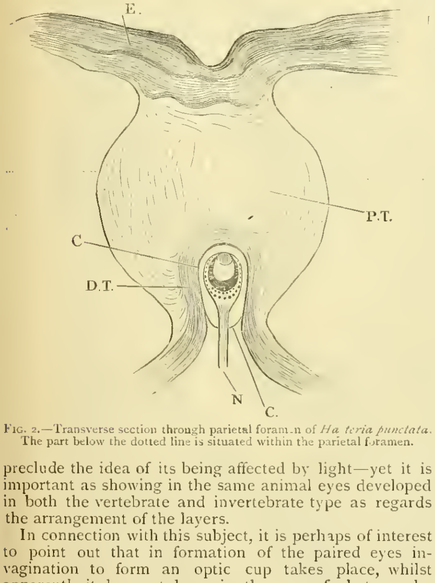

FIG. 2.—Transverse section through parietal foramen of *Hatteria punctata*. The part below the dotted line is situated within the parietal foramen.

preclude the idea of its being affected by light—yet it is important as showing in the same animal eyes developed in both the vertebrate and invertebrate type as regards the arrangement of the layers.

In connection with this subject, it is perhaps of interest to point out that in formation of the paired eyes invagination to form an optic cup takes place, whilst apparently it does not do so in the case of what may be called the *parietal eye*. A little consideration shows that the relative position of the rods depends entirely upon this invagination. In both cases they are formed upon the inner surface of the wall in the position corresponding to the epithelium of the neural canal: but in the one instance they are, by decay of the outer wall of the invaginated cup, placed apparently on the outside of the optic vesicle; whilst in the other instance they are formed in a similar position, but, as no invagination takes place and subsequent decay of one wall, they line the cavity of the vesicle. According to this we must suppose that the part of the wall where the lens is present has either disappeared or become modified into this.

We may further observe that in both types of eye the nerve enters into connection with the elements on the surface opposite to that on which the rods are developed.

In conclusion my thanks are due to Prof. Moseley for his kindness in drawing my attention to the subject, and to Mr. E. B. Poulton, of Keble College, Oxford, who kindly placed two specimens of Hatteria at my disposal, and to Mr. Beddard, of the Zoological Society, for the use of another specimen. W. BALDWIN SPENCER

Anatomical Department, University Museum, Oxford

NOTE.—Since writing the above I have found the eye present in several other lizards, notably in Iguana, *Chameleo vulgaris* and *Lacerta ocellata*, and have traced the nerve into the proximal part of the epiphysis.

ASTRONOMICAL PHOTOGRAPHY[1]

SOME attempts made last year at photographing the heavens by means of an instrument quite rudimentary having yielded good results, the director of the Paris Observatory gave orders for the construction of a special apparatus, the design of which is shown in the accompanying figure (Fig. 1). The mechanical part has been executed in a highly remarkable manner by our accomplished artist, M. Gautier; the objective is our own production.

This new instrument is composed of two telescopes in juxtaposition inclosed in a single metallic tube in the form of a parallelopiped, and separated from each other along their whole length by a narrow partition.

One of the object-glasses, with an aperture of 0·24 m. and a focal length of 3·60 m., is intended for eye observation, and serves as a pointer. The other, with an aperture of 0·34 m. and a focus of 3·43 m., is achromatised for the chemical rays, and serves the purpose of photography. The optical axes of these two objectives being parallel, every star kept in the centre of the field of the eye-piece belonging to the first telescope produces its impression in the centre of the sensitive plate of the photographic apparatus.

The equatorial is mounted in the form called English, that is to say, the centre of the tube rests always in the polar axis of the instrument. This arrangement allows of a star being followed from its rising to its setting without involving the necessity of bringing the instrument back to the vicinity of the meridian. Like a common equatorial it is furnished with hour circle and circle of declination, and with a clock movement keeping the apparatus in operation for three hours without fresh remounting. There are, moreover, independent very slow movements, whereby the axis of the telescope can be kept on a fixed point in the heavens, notwithstanding some slight irregularity in the movement of the clockwork, the orientation of the telescope, or the variations of atmospheric refraction.

The photographic objective—the largest ever yet produced—is formed according to a simple achromatic system, and, though of an extremely short focal length, is able, without the use of any diaphragm, exactly to cover the very considerable field of 3° diameter.

Although but very recently mounted, this apparatus has already availed for the performance of numerous tasks. On star photographs it is possible to distinguish traces of stars of the 15th magnitude, too feebly marked, however, to bear transference on paper. The stars of the 14th magnitude are reproduced with a diameter of 1/40 of a millimetre.

It is obvious that such small points might be liable to be confounded with the impurities of the sensitive coating if the precaution is not taken to multiply the stationary points. Each star is formed by a group of three points constituting an equilateral triangle, each side of which is no more than 1/12 of a millimetre. To the naked eye these three points appear to merge into one, but on examining them with the aid of a somewhat

[1] From an article by the Brothers Henry in *La Nature*.

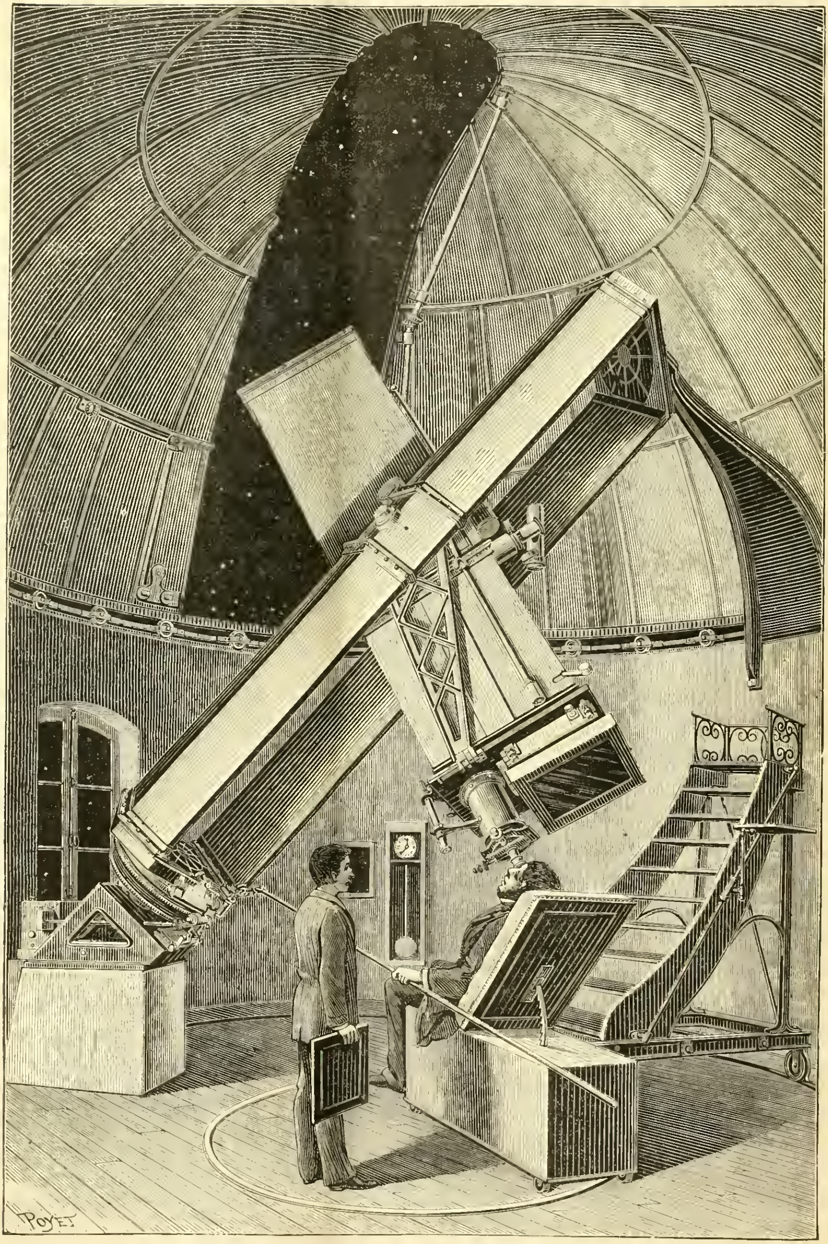

FIG. 1.—Parallactic apparatus newly established at the Paris Observatory for celestial photography.

powerful lens the three points come out distinctly, and it is then an easy task to eliminate all that does not belong to the heavens.

The construction of such a map, obtained by the apparatus as above described in three hours, would assuredly have demanded several months of assiduous labour by the ordinary processes.

The following is the time of exposure requisite to obtain the image of the stars.[1]

Magnitude		h.	m.	s.
1	...	0	0	0·005
2	...	0	0	0·013
3	...	0	0	0·03
4	...	0	0	0·08
5	...	0	0	0·2
6	The extreme limit of magnitude of stars visible to the naked eye	0	0	0·5
7	...	0	0	1·3
8	...	0	0	3
9	...	0	0	8
10	...	0	0	20
11	Mean magnitude of the asteroids	0	0	50
12	...	0	2	0
13	...	0	5	0
14	...	0	13	0
15, 16	The last of the stars visible with the aid of the great instruments	1	23	0

All the above figures represent a minimum. To secure good reproductions on paper the time of exposure would have to be increased threefold.

The above table shows that the time of exposure required in taking a star of the first, and that in taking a star of the last magnitude differ from each other as 1 : 1,000,000. (The relation adopted between the brightnesses of two consecutive magnitudes is 2·542.)

Outside the construction of celestial maps, another field of study of great importance now created by photography may be cited, the discovery, namely, of the asteroids. The little stars fixing themselves on the plate as so many mathematical points, so to say, the planets are distinguished therefrom, each by a little line perfectly defined indicating its proper movement in amount and direction during the time of exposure of the apparatus. It is in this way we have already succeeded in obtaining the trace of a small planet of the 11th magnitude which by a small line extremely well defined gave account of its march among the fixed stars.

It will even be possible to study the movement of the satellites round their planet, and perhaps discover new ones.

The study of the double and multiple stars will be greatly facilitated, and photography will be equally available in the investigation of the parallaxes.

Finally, photometry must be adduced as one of the branches of astronomy which will now be able to collect very valuable information through the utilisation of photography.

In conclusion, it is worth while remarking how this fresh step in advance has sensibly enlarged the scope of man's vision. In consequence of it we can now obtain the image of a star, which instruments of the same opening as those employed by photography would never of themselves have elicited out of their invisibility.

PAUL ET PROSPER HENRY

NOTES

THE first *soirée* of the Royal Society this season took place last night. A large number of Fellows and visitors were present, and many objects of interest were exhibited.

THE visitation of the Royal Observatory by the Board of Visitors is fixed this year for June 5.

[1] For these results we have made use of the gelatino-bromide plates of Monckhoven.

THE Royal Irish Academy is celebrating the centenary of its foundation this week.

DR. GILL, Her Majesty's Astronomer at the Cape, has been elected Corresponding Member of the Imperial Academy of Sciences of St. Petersburg.

THE fifty-seventh anniversary meeting of the Zoological Society was held on Thursday week. The chair was taken by Prof. Flower, LL.D., F.R.S., the President. The report of the Council on the proceedings of the Society during the year was read by Mr. P. L. Sclater, F.R.S., Secretary of the Society. It stated that the number of Fellows on December 31, 1885, was 3193, showing a decrease of 62 as compared with the corresponding period in 1884. The total receipts for 1885 had amounted to 25,809*l.* 10*s.* 1*d.*, being a decrease of 3129*l.* as compared with the previous year. This decrease was mainly due to the falling off in the receipts under the head of admissions to Gardens, and in the amounts received for admission and composition fees from newly elected Fellows. The ordinary expenditure for 1885 had been 24,593*l.* 11*s.* 8*d.*, against 26,539*l.* 4*s.* 1*d.* for 1884. Besides that, an extraordinary expenditure of 491*l.* 0*s.* 6*d.* had been incurred, which brought up the total expenditure for the year to 25,084*l.* 12*s.* 2*d.* The visitors to the Society's Gardens during the year 1885 had been 659,896, against 745,460 in 1884. The Davis Lectures on zoological subjects, having been well attended during the past year, would be continued during the present season, beginning with a lecture on "Pigs and their Allies," by Prof. Flower, LL.D., F.R.S., on Thursday, June 3, at 5 p.m. The number of animals in the Society's collection on December 31 last was 2551, of which 756 were mammals, 1366 birds, and 429 reptiles. Among the additions made during the past year 21 were specially commented upon as of remarkable interest, and in most cases new to the Society's collection. About 36 species of mammals, 15 of birds, and 4 of reptiles had bred in the Society's Gardens during the summer of 1885. The report concluded with a long list of the donors and their various donations to the menagerie during the past year.

WITH regard to the recent explosion of the 43-ton gun, it is fortunate that it has happened without loss of life. Competent authorities, as seen from Col. A. Moncrieff's letter (which we reproduce from the *Times*) show that it could. How long are our gun factories to go on making guns condemned by easily-understood scientific principles? "Col. Maitland's interesting paper read at the Royal United Service Institution on June 20, 1884," Col. Moncrieff writes, "published the process adopted at Woolwich in settling the types of the new steel breech-loading ordnance for the British service, as well as the proportions of the new guns on these types then in process of manufacture. Mr. W. Anderson's investigations, published in a lecture read before the Society of Arts on January 29, 1885, and also commented upon in the *Engineer* of February 6, 1885, clearly demonstrated that these guns were deficient in strength in front of the trunnions. It is a remarkable fact that several of the guns have now burst at the point and in the manner which could have been predicted by any one consulting Mr. Anderson's demonstrated results. As the subject is of vital importance to the country, it would seem wise either to refute Mr. Anderson or accept his method and consult him; his valuable service in having discovered the prevailing error and worked out this most difficult problem is too little known; it would thus be utilised and acknowledged to the advantage of the service. By treating a gun as a heat-engine and accounting for every part of the energy generated by the explosion of the powder, he has, in a scientific and complete manner, proved that the metal crusher gauges from which the accepted curve of pressure is obtained are not to be relied on.

The form of the guns is adapted to the curve of pressure; that curve, as shown by Col. Maitland at the Royal United Service Institution, is wrong; the maximum pressure which is near the breech is known, but with the slow-burning powder in a long gun the total pressure, and the maximum pressure at any point of the bore, has never been accurately determined. If Mr. Anderson's conclusions carefully arrived at by calculation are correct, these guns are out of proportion between the trunnions and the muzzle, where the bursts have all taken place. Another branch of the same subject is the measurement of the energy of recoil, of much importance in designing disappearing carriages. In this branch, I can answer for it, that Mr. Anderson's conclusions tally with the practical result—a satisfactory proof of their correctness. His discovery is of great practical value in making gun-carriages of all descriptions, and has changed, once and for all, the previously accepted formulæ for the force of recoil given in the text-books, which often led to costly mistakes in construction. Mr. Anderson has been trying, since the publication of his lecture at the Society of Arts, to induce the Government to test the correctness of his views by means of the Sebert velocimeter, but without success. It must be admitted that in determining so important a matter, one on which the efficiency of our ships and a large national expenditure depends, it should be the first desire of every one to secure without delay the highest scientific and practical experience within reach, and to consult men who have devoted special study and research to the subject."

On April 30 there took place in Paris, at the Ministry of Public Instruction, a meeting of French astronomers. M. Faye was in the chair. It was decided unanimously to build three photographing telescopes. One of these is destined for the Algiers Observatory. The destination of the others will be determined upon when finished. The construction will take eighteen months.

M. Janssen has terminated the installation of the tubes for analysing the influence of the atmosphere on spectroscopic analysis, absorbing power, &c. Their length is 100 metres, and they can be filled with gas under a pressure of 100 atmospheres. The light is supplied by a battery of 60 Bunsen elements. Experiments are conducted on nitrogen, oxygen, common air, &c.

Herr Paul von Ritter, who died at Basle, has left to the University of Jena a sum of 300,000 marks, to be employed for the furtherance of zoological studies.

Canada is nearly the only important British colony without its Government Botanic Garden; the identity of its flora with that of the Northern United States rendering such an establishment of much less value than in most of our colonial possessions. But for some years past leading Canadians interested in horticulture have been exerting themselves for the establishment of a Botanic Garden at Montreal. Through the co-operation of the authorities of McGill College and the Council of the Montreal Horticultural Society, this object is now secured, and the "First Annual Report" of the "Montreal Botanic Garden" is issued. The Garden is not yet in existence; but a very favourable site of seventy-five acres has been secured in Mount Royal Park, a varied piece of ground admirably adapted for the purpose, on the slope of the beautiful mountain overlooking the city, from which it derives its name. An Act of Incorporation for the "Montreal Botanic Garden Association" has been obtained, wherein the objects of the corporation are stated to be "By the medium of a Botanic Garden and other accessories, to promote research in forestry and economic botany, and advance the interests of technical and general botanical knowledge." Among the means contemplated in the future for carrying out these objects we are glad to see the establishment of courses of lectures on special subjects and a laboratory for special research. We wish every success to the new Association, which solicits contributions in trees, shrubs, seeds, and publications.

At the last general meeting of the Folk-Lore Society, Capt. Temple read a paper on the science of folk-lore. At the conclusion he referred to terminology. Folk-lore, he said, is a fine English compound, but there is a sad want of an alternative, if only for the sake of useful and necessary derivatives. Folk-lorist and folk-loric are not pleasant forms, but students have been driven to use both. He suggests some classically-formed synonym, such as *demology*, *demosophy*, or *demonocy*—the last for choice—capable of easy development into passable derivatives, as being of practical use. Dogma has been appropriated already, or *dogmology* might, he thinks, answer, and *demodogmology* is too long. *Dokeology* and *dokesiology*, as the study of fanciful opinions, are also suggested.

In a recent article in *La Nature* M. Martel refers to a discovery which he has made in the prehistoric caves in Lozère. For fifteen years past Dr. Prunières has prosecuted his investigations into the dolmens and neolithic grottos of the gorges of the Tarn, and has obtained some curious results on the fusion of a race of the age of polished stones and of an invading race of the Bronze Age. Last year in the cave of Nabrigas, M. Martel found in immediate contact with the remains of at least two skeletons of the *Ursus spelæus*, or great Quaternary bear, nine fragments of human skulls, of which one left superior maxillary had three teeth, and a piece of rough pottery, not turned in a lathe. The question whether, in the Stone Age, man, the contemporary of the reindeer and the great bear, was acquainted with the use of pottery is much debated, eminent names being found supporting the negative as well as the positive. But (continues M. Martel) the curious point about the present find is that fifty years ago, before the birth of "pre-history," when the existence of even Quaternary man was contested, M. Joly found in this very cave of Nabrigas a fragment of a large vessel in contact with the skull of a fossil bear. M. Martel is strongly of opinion that the usual theory of the fortuitous contact of these objects does not apply here; there is no trace of any disturbance, nor are any other neolithic objects found, the skull is in its natural position,—for these and other reasons he is persuaded that fossil man of the palæolithic age was acquainted with the potter's art.

The fish-hatching season at South Kensington, accounts of which we have published from time to time, is now drawing to a close, although there are still half a million fry on view at the Exhibition which have not yet absorbed their *umbilical sac*. The various species of fish bred have been presented by the National Fish Culture Association to public waters in the vicinity of London and in the country, whilst the Fishery of the Association has been well stocked with fry.

The Thames Angling Preservation Society, which is ever ready to secure fresh supplies of fish for the Thames, have lately netted one of the ponds in Kew Gardens for this purpose.

During the present week large consignments of fish have arrived at the aquarium of the Colonial and Indian Exhibition from the south coast and North Sea. The latest arrivals consist of cod, lings, haddocks, crustaceans of various species, grey mullet, bream, and Salmonidæ. A large Ascension turtle has also arrived in the tropical department, measuring 4½ feet by 3 feet. Considering the protracted period it was out of the water during transit, its condition on being placed in the Chelonian tank did not evidence the slightest signs of diminished vitality, which is another proof of the hardihood and tenacity of life possessed by this species. The turtle tank now contains twenty large specimens of the green and hawksbill kind, all of which seem in good health notwithstanding the artificial existence to

which they are subjected. In contiguity to the tank is a miniature beach whereon the turtle rest when out of water. A consignment of turtle eggs is expected this week, which will be laid out in the hatchery on arrival for the purpose of incubation. Some West Indian tortoises have just arrived, together with a selection of snakes and lizards, which form interesting exhibits. In consequence of the inability of the Royal Commissioners to obtain Indian and Colonial fishes, the National Fish Culture Association have taken the matter into their own hands, and have made arrangements with the Zoological Society in Calcutta and other bodies for supplies of tropical and other piscatorial specimens, so that the aquarium will be supplemented with many rare and important specimens.

MR. OTIS T. MASON's account of the valuable Guesne collection of antiquities in Point-à-Pitre, Guadaloupe, which appeared in the Smithsonian Report for 1884, has recently been issued in separate form. The collection originated with M. Mathieu Guesne, whose series of Carib stone implements attracted considerable attention at the Paris Exhibition of 1867. Since then it has been continued, and all but completed, by the son, M. Louis Guesne, who has devoted nearly twenty years of assiduous labour to the task of rescuing from destruction all existing relics of the ancient Carib race in the Island of Guadaloupe. He has also applied his artistic skill to the illustration of these objects, filling two large albums with aquarelles in natural size and colour of all the types in his museum. From these sources Mr. Mason has mainly compiled the present account, which is enriched with no less than 215 carefully prepared woodcuts of the Point-à-Pitre collection, and of a few others introduced for the purpose of comparison, and to supply omissions in West Indian archæology. The collection includes roughly-worked stones, indicating an industry in its infancy; and others so perfectly finished that it would be difficult to improve upon them either in design or workmanship. But all alike belong to what would be called the Neolithic period in Europe; all the stone implements are polished, and there is not a single object of this class formed solely by being chipped. In fact, the volcanic materials of which they are made cannot be worked by chipping, like flint, quartz, or obsidian. Some, especially, of the axes are so small that they seem to belong to a race of pigmies, while others are so large and heavy that they suggest a generation of Titans rather than of human beings. Besides the movable objects, mention is made of enormous stones carved with strange designs resembling those described by Mr. Im Thurn in British Guiana, some so high up as to be almost out of reach, others close to the ground or buried under the surface. Similar inscribed stones occur in the beds of rivers in the Island of St. Vincent, the last refuge of the Caribs in the West Indies.

HERR SCHÖYEN, in a paper recently reprinted from the *Transactions* of the Scientific Society of Christiania, describes a form of disease affecting the roots of growing barley, through which the farmers in Norway have of late years been suffering extensive loss. Contrary to the common opinion that the ravages due to this blight—which is popularly known as "Krog," crook, from the form of the deposits—were produced by an insect, Herr Schöyen maintains that this special barley-pest is a microscopic round worm, of the genus Tylenchus. After describing the appearance and character of the parasitic germs, which are deposited at the extremities of the roots, where their presence speedily manifests itself by the withering and death of the stalk before the grain can be set, he draws attention to the fact that similar deposits have been noticed on the roots of *Elymus arenaria*, the bind-grass so frequent on the Scotch, as well as the Norwegian, coasts. This observation derives special practical importance from the circumstance that at Lom, in Norway, where the barley crops have suffered most severely from the "Krog," the affected fields are in close vicinity to extensive tracts of *Elymus arenaria*. He proposes to continue his observations next summer with special reference to this point, but in the meanwhile he recommends as the only remedy available for the present that barley should not be re-sown on ground where the disease had manifested itself in the preceding season, nor in any locality where Elymus abounds. He finds that the bladder-like egg-cases of *Tylenchus hordei* can be thoroughly desiccated without destroying the inclosed worms.

SOME interesting statistics of the Japanese press have lately been published in the *Oesterreichische Monatsschrift für den Orient*, in which the newspapers and periodicals of Japan are arranged according to the subjects with which they deal. It appears that 37 publications are devoted to matters connected with education, and that these have a total circulation of 42,649 per month. There are 7 medical papers, with a monthly circulation of 13,514; 9 relating to sanitary matters, with a circulation of 8195; 2 on forestry; and 2 on pharmacy. There are 7 devoted to various branches of science, with a circulation of 2528; but to these must be added 29 engaged in popularising science, with a total circulation of 70,666.

THE additions to the Zoological Society's Gardens during the past week include a Purple-faced Monkey (*Semnopithecus leucoprymnus* ♀) from Ceylon, presented by Mrs. Larkins; a Brazilian Tree Porcupine (*Sphingurus prehensilis*) from Brazil, presented by Mr. J. E. Wolfe; two Sloth Bears (*Melursus ursinus* ♂ ♀) from India, presented by Mr. H. Mainwaring; a Burmese Squirrel (*Sciurus atrodorsalis*) from Burmah, presented by Mr. C. Crofton Black; a West Indian Agouti (*Dasyprocta cristata*) from West Indies, presented by Dr. A. Boon, F.R.C.S.; an Orange-thighed Falcon (*Falco fusco-cærulescens*) from Chili, presented by Capt. W. M. F. Castle, R.N.; five Senegal Parrots (*Pœocephalus senegalus*) from West Africa, presented by Mr. R. B. Sheridan; two Kestrels (*Tinnunculus alaudarius*), British, presented by Mr. J. S. Malcolm; a Wedge-tailed Eagle (*Aquila audax*) from Australia, presented by Mr. R. B. Colvin; a Tuberculated Iguana (*Iguana tuberculata*) from West Indies, presented by Mr. D. Morris; seven European Tree Frogs (*Hyla arborea*), European, presented by Mr. Thompson Hudson; a Californian Quail (*Callipepla californica*) from California, a Herring Gull (*Larus argentatus*), British, presented by Miss Hodge; a Two-banded Monitor (*Varanus salvator*), two Rat Snakes (*Ptyas mucosa*), an Indian Cobra (*Naia tripudians*) from Ceylon, presented by Mr. Carl Hagenbeck; a Moorish Toad (*Bufo mauritanica*) from Italy, a Green Toad (*Bufo viridis*) from Malta, presented by Mr. Alban Doran, F.R.C.S.; two Greek Tortoises (*Testudo græca*), European, presented by Admiral Mellersh; two Common Vipers (*Vipera berus*), British, presented by Mrs. Mowatt; a Small Hill-Mynah (*Gracula religiosa*), from Southern India, deposited; a Hog Deer (*Cervus porcinus*), seven Long-fronted Gerbilles (*Gerbillus longifrons*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE FLEXURE OF MERIDIAN INSTRUMENTS.—In a paper which forms Appendix III. to the "Washington Observations" for 1882, Prof. Harkness has made an exhaustive discussion of the subject of flexure, and the means available for eliminating its effects from star-places. He discusses separately the flexure of transit instruments and of vertical circles. The former are divided into two classes, according as their telescopes are straight or bent, but it is in the latter form that the effects of flexure are by far the greatest, the flexure-coefficients being in some instances as much as 0·55s. Prof. Harkness shows that the effect of flexure cannot be satisfactorily eliminated from the concluded right ascension of a star by simply taking the mean of the four results obtained by observing it directly and by reflec-

tion with the clamp of the instrument both west and east. It is better in his opinion to determine for each instrument the necessary corrections to be applied by means of the methods and formulæ explained in this paper.

In discussing the flexure of vertical circles Prof. Harkness compares Repsold's method of eliminating the flexure, by interchanging the object and eye-end of the telescope, with Bessel's method of attaining the same result by observing a star both directly and by reflection with the clamp successively west and east, demonstrating the superiority of the latter method, which appears to be the most satisfactory procedure hitherto devised for freeing an observed declination from the effect of flexure.

Prof. Harkness shows that when there are terms in the flexure depending on multiples of the zenith distance, they cannot in general be completely eliminated, and therefore that star-places derived from observations made with a single instrument are likely to be affected by systematic errors, which will appear when the work of different instruments is compared together. The detection and elimination of such errors can probably, Prof. Harkness thinks, be greatly facilitated by the use of equal altitude instruments of the zenith telescope class, which are so remarkably free from systematic errors.

THE SPECTRUM OF FABRY'S COMET.—M. Trépied having frequently observed the spectrum of this comet since April 7, gives (*Comptes rendus*, vol. cii., No. 18) the following account of it. The three usual cometary bands were seen, and as the brightness of the spectrum allowed a fairly narrow slit, 0·2 mm., to be used, the coincidence of these bands with those of the hydrocarbon spectrum could be very satisfactorily verified. Besides these bands there was also a continuous spectrum, but the remarkable feature of the case was that although the nucleus, which was very distinct and of a truly stellar appearance, appeared very bright as compared with the neighbouring portions of the coma, the band spectrum given by these latter and by the tail was much more brilliant than the continuous spectrum of the nucleus. This circumstance, which was also observed by MM. Thollon and Perrotin at Nice, had been remarked by M. Trépied in Encke's comet last year. He is therefore led to conclude that there is a predominance of gaseous elements in both these comets, and that, further, the relative brilliance of the nucleus of a comet is not necessarily in accord with the degree of condensation of the cometary matter.

On April 14 the bright bands could be easily detected in the spectrum of the tail to a distance of 20′ from the nucleus. The total length of the tail was then more than 3°.

TWO NEW COMETS.—Mr. W. H. Brooks, Red House Observatory, Phelps, New York, discovered two new comets in the last week of April, the first on April 27, the second on April 30. The former is described by M. Bigourdan as being on May 1 a round nebulous object, about 2′ in diameter, brighter towards the centre, but without a nucleus. The existence of a very faint nucleus was, however, suspected on the following night. On May 6 Lieut.-Col. Tupman estimated the comet as being of the 8th magnitude. Dr. H. Kreutz has computed the following elements and ephemeris for it:—

T = 1886 June 6·9585 Berlin M.T.

$$\left.\begin{aligned} \omega &= 202^\circ\ 55'\!\cdot\!68 \\ ☊ &= 191\ 48\!\cdot\!58 \\ i &= 87\ 33\!\cdot\!03 \end{aligned}\right\}\text{ Mean Eq. 1886·0}$$

$\log q$ = 9·40752

Ephemeris for Berlin Midnight

1886	R.A. h. m. s.	Decl. ° ′	Log Δ	Brightness
May 13	2 9 38	51 43·7 N.	0·1062	2·2
17	2 30 25	47 52·2	0996	2·9
21	2 49 32	43 25·9	0931	4·1
25	3 7 48	38 17·2	0865	6·2
29	3 26 26	32 15·3 N.	0·0794	10·0

The brightness on April 29 is taken as unity.

The second comet is described (*Astr. Nach.* No. 2728) by the Baron von Engelhardt as being very bright on May 3, although the evening was misty. The comet was visible in a bright field, and showed a circular nucleus, from whence proceeded a brighter offshoot, 2′ in length, in the direction of the axis of the tail. The tail was 8′ in length and very bright, narrow at first, but broadening by degrees, and curved with the convex side towards the north. A secondary tail, 6′ in length, faint, and bending towards the south, forked off from the principal tail about 6′ from the nucleus. The following elements and ephemeris are by Dr. E. Lamp:—

T = 1886 May 4·13040 Berlin M.T.

$$\left.\begin{aligned} \omega &= 37^\circ\ 50'\!\cdot\!15 \\ ☊ &= 287\ 22\!\cdot\!88 \\ i &= 99\ 47\!\cdot\!53 \end{aligned}\right\}\text{ Mean Eq. 1886·0.}$$

$\log q$ = 9·92518

Error of middle place ($O - C$).

$d\lambda = +\ 0''\!\cdot\!19 \qquad d\beta = -\ 0''\!\cdot\!02$

Ephemeris for Berlin Midnight

1886	R.A. h. m. s.	Decl. ° ′	Log r	Log Δ	Brightness
May 12	23 52 46	47 23·0 N.	9·9326	9·9924	1·0
14	0 5 38	51 23·8	9·9364	9·9980	0·9
16	0 20 38	55 10·5	9·9410	0·0056	0·9
18	0 38 9	58 39·8	9·9462	0·0149	0·8
20	0 58 38	61 49·5	9·9520	0·0255	0·8
22	1 22 15	64 35·6	9·9583	0·0373	0·7
24	1 49 21	66 55·8	9·9652	0·0501	0·6
26	2 19 47	68 48·4	9·9725	0·0636	0·5
28	2 52 55	70 11·7	9·9802	0·0775	0·5
30	3 27 37	71 5·8 N.	9·9881	0·0918	0·4

The brightness on April 30 is taken as unity.

NEW MINOR PLANET.—A new minor planet, No. 258, was discovered on May 4 by Dr. Luther at Dusseldorf, R.A. 15h. 20m., Decl. 9° 31′ S.; daily motion, R.A. − 48s., Decl. + 7′; mag. 11.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 MAY 16-22

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on May 16

Sun rises, 4h. 8m.; souths, 11h. 56m. 9·3s.; sets, 19h. 44m.; decl. on meridian, 19° 9′ N.: Sidereal Time at Sunset, 11h. 22m.

Moon (Full on May 18) rises, 17h. 51m.; souths, 23h. 5m.; sets, 4h. 10m.*; decl. on meridian, 11° 22′ S.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	3 34	10 24	17 14	9 7 N.
Venus	2 48	9 4	15 20	2 29 N.
Mars	12 23	19 13	2 3*	8 57 N.
Jupiter	13 52	20 10	2 28*	2 53 N.
Saturn	6 37	14 49	23 1	22 49 N.

* Indicates that the setting is that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich)

May	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
17	η Libræ	6	22 16	23 32	64 240
19	24 Scorpii	5	0 37	near approach	167 —
21	d Sagittarii	5	22 1	22 57	19 266
22	B.A.C. 6658	6	5 2	near approach	206 —

Variable Stars

Star	R.A. h. m.	Decl. ° ′		h. m.
ζ Geminorum	6 57·4	20 44 N.	May 20,	2 20 m
V Virginis	13 21·9	2 35 S.	,, 17,	M
δ Libræ	14 54·9	8 4 S.	,, 16,	2 8 m
U Ophiuchi	17 10·8	1 20 N.	,, 16,	0 44 m
			,, 21,	1 30 m
X Sagittarii	17 40·4	27 47 S.	,, 19,	2 25 m
			,, 22,	0 0 M
W Sagittarii	17 57·8	29 35 S.	,, 21,	21 40 M
β Lyræ	18 45·9	33 14 N.	,, 16,	0 0 m
S Vulpeculæ	19 43·7	27 0 N.	,, 17,	m
η Aquilæ	19 46·7	0 7 N.	,, 19,	0 0 m
δ Cephei	22 24·9	57 50 N.	,, 20,	21 35 m

M signifies maximum; m minimum.

Positions of the Comet Barnard (for Berlin Midnight)

May	R.A. h. m. s.	Decl. ° ′	Log. Δ	Brightness
16	2 20 49	28 0 N.	9·682	284
18	2 35 41	23 17	9·637	318
20	2 53 8	17 23	9·596	349

GEOGRAPHICAL NOTES

AMONGST the members of the mission proceeding from India to Tibet, under the charge of Mr. Colman Macaulay, are Col. Tanner, surveyor, Dr. Oldham, geologist, and Dr. Cunningham, naturalist. The expedition will leave Darjeeling about the end of the present month, and, marching through independent Sikkim, will cross the Jalepla Pass into Tibet. Its destination is Lhassa, the capital. Once only has this city been visited by an Englishman, Thomas Manning, and practically the whole route lies through a *terra incognita*. As Mr. Macaulay bears letters from the Chinese authorities, for which he made a special journey to Pekin last year, it is not anticipated that he will meet with any obstacles on his way to, or during his stay on, "the roof of the world." The three scientific members of his mission will find abundance of work to do, and the news of the progress of the expedition may be looked for with interest.

THE new number of the *Journal* of the Royal Asiatic Society (vol. xviii., part 2) contains an interesting article by Mr. Morison, of Tiflis, on the geographical distribution of Turki languages. The following is a summary. Dividing Turki into five sub-branches—Turki proper, Nogai, Uigur, Khirghiz, and Yakut—he states that the various subdivisions of, first, Turki proper, are spoken by the ruling class of the Ottoman Empire and the inhabitants of Asia Minor, in the Governments of Nijni Novgorod, Kasan, Simbirsk, Viatka, and Orenburg, in Trans-Caucasia, and North-Western Persia ; the Nogai in Bessarabia, the Crimea, Cis-Caucasia, the Volga Delta, North-Eastern Daghestan, Terek Valley, the north-western shore of the Caspian, the Governments of Kasan and Simbirsk, Astrakan, Orenburg, and Ufa ; the Uigur in Yarkhand and Chinese Tartary, the country of the Tekke, Zarafshan Valley, and generally in Central Turkestan, in the Khanate and Desert of Khiva and south of the Aral Sea, and in Kuldja ; the Kirghiz from the Volga to the confines of Manchuria, but most compact in South-Western Siberia ; and the Yakut in North-Eastern Siberia and on the northern slopes of Mount Sayan. Broadly speaking, says Mr. Morison, the Ugro-Altaic languages, of which Turki is one, are spoken over a region extending through more than 100° of longitude, from the shores of the Adriatic to the Great Wall of China and the plateau of Tibet, and through 35° of latitude, from the frozen steppes of Samoyede and Yakut to the plains of Northern Persia and the head-waters of the Indus. The Turki alone, according to the figures given, is spoken, in one or other of its various forms, by more than 20,000,000 of people.

THE *Proceedings* of the Royal Geographical Society for May contains a paper by Mr. Carles on his recent journeys in Corea, accompanied by a very useful map of the peninsula. Some account of these journeys has already appeared in Parliamentary Blue-Books, but much is added in the present paper. The writer refers to the many different types found amongst the Coreans of the present day ; the facial characteristics of the people greatly resemble those of the Manchus, but Jews, Japanese, and Caucasians appear to be universally represented. There is also a curious reference to evidence of some forms of religion other than those imported from China in the *miriok*, or half-length human figures carved in stone. Mr. Needham also contributes an account of an excursion to the Abor Hills from Sadiya in Upper Assam.

BARON MIKLUHO-MACLAY has just returned to Odessa from his journey to New Guinea, which has lasted two years. He has brought a large collection of rare fishes, lizards, snakes, insects, and so on, packed in twenty-two boxes.

ANOTHER Russian traveller, M. Goudatti, the Secretary of the Moscow Society of Friends of Natural Science, who has also just returned from his journey to the north of Siberia, gives a curious account of his failure to accomplish his purpose. The Ostiaks and Samoyedes took him for a Government official on a recruiting mission, especially when he attempted to measure their heads, and took notes in his note-book. Finally the book was stolen, and all the results of his efforts lost.

HERR RADDE, who had started in January last with a scientific expedition from Tiflis to the Transcaspian region, writes from Askabad lately that this spring was very unfavourable for his researches, being three to four months later than usual. Therefore up to the middle of April he had not succeeded in collecting more than 35 species of plants and about 150 birds. Amongst these latter there is an interesting novelty, the *Picus sindiacus*, a pretty bird living in the high shrubs of *Tamarix*. The explorer intends to proceed during the present month to the mountain region between the Murghab and Tejen, and to return to Askabad through Sarakhs.

THE May number of the *Scottish Geographical Magazine* has an interesting article by Mr. Tripp on the physical configuration and rainfall of South Africa, with notes on its geology, diamond and coal-fields, and forests. The paper is accompanied by two maps showing contours and mean annual rainfall. A note by M. Dingelstedt on geographical education in the schools of the Caucasus shows that in Russia primary instruction in geography is as defective as in England. It is not made attractive, the writer complains ; it only taxes the memory ; the text-books are written to match, and few teachers are equal to the task of interesting their pupils in the subject. There are some interesting notes on the place-names of Kinross-shire by Mr. Liddall, and on the seaboard of Aberdeenshire, by Mr. Ferguson. The geographical notes are particularly copious and comprehensive.

THE current number (Bd. xiii. No. 4) of the *Verhandlungen* of the Berlin Geographical Society contains only one paper—a lecture by Dr. Naumann on the Japanese Islands and their inhabitants. The *Zeitschrift* of the same Society (Bd. xxi. Heft 2) is mainly occupied by a paper of Dr. Schweinfurth's on a journey which be made in the "region of depression" around Fayoum at the commencement of the present year. It is accompanied by a map, and fills 53 of the 66 pages forming the number. There is a short paper of great interest on the Maori population of New Zealand, based on the last census of that colony. The writer (who does not give his name) discusses the causes of the dying out of the race, and also the attitude of the Colonial Government towards the Maories. There is a note from Prof. Kunze on the climatology of South America, and, lastly, a long list of barometrical observations by Lieut. François in the Kassai region.

THE SUN AND STARS[1]

VI.

Summary of Results

IN what has gone before we have found that the prominences, and the spots, have special spectra unlike the ordinary spectrum of the sun, and unlike the spectra of the chemical elements.

Further, we know that when we proceed outwards to the spectra of the inner and outer corona we find ourselves very little better off, for, with the exception of hydrogen, there is no substance which is perfectly familiar to us ; and finally, when we come to study the association of phenomena on the sun, we find that, exactly while the spots and prominences give us the greatest divergences from terrestrial conditions, solar facts indicate that these phenomena are allied in the most close and obviously important manner. We must henceforth consider that the spots and the metallic prominences and the faculæ represent different indications of the same solar action.

Now, to continue this part of the inquiry is fundamental for us. It is almost impossible to see a large spot at the edge of the sun, which is the place for observing it best, without finding this downrush towards the photosphere answered, so to speak, by an uprush from below the photosphere—without finding this downrush of cool, absorbing, dark-and-widened-line-producing material, re-echoed by an uprush of bright-lined substance.

There is one word which expresses, as well as anything I can think of, the impression which is made on one by the phenomena. There is a *splash*. Imagine an enormous cauldron of liquid iron, as hot as you like. Play some water into it from a hose ; there will be a splash. The water, of course,

[1] A Course of Lectures to Working Men delivered by J. Norman Lockyer, F.R.S., at the Museum of Practical Geology. Revised from shorthand notes. Continued from vol. xxxiii. p. 543.

would be very violently heated; we probably might get some explosions, and as the result of these explosions some liquid iron might be carried with the liquid water which has entered into the liquid iron here and there. The metallic prominences always are close to spots. They almost always follow them like the faculæ. I might have told you, in fact, while talking of this, that of 1100 cases in which spots and faculæ have been observed together, in 581 the faculæ were to the left of, or behind, the spots. Only in 45 were they on the right or in front. We shall see the importance of this by and by.

If we can invariably, as we do, associate the descent of material which, though we do not see it falling, we know is there, and that it is relatively cool—*if we can associate these descending absorption-phenomena with a subsequent upward splash*, we must look upon the most intensely active prominences as being return upward currents, though in some cases it may be that what we see as the spectrum of a prominence at the limb is, in part, that of the vapour descending to form a spot.

The Sunspot Period

The next thing we have to do is to discuss the periodicity of the various solar phenomena, to which attention has already been directed. It is worth while again to refer to the two very interesting and important curves in which Prof. Spörer has recorded the results of his own observations.

When the spots are at their fewest the small number we do see begin in a high latitude N. or S., from 30° to 35°; as the spots increase in number and activity we get, at the maximum sunspot period, the chief appearances observed in middle latitudes—about lat. 18°; and then the mean latitude of the spot zone still gets lower and lower, until at the next sunspot minimum we get two systems of spots—one of them, lowest in latitude (about 8° N. and S.), ending the first cycle, and another in latitude 30° beginning the next. These are the salient features of the periodicity to which we have now to confine our attention.

It has been previously pointed out that there are other periodicities with a much shorter period than eleven years; certain changes are seen to occur among the quiet prominences. Still this is the main periodicity with which we are familiar on the sun; and what we have now to do is to endeavour to see whether we can follow all the phenomena in their changes.

The two last maxima occurred in the year 1871 and eleven years afterwards in 1882 and some time after that year. At those times we got the greatest amount of spotted area and the most intense solar action. Similarly the two last periods of minimum activity were in 1867 and eleven years afterwards in 1878.

Now, in order to investigate this question in the most satisfactory manner, I think, and I doubt not you will agree with me, that we should begin with the simplest case.

The Minimum

The simplest case is evidently that in which the sun is quietest. At first sight it may appear a little hazardous to talk about the sun being at its quietest; but we know, as a matter of fact, that there is a tremendous difference at different times in the solar activity along the lines to which reference has been made.

But in the light of what has already been stated let us suppose the sun at its quietest, what phenomena shall we see?

There will be very few of the ordinary tree-like prominences anywhere on the sun, and especially will there be a dearth of them near the poles and near the equator.

There will be faculæ, but the faculæ will be dim; they will not present the bright appearance they generally do, and what there are will be mostly confined to the regions of latitude comprised between 20° N. and 20° S.

If by means of a spectroscope we attempt to determine the chemical materials in the chromosphere, we shall find just those five lines only to which we have referred in the spectrum as ordinarily visible—that is, four lines of hydrogen, and one line named D_3.

Practically speaking, there will be no spots visible upon the disk; the disk will appear to be perfectly pure, almost equally illuminated throughout, barring always the darkening towards the limb.

As there are no spots, or only very small ones in high latitudes, there will be, we can easily understand from what has gone before, no metallic prominences whatever. The spectroscope searching right round the limb of the sun will gather no indications of violent action—no region giving us many lines—nothing but that simple spectrum of hydrogen to which I have already referred.

What, then, is the appearance put on by the corona if we can manage to get an idea of a corona at the minimum sunspot period? We see, the moment that question is suggested, how excessively important it is that all eclipses should be observed, whether they occur at the maximum or the minimum of the solar activity. Fortunately, since the year 1860 these wonderful phenomena have been observed with more or less diligence; and since the year 1871—that is fifteen years ago now—with few exceptions, not only have those eclipses been observed by the eye with great care, but photographs of the extremest value have been obtained.

Unfortunately, that first minimum to which I have referred—the minimum in 1867—took place practically before the general introduction of this perfect photographic record of eclipses; and there is no good photograph extant of that eclipse; but fortunately, good photographs were secured of the eclipse of 1878. You can imagine our American cousins did not let an opportunity like that of advancing knowledge slip; and the result was that the whole land along the line of totality bristled with telescopes and cameras, which did their work in an admirable way. So that in the eclipse of 1878 we did get a photographic record, that is to say, an absolutely trustworthy record, of the appearance presented by the sun's corona at the minimum sunspot period. If it were not so, I should have hesitated to show you the drawing made in 1867; but I think you will say, when I show you these records together, that the drawing in 1867 is so like the photograph taken in 1878 that on that ground alone it is worthy of extreme confidence; and if we can accord such confidence to it, we arrive at the very important conclusion that at two different sunspot minima the appearances presented by the corona were very much alike indeed.

At the minimum period the chief feature is a tremendous extension of the corona in the direction of the solar equator. At both the poles, north and south, there is a wonderful curving

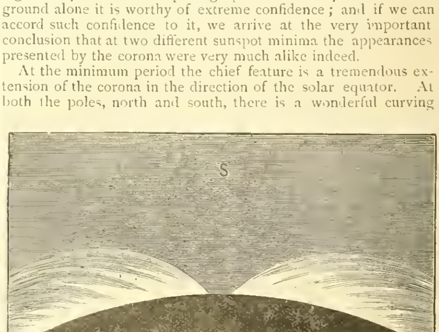

Fig. 10.—Outcurvings at the solar poles (1878).

right and left; this outcurving having been one of the most exquisite things which it is possible to imagine.

There is further evidence indicating that the equatorial extension on the photograph may only after all have been a part of a much more extended phenomenon, one going to almost incredible distances—considering it as a solar atmosphere—from the sun itself.

It has already been stated that at that eclipse one observer took extreme precautions to guard his eyes from being fatigued by the light of the inner corona, which sometimes is so bright that observers have mistaken it for the limb of the sun itself. What this gentleman, Prof. Newcomb did, was to erect a screen which covered the moon and a space 12′ high round it. The result was, that as soon as he took his station at the commencement of totality, he saw a tremendous extension of the sun's equator on both sides the dark moon, the extension being greater than that recorded in the photograph. It does not follow that the photograph gives us the totality of the extension; it may be that the extended portions may have been so delicately illuminated, that they would not impress their image on the photographic plate in the time during which that plate was exposed, or that the light itself is poor in blue rays. So considerable was this extension, amounting to six or seven diameters of the dark moon, which practically may be taken to be the same as that of the sun behind it, that Prof. Newcomb had at once suggested to him the zodiacal light. It happened that while this eclipse was

being observed by Prof. Newcomb and myself—we were practically close together at a height of 7000 feet—other observers were viewing the eclipse from Pike's Peak, some few hundred miles away, at a height of 13,000 feet. You can imagine the purity of the air at that height; there was not too much of it—so little in fact that some observers had to go down. These saw the corona very well indeed; and one or two observers, without taking the precaution of putting up a screen, saw an extension almost comparable with that recorded by Prof. Newcomb.

That, then, we must take to be the undoubted result arrived at during the eclipse of 1878, which happened at the last sunspot minimum. We have a tremendous equatorial extension; that is the great feature, and it is proved by photographs.

The drawing made in 1867 gives us the same result. We again get the equatorial extension east and west, and the wonderful outcurving right and left from the sun's poles.

Hence, then, we must associate a corona of that kind, *i.e.* having a considerable equatorial extension, with that quiet condition of things at the sun, during which metallic prominences, ordinary prominences, faculæ, and spots show a minimum of activity.

You will remember that we saw from the sunspot curve that from minimum to maximum it mounts rapidly, reminding one of a steep cliff. We have in fact only three years from minimum to maximum, while we have eight years from maximum to minimum.

The Approach to Maximum

We have then next to consider the solar condition between minimum and maximum. We must suppose ourselves to be half-way up the steep part of the curve that connects the maximum with the minimum. In this case we find a greater activity in all directions. The hydrogen—or the quiet—prominences are more numerous. The faculæ are brighter. If we now examine the chromosphere we find hydrogen and D^3 are not the only constituents—we get those other short lines added of which Prof. Tacchini has given us such a valuable list, among them chiefly being those three lines of magnesium which are designated b^1, b^2, b^4. That is the chemical difference between the chromosphere of the sun at this time, and the first period at which we considered it. The spots also are more numerous, and what spots there are we have in a lower latitude; instead of making their appearance in latitude 35°, they will be nearer latitude 25°—they will have come down 10° from the solar poles towards the equator. These more numerous spots will also be constantly accompanied by metallic prominences, and the number of lines visible as bright lines in these prominences we shall find increasing as the observations are made month after month.

How about the outer atmosphere of the sun? Well, remarkable changes begin to take place in it too. In considering the minimum corona I said nothing about its spectrum, for the reason that I wished that wonderful bilateral and symmetrical and simple form to rivet the attention. But now it is right that I should say that one of the chief changes between this corona as the maximum is approached and the minimum one is not only the change of form to which I shall have to draw attention, but a change in the spectrum. At the minimum sunspot period the corona gives exactly, or very nearly exactly, the same spectrum as the lime-light or a jet of gas—we get very nearly a continuous spectrum. The chief difference between the spectrum of the corona, then, and the spectrum of the gas jet, is that in this continuous corona spectrum certain dark lines will be seen, but no very obvious bright lines are there. We therefore have to come to the conclusion that at the minimum the corona is not chiefly gaseous in its spectrum, but that it consists of solid particles to a very large extent; and that these solid particles are not only competent to reflect light, but that they actually do reflect light coming from the lower portions of the sun; and in that way we account for the presence of the Fraunhofer lines.

But when we come to the second period we are now discussing, these change to a very large extent; the spectrum is no longer continuous; bright lines begin to make their appearance, and with this coming-in of bright lines comes in a greater brilliancy.

And then as to the form. The diagram is copied from a drawing taken in the year 1858, at exactly the right period to illustrate any change which may have taken place on the approach to maximum. Unfortunately it is not a photograph. Those who lecture in this theatre twenty or a hundred years after me will be under many better conditions than we are, because they will have a more complete series of photographs to refer to; but in the absence of photographs we must do the best we can. Strange though the drawing is, it brings together so many features seen in other eclipses, that there is very little doubt that it is near the truth. However that may be, it must be acknowledged that between the last drawing you saw and this

FIG. 20.—Corona of 1878.

there is a most enormous difference. The remarkable point about it is that we have no special feature in the equatorial zone: we get a streamer here with very strange outlines, and we get another there, but the point of this drawing is that we get in middle latitudes, north and south, four wonderful luminous cones, and the amount of light and structure in the corona has increased to such an extent, that that exquisite, that beautiful tracery and double curves—curves east and curves west—we saw at both poles at the minimum, are now hidden in a strong radiance. So much then for the second act, so to speak, in this solar drama.

The Maximum

We next deal with the maximum period when all the solar forces are working full time, and when we get both in prominences and in spots, and indeed in every outcome of action that we can refer to, indications of the most gigantic energies at work, and the most wonderful changes produced; energies and changes displayed from one pole of the sun to the other. When we come to this period of enormous action, we shall find that, although it becomes more general in one sense, it is really more limited in another.

The ordinary prominences, instead of clinging to the equator as they have done previously, are now found to be most frequent at the solar poles; the faculæ are brighter and more spread over the solar surface than they have ever been before. The chromosphere is richer in lines.

The spots occupy broad zones, the mean latitude being in about 18° N. and S. No spots near the poles, no spots near the equator, but spots indicating enormous activity and of enormous area, surrounded by gigantic faculæ, will be seen following each other in these zones. We shall find every one of these indicators of solar activity accompanied by enormous prominences. It is at this time we get the greatest velocities of upthrow in the prominences, and the greatest indications of tremendous downward velocity in the vapours which form the spots. It is at this time we get the spots riddled by bridges of intense brilliancy, full of veiled tints, red, yellow, blue, and violet, and all those other more delicate and beautiful phenomena described by M. Trouvelot and others, to which I have drawn attention.

How about the outer atmosphere of the sun? What has happened to that? Here, fortunately, we have the photographic records of two years—of two maximum years—to study. In these records there is no doubt that we have a thing which is absolutely and truly solar, for the reason that the photograph has undoubtedly, I think, sifted out what may be considered as due to non-solar causes. I say this distinctly, because I was fortunate enough to see both of these eclipses, one in India, and one in Egypt, and certainly there were things which I saw with the

naked eye which one does not see in the photographs. We will consider the eclipse of 1871 first.

We see in a moment that we have something here at the maximum sunspot period different from what we have had before. To compare it with the record of the preceding minima in 1867 and 1878. Instead of having streamers limited to the equator, they exist in high latitudes, and instead of having them limited to four chief maxima, as we had in the year after the minimum in 1868, the energy is now so great that they practically extend to every part of the sun.

The directions of the lines of force, as they may be called, are very various: there are straight rifts; there are curved boundaries; here another streamer is curved bodily, and so we go on. We must always remember that in this photograph what we see is, after all, a projection. We have the spherical moon in front of the spherical sun, from which these streamers project in all longitudes—some straight towards the earth, the tips of which are seen over or under the moon, some more sideways from parts of the sun nearer to or further from the eye than the central section, so that the unravelling of the appearances is very difficult, especially if the eclipse happens when either the sun's north or south pole is tipped towards us or away from us to the greatest extent.

So much for 1871: in another eleven years we have another maximum—that of 1882—an eclipse seen in Egypt. In this case we find the activity more general than in the former one. The top and the bottom of the diagram represent the north and the south poles of the sun as before; but we see now that the streamers are more broken up, and furthermore that the rifts visible round the north and south pole at the previous maximum are entirely covered up—not that the rifts were not there, but that one could not see them in consequence of the extreme brilliancy of the streamers that were thrown towards the eye from the sun between us and the plane passing through the solar poles.

Independently of that, it is easy to recognise that there is a tremendous family likeness between the photographs taken at both maxima, whereas there is the greatest possible difference between either of them and the drawings or photographs taken at the minimum sunspot period. If we accept that, that is a very great step gained.

After Maximum

We need not, after what has gone before, take up any more time, which is short, by discussing the gradual descent to minimum. I say the *gradual* descent because we know there are more years consumed in going from maximum to minimum than from minimum to maximum.

Of course all the various energies slacken down, the mean latitude of the spots and metallic prominences still getting lower till they reach lat. 8° N. and S.; then another series of spots breaks out in lat. 35° N. and S., and the whole story begins anew.

Summary

Now let us deal with the results we have arrived at. At the maximum period the continuous spectrum of the corona gives way almost entirely to a spectrum of bright lines. When I say gives way almost entirely, I mean so far as this: the striking thing when you observe the spectrum of the corona at the maximum period is a series of brilliant lines, or of brilliant circles, according as we use a slit, or simply look through a prism, and the brilliancy of the spectrum seen between these lines or rings is small compared with the brilliancy of the lines or rings. That indicates that the temperature of the gases in the corona is greater than the temperature of the other substances, and of course is very much higher than it is during the time of the minimum, when the gases do not make themselves visible, and, as I said before, the chief spectroscopic effect obtained is the continuous spectrum with dark lines here and there, showing that some part of the light is derived from cooled solid particles which can and do reflect light from the subjacent photosphere.

To deal with results, and to bring them together as sharply as may be, we find, first, that the dimness of the light and absence of bright lines at the minimum shows that the outer atmosphere of the sun is cooler at the minimum sunspot period. When I was in America in 1878, at the period of minimum to which we have referred, I saw at once that the corona was not anything like so brilliant as I had seen it previously in 1871 in India. Eventually, when the observations came to be discussed, the conclusion arrived at was that the brilliancy was not one-seventh of what it was at the previous maximum. There is a very considerable difference which no one can mistake who observes one eclipse after another.

Secondly, when the corona is thus cooler, and therefore dimmer, an extension in the plane of the sun's equator is seen. A question arises here whether this extension is not seen at the maximum because the eye is so much affected by the very brilliant corona? That is a subject which will require to be investigated in subsequent eclipses.

Thirdly, there are plenty of minor prominences at the minimum sunspot period; there are no spots, or very few.

Fourthly, the lower temperature, and therefore relative quiescence, of the solar atmosphere seems to depend on the absence of spots. That is an important matter; and the point I wish to make is this: the quietude cannot depend on the absence of prominences, because they are there—not so many of them, but still some prominences.

Fifthly, when the spots begin in these higher latitudes, 30° or 35°, as we have seen, we get the first brightening of the corona.

Sixthly, the coronal streamers follow the spots—by which I mean that the cones and coronal streamers put on their greatest intensity according as the spots have moved nearer to the equator. When we have the minimum sunspot period, you can hardly call that equatorial extension a streamer at all, because it is so very dim; and further, I take it, it is really of a different nature and origin.

The Circulation in the Sun's Atmosphere

If we make an attempt to discuss the circulation of the atmosphere, a question which we acknowledge to be an extremely difficult one, we must bear in mind the enormous difference between solar and terrestrial conditions. When a portion of the earth's surface is heated in a whole zone—as the equator is in the tropics—by the sun, you see the heat is outside, an ascending current is formed, and winds from north to south set in. For instance, if we consider the equator, and suppose the sun to be over it, we get the earth's atmosphere over that region more highly heated than those parts of the atmosphere near either pole; and the result is, we get an indraught current in that way, both from the northern and southern hemispheres. In consequence of these two currents meeting and beginning their ascent at some distance from the equator, we get a belt of calms, of reduced pressure, and we get almost perpetual rains.

Now, you see. that is all very well as a piece of terrestrial meteorology, but it is of no value to us from the solar point of view, unless it sets us thinking how very different the conditions are.

The sun cannot be heated from the outside. We have seen, in fact, that one chief point about the sun is that it is cooled on the outside; that masses of gas going up to tremendous altitudes eventually arrive where the atmosphere is cold and quiet, and where they again take on the solid or liquid forms, when they begin to go down again. Now the sun, if it is heated at all, must be heated from the inside. What do I mean by the inside? I mean—seeing that the phenomena we have been discussing in these lectures take place outside the photosphere—that the inside must be something below the level of the photosphere. Now what form must that heat take? It will take, as undoubtedly we see in the metallic prominences it does take, the form of the ejection of the tremendously brilliant and incandescent vapours. How are these produced? Something must produce them; they do not ascend of their own sweet will, or they would not come up so locally as we see them.

We get this fact most indisputably, which I hope I have been able to make quite clear, that these ascents of vapours from below the spot region always accompany the spots, and they always follow the spots in time. Then is it not reasonable to suppose they are produced by the spots? You remember I objected to the word "eruption" in connection with these prominences. I do not so much object to the word "explosion," for I cannot understand how if you get twenty million tons of meteorites falling down in a particular latitude of the sun, and plunging into the photosphere—I do not understand how there must not follow after that the most gigantic and terrific explosion, driving heated gases many hundreds and thousands of miles into the upper air along the line of least resistance, and disturbing the photosphere for months afterwards. Now that really does seem to be the plain English of what happens.

I have told you that in the origin of the spots the first disturbance is the formation of a few little openings probably by the advanced guard of the falling solid material. In a few days, by the continuous downpours, these develop into a spot. This spot is followed by a metallic prominence sending up the masses of gas at the rate, may be, of 250 miles a second—a rate Prof. Young has observed; and after that the faculæ appear. I throw that idea out because the greatest prominences are associated with the greatest spots; the spots begin the disturbance, and the energies radiate from the point where we first see the disturbance, which, I repeat, the spot begins.

We see, then, that on the sun the action will be almost just the opposite of what it is on the earth. We get first of all the descent of cooled matter on to the part of the sun where the disturbance begins.

Here we get ascent in consequence of greater heat outside. At the sun the greater heat inside the sun is liberated by the splash and explosion of spot-producing material.

Now, when the material falls in the way we have indicated, we shall get, if the idea is true, a considerable temperature in the region above the fall accompanying the return current in the shape of prominences. We may probably also get a current in the lower atmosphere set up towards the north and towards the south, and another thing we shall certainly get will be a tremendous brightening of this part of the solar atmosphere.

One of the great differences between one part of the solar atmosphere and another depends upon its temperature; so that you must imagine that the moment we get any great change in the temperature of any part of the atmosphere we must get a great change in its brilliancy, even in the higher regions: this may explain the streamers.

If there are these lower currents towards the poles there will probably be upper currents away from them which may in some way locate spot-forming material over the spot zones. On this subject, however, which, though one of the most important in solar physics, is one in which we see our way least clearly, I have not time to enter. J. NORMAN LOCKYER

(*To be continued.*)

THE INSTITUTION OF MECHANICAL ENGINEERS

THE Institution of Mechanical Engineers held their annual meeting, under the presidency of Mr. Jeremiah Head, at the Theatre of the Institution of Civil Engineers, on the 6th and 7th inst.

Mr. T. B. Lightfoot read a paper on refrigerating and ice-making machinery and appliances. He commenced by describing a complete refrigerating machine as an apparatus by which heat is abstracted, in combination either with some system for renewing the heat-absorbing agent, or, as is more usually the case, with a contrivance by which the abstracted heat is rejected and the agent is restored to a condition in which it can again be employed for cooling-purposes.

The first method by which heat is abstracted by the rapid fusion of a solid is probably the oldest. It depends upon the very strong tendency of mixtures of certain salts with water or acids, and of some salts with ice—which form liquids whose freezing-points are below the original temperatures of the mixtures—to pass into the liquid form; heat is absorbed more quickly than it can be supplied from without, and the temperature consequently falls. This method has been mainly employed for domestic and laboratory purposes.

When heat is abstracted by the second method, that is, by the evaporation of a more or less volatile liquid, other things being equal, the liquid with the highest latent heat will be the best refrigerant, because for a given abstraction of heat, the least weight of liquid will be required, and therefore the power expended in working the machine will be the least. There are four different kinds of processes employed.

The first, in which the refrigerating agent is rejected with the heat it has acquired, is generally known as the vacuum process. Water, the only agent cheap enough to be employed, must be reduced to a pressure below 0·089 lb. per square inch, which is the pressure of water-vapour at the temperature of melting ice. A vacuum-pump is employed, combined with a vessel containing strong sulphuric acid, for absorbing the vapour from the air drawn over, and so assisting the pump. Lately an improvement has been effected in this process by the employment of a pump with two cylinders and intermediate condenser, the water being admitted to the ice-forming vessels in fine streams, so as to offer a large surface for evaporation. The second, or compression-process, is used with liquids whose vapours condense under pressure at ordinary temperatures. The first apparatus employed, though in some respects crude, is yet the parent of all compression-machines used at the present time, the improvements being generally in matters of constructive detail. The water to be frozen was placed in a jacketed copper pan, the jacket being partially filled with the volatile liquid, and carefully protected on the outside with a covering of non-conducting material. A pump drew off the vapour from the jacket, and delivered it compressed into a worm, around which cooling water was circulated, the pressure being such as to cause liquefaction. The liquid collected at the bottom of the worm, and returned to the jacket through a pipe, to be again evaporated. Most modern machines comprise a refrigerator, a water-jacketed pump, a condenser, and ice-making tanks containing moulds or cells, around which brine cooled to a low temperature in the refrigerator is circulated by means of a pump. The working pressure in the refrigerator depends upon the reduction in temperature desired, the higher the pressure the greater being the work that can be got out of any given capacity of pump. The liquefying pressure in the condenser depends on the temperature of the cooling water, and on the quantity that is passed through in relation to the quantity of heat carried away; this pressure determines the mechanical work to be expended. To produce transparent ice, the water has to be agitated during freezing, so as to allow the air to escape. Various refrigerating media have been used, such as ether, sulphur dioxide, and anhydrous ammonia. The third is known as the absorption process: the principle employed is chemical or physical rather than mechanical, and depends on the fact that many vapours of low boiling-point are readily absorbed by water, but can be separated again by the application of heat to the mixed liquid. Taking advantage of the fact that two vapours, when mixed, can be separated by means of fractional condensation, an absorption machine has been brought out in which the distillate was very nearly anhydrous. Ordinary liquid ammonia of commerce was heated, and a mixed vapour of ammonia and water was driven off. By means of vessels termed the analyser and the rectifier, the bulk of the water was condensed at a comparatively high temperature and run back to the generator, while the ammonia passed into a condenser, and there assumed a liquid form. The nearly anhydrous liquid was then evaporated in the refrigerator in the ordinary way; but, instead of the vapour being drawn off by a pump, it was absorbed by cold water or weak liquor in a vessel called an absorber, which was in communication with the refrigerator, and the strong liquor thus formed was pumped back to the generator and used over again. In the fourth, which is known as the binary absorption system, liquefaction of the refrigerating agent is brought about partly by mechanical compression and partly by absorption; or else the refrigerating agent itself is a compound of two liquids, one of which liquefies at a comparatively low pressure, and then takes the other into solution by absorption. An interesting application of this system has been recently made by Raoul Pictet, who found that, by combining carbon dioxide and sulphur dioxide, he could obtain a liquid whose vapour-tensions were not only very much less that those of carbon dioxide, but were actually below those of pure sulphur dioxide at temperatures above 78° Fahr. This very remarkable and unlooked-for result may open up the way for greater economy in ice-production.

The third method is that in which machinery is used by which gas is compressed, partially cooled while under compression, and further cooled by subsequent expansion in the performance of work, the cooled gas being afterwards used for abstracting heat. This method has been much employed of late years, under the title of "Cold-air machines" for the preservation of meat and other perishable food. The author has designed machinery of this class, in which a weight of 1000 lbs. of air per hour can be reduced from 60° above to 80° below zero Fahrenheit, with cooling water at 60° F., with the expenditure of about 18 indicated horse-power. The air after being compressed in the compresser passes to the coolers, which consist of a couple of vessels containing tubes, through which water is circulated by a pump. The compressed air passes through one cooler and returns through the second, being cooled to within 5° or 6° of the initial temperature of the cooling water, which circulates in a direction opposite to that of the air. From the coolers the air passes to

the expansion cylinder, and after performing work upon the piston, and returning about 60 per cent. of the power expended in its compression, it is exhausted, having been cooled down from 70° above to 90° below zero Fahr. Besides its application to the importation of dead meat, live cattle, &c., an interesting application was made last year in the construction of a tunnel through a hill in Stockholm, in the excavation of which, some running ground was met with, consisting of gravel mixed with clay and water, which it was determined to freeze. The innermost end of the tunnel next the face was formed into a freezing-chamber by means of partition walls, which were made of a double layer of wood filled in between with charcoal. The temperature of the freezing-chamber was generally from 6° to 15° below zero Fahr. after twelve hours' running, but soon rose to freezing-point when the men began to work. The tunnel was driven through its length of 80 feet with entire success, the daily progress averaging about 1 foot.

A paper on the distribution of the wheel-load in cycles, illustrated by means of fifty-six figures, was read by Mr. J. Alfred Griffiths. The author gives the following five points of efficiency as applying to cycles generally, viz. reduction of dead weight by the avoidance of very large wheels and of heavy or purely ornamental or unnecessary framing; reduction of resistance by avoidance of very small wheels, and by employment of the best designs in bearings and in driving-mechanism for the diminution of internal friction; perfection of load distribution by entire avoidance of wheels that neither transmit motive-power nor assist the steering, and by concentration of the load on the driving-wheels and reduction of that on the steering-wheels; stability when at rest and when in motion on the straight and round curves, when on a smooth surface and also on a rough and lumpy road, and when the brake is applied either suddenly or gradually; arrangement of load and driving-mechanism so that the distribution of the wheel-load shall be as good on rising or falling gradients as on a level. Tables of dimensions and distribution of wheel-load were appended.

A paper on the raising of the wrecked steamship *Peer of the Realm*, which was effected by the platforming method, and without the aid of divers for any part of the operation, was read by Mr. T. W. Wailes, of Cardiff.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The Council of Somerville Hall have decided to build additional rooms for twenty students. Two Entrance Scholarships of 35*l*. and 40*l*. a year are offered for competition on May 25.

SCIENTIFIC SERIALS

American Journal of Science, March.—Examination of Dr. Croll's hypothesis of geological climates, by Dr. A. Woeikof. The author subjects Dr. Croll's theories to a searching criticism, traversing all his fundamental principles. The statement that the ocean must stand at a higher *mean* temperature than the land is shown to be quite erroneous, the oceans which receive cold currents from Polar seas, and even seas like the Mediterranean and Red Sea, which receive no such currents, having a mean temperature considerably lower than the continents. His whole system of estimating temperature breaks down when seriously tested, the errors being enormous, in some cases upwards of 100° F., or greater than the difference of annual temperature between the equator and the North Pole. His hypotheses, although brilliant and fascinating, cannot be accepted, the main points on which they rest being opposed to the most certain teachings of meteorology, and the whole fabric in its explanation of glaciation and geological climates generally being entirely fallacious.—Tendril movements in *Cucurbita maxima* and *C. Pepo* (concluded), by D. P. Penhallow. The author concludes generally that growth is promoted by an increase of temperature and humidity, but may be retarded by an increase of temperature when other conditions are unfavourable. It is also retarded by excessive transpiration, while the conditions favourable to growth, arising from temperature and humidity, may cause greater growth during the day in opposition to the retarding influence of light. Movements of tendrils and terminal buds, being phenomena of growth, are modified by whatever variations of condition affect growth.—Note on a method of measuring the surface-tension of liquids, by W. F. Magie. It is shown that Poisson's formula determining approximately the height of a large liquid drop standing on a level plate holds good, without any change, for a bubble of air formed in a liquid under a level plate.—Remarks on W. B. Rogers's "Geology of the Virginias" (continued), by J. L. and H. D. Campbell. In this concluding paper the authors deal with the most salient points in the higher formations of the geological system of Virginia and West Virginia. Their remarks, based mainly on personal observation, are intended to be supplementary to Mr. Rogers's comprehensive treatise on the geology of this region.—Observations on the Tertiary of Mississippi and Alabama, with descriptions of new species, by D. W. Langdon. An important result of these observations is the establishment of the relation of the Jackson beds to the Orbitoides limestone and marl beds of Byram Station. The new species, which will be figured in the forthcoming Report of the Geological Survey of Alabama are: *Verticordia eocensis*, apparently the first Verticordia described from this epoch; and *Bulla* (*Haminea*) *aldrichi*, an elongate oval shell resembling *Bulla glaphyra*, Desh.—On the area of Upper Silurian rocks near Cornwall Station, Eastern Central Orange County, New York, by Nelson H. Darton. The paper contains a careful study of the Towsend Iron Mine district and vicinity, where a small mass of Lower Helderberg limestone has been protected from the general denudation by a firm backing of coarse strongly cemented sandstones. The whole forms a ridge running just west of Cornwall Station, its more prominent geological features being shown on the accompanying map.

Rivista Scientifico-Industriale, March 15.—On the crepuscular lights that followed the Krakatão eruption, by Prof. Alessandro Sandrucci. The author surveys with Hirn the various theories propounded to explain this phenomenon, and rejects them all as inadequate, or else based on impossible assumptions. He concludes that for the present the after-glows must be classed with the numerous effects the causes of which have not yet been fathomed.—On the origin of atmospheric electricity, by Prof. Luigi Palmieri. A simple experiment is described, by which it is clearly shown that positive electricity is generated by the moisture of the air, when it becomes condensed by a lowering of the temperature. This conclusion is reconciled with the theory recently advanced by Prof. Edlund, of Stockholm, who argues that the electricity of the air is derived from the earth by the unipolar induction of terrestrial magnetism, while its return to the earth is caused by the condensation of the aqueous vapours, and especially by their conversion into the fluid state.

Rendiconti del Reale Istituto Lombardo, April 1.—Reptiles of the Orta-Kenei district, Adrianople, by Prof. F. Sordelli. This is an account of the collection recently made at the southern foot of the Balkan Range by the Cavaliere Luigi de Magistris, and by him presented to the Civic Museum of Milan. Of over twelve species of reptiles three only are found in the Po Valley, all the rest being of an essentially Eastern character, with a range extending from the Balkan Peninsula to the Iranian Plateau.—Note on a fundamental theorem in the theory of the functions of a complex variable quantity, by G. Morera.—Stratigraphic observations in the province of Avellino, by Prof. T. Taramelli. The paper contains a systematic study of the stratified rocks exposed by the cuttings of the Avellino and Santa-Venere line of railway, and ranging through the whole series from the Lower Chalk through the Eocene, Miocene, and Pliocene, to the more recent Quaternary formations.—Account of a rare and interesting ornithological specimen, by Prof. Pietro Pavesi. The author describes a fine specimen of *Bernicla leucopsis*, Bechst., recently shot at Corana in the Po Valley, and now preserved in the Civic Museum of Pavia.—On the rational curves in a linear space to any number of dimensions, by A. Brambilla.—Meteorological observations made at the Brera Observatory, Milan, during the month of March.

SOCIETIES AND ACADEMIES

LONDON

Zoological Society, May 4.—Prof. W. H. Flower, LL.D., F.R.S., President, in the chair.—Mr. E. L. Layard, F.Z.S., exhibited a fine example of a rare Beetle of the family Cerambycidæ (*Macrotoma heros*), obtained in the Fiji Islands; and a series of specimens of shells of the genus Bulimus from New Caledonia and the adjacent islands.—A letter was read from Mr. F. W. Styan, F.Z.S., relating to some Chinese ani-

mals of which he had lately obtained specimens.—Mr. W. F. Kirby read some remarks on four rare species of Sphingidæ, of which he had lately examined specimens.—Mr. F. E. Beddard read a paper containing observations on the ovarian ovum of Lepidosiren (Protopterus), and described the entrance of follicular cells into the interior of the ovum. It was believed that these cells played an important part in the formation of the yolk.—Mr. Beddard also communicated a paper by Mr. J. T. Cunningham, on the mode of attachment of the ovum of the smelt (*Osmerus eperlanus*).

PARIS

Academy of Sciences, May 3.—M. Jurien de la Gravière, President, in the chair.—On the magnetic principle, by M. Mascart. The author's theoretical studies lead to the general inference that, in a magnetic and isotropic body of any form there are three rectangular directions for which the magnetic force is parallel to the outer field with different coefficients, f, f_1, and f_2. These coefficients possess the same properties as those of a sphere of slightly magnetised anisotropic substance. For steel the mean coefficient of longitudinal magnetic force is much weaker than for soft iron; hence the increased importance of transverse magnetisation.—On the formation of oxalic acid in plants: a study of *Rumex acetosa* (sorrel), by MM. Berthelot and André. The analysis of the dried seeds of this plant yielded 0·05 per cent. of oxalic acid, which is also largely present in the leaves and stalk, but to a less degree in the root.—Remarks on MM. Berthelot and André's communication on the proportion and quantitative analysis of the ammonia present in the ground, which appeared in the last number of the *Comptes rendus*, by M Th. Schlœsing. The author takes exception to MM. Berthelot and André's account of his process for effecting the analysis, and also traverses the statement that arable land, when irrigated, tends constantly to liberate the ammonia of the ammoniacal salts contained in it.—On holmine, or M. Soret's earth X, by M. Lecoq de Boisbaudran. This was a sealed paper recently deposited with the Academy, and now opened at the author's request. It shows that holmine contains at least two metallic radicals.—On dysprosium (symbol Dy), by M. Lecoq de Boisbaudran.—Remarks on a work entitled "Science and Philosophy," presented to the Academy by the author, M. Berthelot.—Observations on the new comet 1886 *a* (Brooks I.), made at the Paris Observatory (equatorial of the West Tower), by M. G. Bigourdan.—On the spectrum of the Fabry Comet, by M. Ch. Trépied. In this comet, as in that of Encke, there appears to be a predominance of the gaseous elements on the one hand, while on the other its spectrum seems to show that the relative brightness of the nucleus bears no necessary relation to the degree of condensation of the cometary matter.—On the density of liquid atmospheric air and its constituent elements, and on the atomic volume of oxygen and nitrogen, by M. S. Wroblewski.—Practical method for the preparation of the Nicol and Foucault prisms (three illustrations), by M. L. Laurent.—On the penetration of light into deep sea-water, by MM. H. Fol and E. Sarasin. From the author's experiments it appears that layers at a depth of 300 metres are illumined every day for the whole time that the sun remains above the horizon; at 350 metres light penetrates for at least eight hours daily. Even after sunset the actinic rays seem to reach considerable depths.—On the combinations of phosphoric acid with titanic acid, zircon, and stannic acid, by MM. P. Hautefeuille and J. Margottet. The general conclusion of the author's researches is that the phosphates of titanic acid, zircon, and stannic acid possess the atomic composition of the phosphate of silica. By employing phosphoric acid as a dissolvent they can be obtained only under the octahedric form, while the phosphate of silica is obtained not only under this but under three other forms incompatible with the first.—Action of vanadic acid on the ammoniacal salts, by M. A. Ditte.—On the constitution of butter, by M. E. Duclaux.—On xenotime, a rare mineral from Minas Geraes, Brazil, by M. H. Gorceix. This substance, which on analysis yields PhO_5, 35·64, $YO + ErO$, 63·75, and insoluble residuum 0·4, appears to be a phosphate of yttria and of a second earth, very probably erbine.—On the endothelium of the inner wall of the vessels in invertebrates, by M. W. Vignal.—On the existence in birds of a series of cephalic ganglia of sympathetic character, corresponding to the segmentary cranial nerves, by M. F. Rochas.—Researches on the structure of the stomach in birds, by M. M. Cazin.—On *Entoniscus mœnadis*, a parasite infesting the *Carcinus mœnas* crab, by M. A. Giard.—On some phenomena connected with the division of the cellular nucleus in plants, by M. L. Guignard.—Remarks on M. Boutroux's recent communication on an acid fermentation of glucose, by M. Maumené. The author shows that this acid differs in no respect from that already determined and described by him in the year 1875, under the name of "hexepic."

BERLIN

Physiological Society, March 26.—Dr. Kossel communicated the results of experiments instituted by Dr. Schotten respecting the cholic acids. As was known, two different nitrogenous acids entered into the composition of the bile, glycocollic acid and taurocholic acid, which broke up respectively into glycocoll and cholalic acid, and into taurin and cholalic acid. The constitution of this azoteless acid, common to both, had not yet been determined. It was, however, known to be different with different animals. In the bile of horned cattle two cholalic acids had been found, distinguished as taurocholic acid and choleic acid. In the bile of swine a third cholalic acid had been found, hypocholalic acid; and in the bile of geese, a fourth, chenocholalic acid. It was probable that still more cholalic acids would be discovered. Dr. Schotten's studies had for their object the elucidation of the constitution of cholalic acid. By heating to 300° C. he was able to split two molecules of water, and to obtain a body of an equal quantity of carbon, a less quantity of hydrogen and of oxygen. By subjection to a still greater degree of heat, from two molecules of the acid a molecule of water was separated, and a substance obtained consisting of two groups of atoms connected by an atom of oxygen. By treatment with anhydrous acetic acid Dr. Schotten established that cholalic acid was both monobasic and monovalent. Finally he investigated the composition of human bile from 350 gall-bladders, with a view to testing the statement that in human bile was contained a peculiar cholalic acid, the anthropocholalic acid of Herr Beyer. Although at first he received the same results, viz. a salt of baryta containing much less carbon than the other cholalic acids, yet subsequently, by continued purification and transcrystallisation of the product, he came to the conviction that in the human bile only the taurocholic acid of horned cattle was present. The results at first obtained of apparently different significance were due to the fact that the soluble barytic cholalate with carbonate of barium very readily formed insoluble double salts which were not easily split.—Dr. Biondi spoke on the intermaxillary bone, and discussed the fact that the doctrine set up by Goethe, that on each side but one intermaxillary bone was developed, namely from the frontal process, while the superior maxilla, on the other hand, was evolved from the maxillary process of the skull, had, in the year 1879, been replaced by a new doctrine advanced by Herr Albrecht. According to this new doctrine two intermaxillary bones were developed on both sides, growing out of the lateral and median frontal process, and then coalescing with the superior maxilla from the maxillary process. According to the older view, at present defended in particular by Dr. Kolliker, the hare-lip originated between superior maxilla and intermaxillary bone. Prof. Albrecht, on the other hand, removed the position of the hare-lip to between median and lateral intermaxillary bone. By way of proof for this latter view, the circumstance was adduced that externally from the fissure an incisor tooth was regularly found. Dr. Biondi had examined a very large number of normal and pathological skulls, and had followed the development in embryos of the facial bones. Like Prof. Albrecht, he regularly found an incisor tooth externally from the hare-lip fissure, and, in the case of embryos, in the intermaxillary bone two points of ossification, whence were developed two separate intermaxillary bones. Between these two were situated the hare-lip fissure and that of the palate. The views of Dr. Biondi and of Prof. Albrecht deviated on the contrary very materially from each other respecting the place where the two intermaxillary bones originated. In accordance with the speaker's views, the superior maxilla and the outer intermaxillary bone developed from the maxillary process, while, on the other hand, the inner intermaxillary bone sprang from the median frontal process. The lateral frontal process did not reach so far down. The hare-lip, in point of fact, therefore, as had been maintained by earlier authorities, was situated between the maxillary and frontal process. The upper lip, in the opinion of the speaker, developed itself, in perfect accordance with the relations obtaining in respect of the superior maxilla and its alveolar margin, out of the maxillary process and the inner frontal process, while the

outer frontal process formed the alæ nasi. In respect of the two intermaxillary bones on each side, the presence of which the speaker assumed along with Prof. Albrecht, Dr Biondi deviated from the latter in so far as that he had found, not an outer and inner intermaxillary bone on each side, but an anterior and posterior. The incisor teeth, as also the supernumerary teeth, developed themselves only in the intermaxillary bone. Dr. Biondi illustrated his address by preparations, drawings, and photographs he produced.—Dr. Pohl-Pincus next gave a supplement to his address on the polarisation colours of the hair of the human head, adducing the reasons which determined him to lay down three types of colouring: the normal, the pathologic, and an intermediate type. It was nevertheless to be understood that a whole series of transitionary hues intervened between the two extremes. He further stated that, in accordance with his experience, hair pathologically changed in its double refraction in consequence of stimulation from inflammation or from psychical excitement was long in returning to its normal condition. The speaker next described two experiments on a frog's heart. When he removed from a frog the anterior part of the cerebrum, under avoidance of heavy bleedings, then set free the heart, and stimulated one or several sensory nerves of the body, he then observed that the systole of the heart was unchanged. During the diastole, however, there appeared on the surface a chess-board-like drawing, and the diastole itself was interrupted in the middle by an intermission. By stimulation of the vagus he was able to overcome this effect of the irritated sensory nerves. The second observation he communicated respected the local diastole which a considerable time previously had been noticed by others as well as by himself. The occurrence of this diastole under local mechanical stimulation of the frog's heart was always a very uncertain one. Dr. Pohl-Pincus had now quite recently found that the local dilatation took place only when the stimulation was given during the second half of the systole. At the beginning of the systole, on the other hand, the stimulation had no effect whatever, and during the diastole it even gave rise to local systole. The effects of the local mechanical stimulation lasted some time, and, besides the local contraction or relaxation, manifested itself in a heightening of the diastolic or systolic state on each occasion at the stimulated spot.

Physical Society, April 2.—Prof. du Bois Reymond spoke on the irreciprocal conduction of electricity found by him in the electrical organ of fishes, and discussed the teleological significance of this property for the capability of fishes to discharge strong electrical currents outwardly (NATURE, vol. xxxiii. p. 407).—Following up the address at the last sitting by Dr. Baur, Dr. Pernet spoke on some other more recent thermostats, in particular on those which effected the regulation by means of vapour pressure. In the closed short leg of a manometer was a small quantity of a fluid readily susceptible of evaporation; above it was placed quicksilver, which also filled the long leg of the manometer. The short leg of the manometer with the fluid referred to lay in the bath, the temperature of which should be kept constant. Did the temperature rise above the desired degree, then the quicksilver of the manometer also mounted in consequence of the pressure of the vapour, and the flow of the gas to the flame got thereby in part shut off. The temperature than sank, the vapour condensed, and the quicksilver in the manometer fell. To render the apparatus available at every over-pressure, the manometer was cut through and connected by a movable piece of tube. As the material best adapted for these flexible connections, the speaker recommended thin steel tube, which was coated over with lead, thereby rendering it easily pliable and not liable to any elastic after-effect. The regulation by means of the long manometer tube was accomplished in an electrical way by an electro-magnet. The details of the arrangement of the thermostats in question were illustrated in part by models, in part by drawings. As fluid for very low temperatures, a mixture of two hydrochloric ethers was used; for higher temperatures, a mixture of ether and alcohol; for temperatures above 100° C., water; and for still higher degrees of temperature, other fluids. With respect to the efficacy of these thermostats, the speaker adduced that he was able to keep a water-bath for a considerable length of time constant to within 0°·02 C.—Dr. König laid before the Society a photometer sent to him from Messrs. Yeates and Son, of Dublin, which apparently far surpassed the Bunsen photometer. It consisted of two quadratic prisms of cast paraffin connected with each other on a longitudinal side. Between these two prisms was placed a silver leaf or a tinfoil leaf. When light from one source fell on the one prism, then it appeared clear white on account of the diffused reflexions. The light was able to penetrate to the other only through the metal sheet. The other prism therefore appeared dark. If a second light was placed on the other side, then the other prism appeared likewise bright. By displacement on a scale the photometer could be brought into the position in which both sides appeared equally bright. The distance from each other of the two sources of light gave in that case the relation to each other of the intensities. The speaker proposed some arrangements which would render this photometer available for coloured light as well. Similar proposals for this purpose had already been made by Dr. Jolly.—Dr. König further made some supplementary communications on the case recently discussed by him of anomalous colour-seeing arising from alcoholism. After determining that the occurrence of a neutral point in the spectrum was a perfectly certain proof that the eyes in question perceived only two fundamental colours, he investigated the extension of the colour curves by the employing of mixed colours, and thereby obtained important results, which he promised to communicate to the Society in a complete form in May or June next.

BOOKS AND PAMPHLETS RECEIVED

"Hand-book of Plant Dissection," by Arthur, Barnes and Coulter (Holt, New York).—"Manual of Physical Geography of Australia," by H. B. de la Poer Wall (Robertson, Melbourne).—"Journal of the Chemical Society," May (Van Voorst).—"Papers and Proceedings of the Royal Society of Tasmania for 1885" (Tasmania).—"Journal of the Royal Agricultural Society of England," April (Murray).—"Proceedings of the Bath Natural History and Antiquarian Field Club," No. 1, vol. vi. (Davies, Bath).—"The Topographic Features of Lake Shores," by G. K. Gilbert (Washington).—"Oils and Varnishes," by J. Cameron (Churchill)—"Year-Book of Scientific and Learned Societies, 1886" (Griffin).—"Notes on Analytical Chemistry," 2nd edition (Churchill).—"Mountain Ascents," by J. Barrow (Low).—"Dogs in Disease," 2nd edition, by Ashmont (Low).—"Bulletin de la Société Impériale des Naturalistes de Moscou," Nos. 1 and 2 (Moscou).—"British Petrography," part 4, by J. J. H. Teall (Watson, Birmingham).—"Missionary Work among the Ojibway Indians," by Rev. E. F. Wilson (S.P.C.K.).—"Our Island Continent," by Dr. J. E. Taylor (S.P.C.K.).—"A Manual of the Diseases of the Elephant," by J. H. Steel (Moore Madras).—"A Treatise on Elementary Statics," by J. Greaves (Macmillan).

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THURSDAY, MAY 20, 1886

THE YEAR-BOOK OF PHARMACY

Year-Book of Pharmacy, 1884. (London: J. and A. Churchill, 1884.)

THIS volume contains the transactions of the British Pharmaceutical Conference at Hastings in August 1884, in addition to abstracts of papers relating to pharmacy, materia medica, and chemistry, from July 1, 1883, to June 30, 1884. The work is of course especially intended for chemists and druggists, but it also contains some information to general readers, and as this might be apt to be overlooked from the special nature of the work, we shall extract from it somewhat more largely than we might otherwise do. Amongst the most striking facts it contains is an observation of Sachs regarding the effect of light on plants, mentioned by Mr. Williams, President of the Conference, in his address. This observation is not only interesting in itself but it appears to give a reason for the rules which the herbalists, centuries ago, laid down for the collection of medicinal plants, and which in modern times have been regarded as simple nonsense, and have consequently been abandoned. The herbalists were particular about collecting their herbs at certain hours of the day or night, and even at special phases of the moon. We have not yet got any exact information regarding the effect of the moon upon the chemical composition of plants, but Sachs's observations show that the amount of starch present in the leaf of any given plant varies considerably under different circumstances. In direct sunshine and under otherwise favourable circumstances, starch is formed very rapidly; but it generally disappears entirely during the night, so that a leaf collected in the evening will prove full of starch, while another leaf of the same plant collected before sunrise will not show a trace. But even in direct sunshine, with all the necessary warmth and moisture, the plant will not form starch if the air in which it is growing be deprived of carbonic acid by means of caustic soda. The method of ascertaining the presence of starch in a leaf is very simple. "The leaf to be examined is first plunged into boiling water for about ten minutes, then taken out and digested in alcohol for about the same time (methylated spirit answers perfectly well). This treatment extracts the whole of the colouring-matter (chlorophyll), and leaves the leaf perfectly white. The leaf is now placed in an alcoholic solution of iodine, and the presence or absence of starch is demonstrated in a few minutes. The absorption of iodine commences at the edges, and soon colours the leaf blue-black if much starch be present, or brown if the quantity of starch be but small. The venation of the leaf appears as a pale network on a dark ground, rendering it a very beautiful object, but all my efforts to preserve a specimen beyond a few hours have hitherto failed." The variations in the amount of starch in the leaves at different periods of the twenty-four hours are peculiarly interesting as rendering it probable that the amount of alkaloidal or other active principles may also vary in a similar way. Since the publication of this book other researches have been made which render such a variation all the more probable inasmuch as they show that some of the poisonous alkaloids formed by the putrefaction of albuminous substances are identical with those occurring in some plants.

In the chemical section of this Year-Book there are several articles on putrefaction-alkaloids or ptomaines. These alkaloids were shortly after their discovery supposed to differ in certain respects from the alkaloids produced by plants, but the points of difference on which reliance was at first placed in order to distinguish between alkaloids which might be formed in a dead body and poisons of vegetable origin which might have been administered for the purpose of producing death have now been shown to be untrustworthy for this purpose. Amongst the most important of the researches on this subject are those of Brieger, some of which are abstracted in this Year-Book, and others of which have been since published in his works ("Ueber Ptomaine" and "Weitere Untersuchungen über Ptomaine," Berlin, Hirschwald). The abstract of another paper by Poehl in this Year-Book contains interesting information regarding alkaloids formed by the decomposition of rye-meal. His results are that ergot and mould have a peptonising action on the albuminous matters of the meal. The degree of putrefaction of albumens is directly proportional to their peptonisation. In the first stages of putrefaction the decomposition of albumens is greater in ergot meal than in mouldy or pure meal. But in the more advanced stages these differences are not so marked. Various alkaloidal products were obtained both from pure and tainted meal after they had been allowed to putrefy. This fact may be of considerable importance in regard to the action of alcoholic drinks. Guareschi and Mosso in a paper abstracted in this Year-Book describe the methods by which they obtained from putrefying fibrine an alkaloid having an action similar to curare. In another work, which is not abstracted here ("Les Ptomaïnes," Première Partie, Rome, Turin, Florence, H. Loescher), they mention that one of the difficulties they had to contend with was the presence of organic alkaloids in different kinds of alcohol. If we consider that a great deal of the spirits used for ordinary consumption are made of so-called silent spirit flavoured with various substances, and that silent spirit is also used in fortifying wines, it is evident that the purity of this spirit is of very considerable importance; but we believe that silent spirit is sometimes obtained by the fermentation of grain which has become mouldy or decomposed to such an extent as to be useless for food, and volatile alkaloids formed during its decomposition will pass over in the process of distillation, and being thus present in the spirit so produced may injuriously modify its action.

The most interesting of the other facts contained in this Year-Book are those which refer to the synthesis of organic alkaloids. Hofmann has shown that piperine, the alkaloid of pepper, can be built up from pyridine, a coal-tar base, and that piperidine, one of the intermediate compounds, which is also obtained with piperic acid when natural piperine is split up by potash, is probably a stepping-stone to the formation of conine and atropine. It was mentioned in a former Year-Book that caffeine, the alkaloid of tea and coffee, can be prepared from theobromine, the alkaloid of cocoa, which in its turn can be obtained from xanthine, a substance which is present in

beef-tea or Liebig's extract, and lastly, xanthine can be obtained from guanine, so that it seems not improbable that the manufacture of caffeine for medicinal purposes from Peruvian guano may be looked for as a consequence of the researches already made on the chemistry of these substances.

EUCLID REVISED

Euclid Revised, containing the Essentials of the Elements of Plane Geometry as given by Euclid in his First Six Books, with numerous Additional Propositions and Exercises. Edited by R. C. J. Nixon, M.A., formerly Scholar of St. Peter's College, Cambridge. (Oxford: Clarendon Press, 1886.)

THE movement for greater freedom in the teaching of elementary geometry than is consistent with a rigid adherence to Euclid's Elements, which may be regarded as having taken definite shape with the formation of the Association for the Improvement of Geometrical Teaching in the year 1871, gains strength surely, if not rapidly. Of this Mr. Nixon's book is one of many indications, notwithstanding his decision in favour of retaining Euclid's Elements as the basis of geometry. For this decision he assigns "*two* substantial reasons of expediency and convenience:—

"(1) That an established *order* of geometric proof is convenient for examination purposes;

"(2) A recognised *numbering* of fundamental results is convenient for reference."

He adds, "as co-operative reasons—the fact that there is no consensus of opinion among experts as to the superiority of any other scheme yet proposed; and the sentiment of repugnance at the thought of sweeping away an institution rendered venerable by the usage of more than 2000 years."

It may be questioned whether Mr. Nixon's first reason "of expediency and convenience" leads as a consequence to the retention of Euclid as a class-book. The experience of the examinations of the University of London is held by many examiners as well as teachers to prove the contrary. But apart from this we would enter our protest against the subordination here, as so often elsewhere, assumed of teaching to examination, of teachers to examiners. Examiners are doubtless strong, but teachers, if they will only combine and assert their convictions in practice, are stronger. We believe too that those who have most carefully considered the question of a rival order of sequence of geometrical propositions would agree that the best order in a logical arrangement does not seriously *conflict* with Euclid's order, except by simplifying it. Rather, by bringing the proofs of each proposition nearer to the fundamental axioms and definitions than Euclid does, it renders less assumption of previous propositions necessary for the proof of any given proposition. It stretches the chain of argument straight instead of carrying it round one or many unnecessary pegs.

Many instances of this may be found in Mr. Nixon's own book. To mention one only—the proof which he judiciously gives of the fundamental proposition that "similar triangles are to one another in the duplicate ratio of their homologous sides" depends directly on the 1st Proposition only of the Sixth Book, instead of the chain being carried round the unnecessary peg of the 15th Proposition, as it is by Euclid himself.

Waiving, however, farther discussion of these general considerations, and granting Mr. Nixon his postulates of expediency and sentiment without farther cavil, we have no hesitation in thanking him for having produced a good and useful book. The conditions under which he has worked are such as to make it unsatisfactory to one who seeks for a natural and symmetrical sequence and grouping of propositions forming a well compacted whole, but all the materials are there for enabling the student, if he has sufficient patience, to make it for himself. The book is well furnished with important propositions not contained in the ordinary editions of Euclid, but various excrescences in the shape of addenda and lemmas have been necessary to accommodate them, and in these addenda those which are of real importance for after use are rather hidden amid a crowd of other consequences, interesting as results, but not necessary parts of the geometrical edifice.

Mr. Nixon has, wisely as we think, distributed his axioms and definitions among the propositions, introducing each one exactly when it is required, instead of commencing with the full series, but it seems to us a serious defect that they are neither numbered nor anywhere collected together for reference. We are rather surprised that he has not taken the opportunity of revising Euclid's editors, and reverting to Euclid's division, into common notions and postulates, of what modern editions call the axioms and postulates; the common notions embracing those general axioms which are true for all magnitudes, while the postulates relate to geometrical magnitudes only and are the really essential basis of geometry.

While retaining the order of Euclid's propositions, Mr. Nixon has very freely revised his demonstrations both in substance and in form. Where he has introduced new demonstrations, they are in all cases, we believe, improvements. The famous *pons* disappears in favour of a proof founded on turning the triangle about one of its equal sides till it falls again into its original plane. Philo's proof of i. 8 is adopted, and consequently i. 7 omitted as useless. In Book II. the diagonals of the rectangles disappear. Euclid's propositions about the correspondence of equal chords, arcs, and angles at the centre of a circle are proved directly by superposition, as recommended in the Syllabus of the Geometrical Association, to which here as elsewhere Mr. Nixon acknowledges his indebtedness, but he still retains the propositions about similar segments which we should have expected him to omit (as in the case of i. 7), as thereby rendered useless. Book V. contains the essentials of the theory of proportion, deduced from Euclid's definition, in the form first suggested by De Morgan. In Book VI. superposition is often employed, where Euclid makes a separate construction, but not invariably, as, we think, might have been done with advantage.

We are less satisfied with the form of Mr. Nixon's demonstrations than with their substance. He objects strongly to Euclid's "prolixity," of which he goes so far as to say, after twenty years' experience as a teacher, that "NOTHING is so great a hindrance to the learner." We doubt this, speaking also not without experience. In

some respects Euclid's prolixity, recalling over and over again propositions which have gone before, does much to fix them in the memory of the learner and prevent looseness in reasoning, and though we think Euclid's style admits with advantage of some pruning, we feel that Mr. Nixon, with overstrained regard to the examination hall, has used the knife too freely, and has run the risk by his style and free use of abbreviations and signs of letting the young pupil believe, as he is only too glad and ready to do, that mathematical work should be written after the fashion of a telegram, grammar disregarded, articles and little words omitted, and what should be sentences written without verb or copula.

Mr. Nixon has deliberately omitted the usual marginal references, on the ground that "learners (1) very generally ignore them; and (2) will gain greater benefit by having to hunt up the references themselves." We believe he has thereby seriously injured the value of his book. Because many boys, in the hands of a careless teacher and left to themselves, ignore the references, is hardly a good reason why they should not be supplied for those who would use them, or who would be required by their teacher to use them; and they would be exceptional pupils under exceptional teachers who could be depended on, or find it easy, "to hunt up the references themselves." Doubtless the exercise of doing this is excellent, but the beginner needs some help in doing it.

We have thought it would be most useful to dwell at some length on Mr. Nixon's revision of Euclid's text, and can only notice generally the exercises and addenda, which occupy more than half the book. The collection of exercises appears to be very well chosen and complete, though we should have been glad to see more of them interspersed among the propositions from which they naturally flow.

The addenda include all the more important developments of the elementary geometry, as well as an introduction to many of the methods of the higher geometry. As a collection of results in the geometry of the straight line and circle this part of the book appears to us from a cursory perusal to be excellent, giving a full account of the important relations of the triangle and its associated circles, centres of similitude, coaxial circles, &c.; while for methods the elementary notions and use of cross ratios, harmonic ranges, inversion, poles and polars are explained and applied to such an extent as to prepare the student naturally for their application to geometry beyond that of the straight line and circle. We fail, however, to find such prominence given to the great principles of duality and reciprocity as their importance, scarcely less in elementary than in the higher geometry, appears to us to demand.

The typography, and the accuracy and clearness of the figures, are to be commended as worthy of the Clarendon Press.

R. B. H.

OUR BOOK SHELF

Practical Histology and Pathology. By Heneage Gibbes, M.D. Third Edition. (London: H. K. Lewis, 1885.)

THE text in this edition does not, on the whole, differ much from that of the previous editions, some new useful formulæ of staining and a more comprehensive arrangement of the subject-matter being the chief differences.

The tables given at the end of the book as to the conversion of degrees of Centigrade into Fahrenheit and *vice versâ*, as to the conversion of English weights and measures into French, are in some points incorrect, and might have been easily correctly copied from any standard work.

E. KLEIN

Farm Live-Stock of Great Britain. By Robert Wallace, F.C.S. F.H.A.S., &c., Professor of Agriculture and Rural Economy in the University of Edinburgh. (London: Simpkin, Marshall, and Co.; Edinburgh: Oliver and Boyd, 1885.)

WITHIN 200 pages octavo, of rather large type, does Prof. Wallace condense much useful information upon farm live-stock. It must, however, be evident that to treat of cattle, sheep, swine, and horses, from a biological, an agricultural, and an economical point of view would at least require double the number of pages, containing twice the number of words, and folded quarto. Brevity has been said to be the soul of wit; but in a work such as this we cannot but feel that it must be intended by its author either for those who know nothing or for those who know something of the subject. It appears to us to fall short of the requirements of each of these classes of readers. Four and a half lines—forty words, in fact—upon the Devon breed of cattle cannot be considered adequate, however terse and compressed they may be (and to the point they undoubtedly are), to giving a good word-picture of this race. Besides, Prof. Wallace must excuse us for differing from him as to his statements even in this very short description. He is wrong in saying "colour blood-red, no white." There is white upon the fore-udder in almost all Devon cattle, and it is unfortunate that there should be a manifest error in this very short description of an important breed.

The book aims at too much, and is too vague in its general plan. The writer is successful in being concise, but he is not free from errors, and a greater amplitude in his observations would have both conferred a greater general interest upon his pages, and been more satisfactory to an earnest student of agriculture. Let us hope shortly to see an enlarged edition.

Common-Sense Euclid. Books I. and II. Part I. By the Rev. A. D. Capel, M.A., St. John's College, Cambridge. (London: Joseph Hughes, 1886.)

THE object of this book, as the author tell us, is to point out, especially to teachers and those teaching themselves, the portions of the treatise which either present difficulties to the beginner or escape their notice altogether.

The propositions are explained in a very clear and concise way, some of them being even worked backwards and their analysis being made in every case; explanations are given, here and there, where they are most required, and are put in the easiest possible way. Problems, at the end of each proposition and also at the end of the book, are given, making a total of 300, followed by hints for their solution.

The figures are not placed opposite each proposition, as they are in most editions, but all together at the end. The plan adopted is a very good one, it being understood that the student must construct the figures for himself.

J.

Arithmetic for Schools. By the Rev. J. B. Lock, M.A. (London: Macmillan and Co., 1886.)

THIS is a carefully prepared school-book, forming, as to scope, a sort of arithmetic mean between Hamblin Smith's and Brook Smith's or Muir's. It contains the usual rivulets of text ending in seas of examples. In the purely arithmetical part of the book logical accuracy is attempted with considerable success. Want of grasp is much more evident in the part which deals with the applications. There the division into subjects is strangely illogical, and

slight inaccuracies of thought and language occur. Is it really the case, for example, that rate of interest (p. 181) is totally independent of *time?*

LETTERS TO THE EDITOR

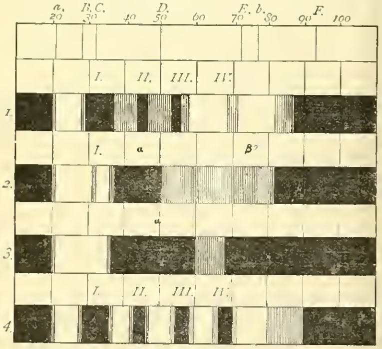
[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Chlorophyll

IN a short note in NATURE (vol. xxxii. p. 342) I mentioned the discovery of a colourless substance produced by the action of reducing agents on chlorophyll and regenerating, on being exposed to the air, the original green solution. I may be allowed to add to my previous statement that if this reaction is not pushed too far and the resulting substance is duly concentrated, it is not colourless, but of a reddish-brown by daylight, of a splendid ruby red (very different from the well-known port-wine red colour of chlorophyll) by limelight. Its spectrum is chiefly characterised by the total absence of band I., and the presence of a broad band corresponding exactly to band II., and the two intervals between I. and II., and between II. and III. Band IV. seems also to be present, though somewhat altered in its position and intensity.

The presence of a slightest trace of oxygen is immediately announced by the appearance of the I. chlorophyll band, so



that the reaction may be considered as a most sensitive test for oxygen. On further exposure to the air, as already mentioned, chlorophyll is regenerated. This new substance being evidently a product of reduction of *chlorophylline*, the green-colouring matter of chlorophyll isolated and described by me in 1869, it may be called *protochlorophylline*, or simply *protophylline*.

Its solutions can be kept in sealed glass tubes containing H_2 or CO_2: *in this latter case in a dark place, for on being exposed to light they turn green.* Can it be inferred from these facts that the oxidation takes place at the expense of CO_2—that carbonic acid is actually reduced under the joint action of light and of a chlorophyll solution? The question, if answered in the affirmative, is of so great importance, that I am now taking all the pains to arrive at a definite conclusion.

The optical properties of *protophylline* seem to indicate its presence in freshly-prepared chlorophyll solutions. Indeed the difference presented by the spectrum of a freshly-prepared green solution and that of Mr. Stokes's *modified chlorophyll* may be easily accounted for by the presence in the former of the broad *protophylline* band intercepting the rays of light in the two intervals between the bands I. II. and III., as just mentioned. To the presence of different quantities of *protophylline* may be likewise attributed the varying relative intensity of the bands II. III. IV.,—a fact that has attracted the attention of many observers.

At all events, it cannot be doubted that the study of this curious substance, though attended with considerable difficulties, all the operations taking place in a total absence of oxygen, and under the continual control of the spectroscope, will throw a new light on that most important of physiological problems—the part played by chlorophyll in the decomposition of carbonic acid by the living plant. C. TIMIRIAZEFF

Moscow University

The Stone Age in the Malay Peninsula

IN NATURE, vol. xxxiii. p. 377, there is a notice of a paper by M. de Morgan, published in *Cosmos*, on the Stone Age in the Malay Peninsula. Will you permit me to offer a few remarks with reference to this matter. In the first place, it is said that M. de Morgan came into contact with three native races, which he respectively names Sakayes [Sakai], Seumangs [Sĕmang], and Rayats [Ryot]. I have put in brackets the commonly-accepted spelling in the Straits. It is funny what peculiar mistakes travellers make when passing through a country the language of which they do not understand, ryot being the word used in the Straits to express those followers or retainers of a native chief who are not actually his debt slaves, but who owe him more or less of feudal allegiance; Malays here invariably use the word when speaking of the following of a Sakai chief. The word ryot is, I believe, also used in the same sense in India. With reference to the tribes of whom M. de Morgan speaks as living in the recesses of the mountains, and whom the Sakaies called "fire apes," I cannot help remarking that I have never heard the Sakaies speak of them myself, nor can I find that any other Government servant here has heard of them either; still we are in pretty constant communication with certain of the Sakaies of these hills, and for my part I have at different times stayed for longer and shorter periods at the clearings of some of the chiefs whom M. de Morgan visited, and moreover I have employed most of the same Malays who followed M. de Morgan. By the bye, these were Sumatran Malays, and they told me some very extraordinary tales about the wild tribes before I started up country with them; these foreign Malays are especially addicted to telling marvellous tales of the wild tribes of the mountains, but so far I have not been able to verify their information in the least degree either from the Sakaies themselves or from native Malay sources. It would be interesting to know what equivalent was used for the expression "fire apes." Was it a Malay word or a Sakai word? With reference to the Stone Age I quite agree with M. de Morgan in believing that at a not very late period—probably just before the Malay invasion—there were tribes living in the interior who were not acquainted with the use of iron; up to the present moment I have been able to collect twenty-two stone implements. I have sent drawings and notices of these to the Anthropological Institute. I may, however, here mention that of these twenty-two specimens one is the half of a stone bracelet; the rest are all chopping-tools of different descriptions, used, I think we may fairly conclude, by a race of boat-builders, who most likely constructed dug-outs, much like the Malays of the present day. I adduce this supposition from the fact that of my twenty-one specimens two are perfect gouges, and six others are of the description which Dr. Evans has classed under adzes. The cutting-edges of nearly all my specimens have been considerably damaged by use. The high polish which M. de Morgan's specimens—and mine also—exhibit is, I think, accounted for in a great measure by the fact that they are used and very highly prized by the Malays as whetstones; the women preserve them, especially to sharpen their razors on, with which they shave the heads of their children during the periods ordered by custom or religious law; and the men were, until lately, very anxious to procure them to sharpen the iron spurs used in cock-fighting. As almost all of the specimens procured by me have been purchased of Malays who have inherited them from their ancestors, and prized them as heir-looms, it is, I think, reasonable to suppose that in their original condition some of them, at least, were considerably rougher than when they came into our hands; this supposition is further confirmed by a remark made to me the other day by a Malay chief. He said that he once had a thunderstone given to him which was so rough that he had to wear it down on his emery-wheel before he could use it as a whetstone. I have one specimen which has no cutting-edge, but is squared off at each end and is almost spindle-shaped. I have

also seen another specimen of the same description. [illegible] that these two specimens have been manufactured by Malays for whetstones out of the so-called thunder-stones? I cannot account for them in any other way; they are too slight for hammers.

For my part, I have always found the Sakaies especially wanting in every respect as to traditions beyond the memory of their own generation, and they have invariably answered my inquiries as to the origin of the stone axes by saying, like the Malays, that they are thunder-stones. To such an extent is this belief held by Malays, that the other day a Malay of considerable social standing assured me that once a cocoa-nut palm was struck by lightning close to his house, and that about a month afterwards he searched about the roots of the tree and found the thunder-stone which was the cause of the damage: it was this man's father who for several years kept a fire alight in his house, which fire was generated from the same tree after it had been struck by the electric current.

I have already drawn attention to the Malay belief as to these so-called thunder-stones (NATURE, vol. xxxii. p. 626). My specimens are all in the Perak Museum at Thaiping.

Kinta Perak, April 5 A. HALE

On a Thermo-electrical Phenomenon in Connection with Prof. Balfour Stewart's Paper on Terrestrial Magnetism

IN the *Philosophical Magazine* for May Prof. Balfour Stewart, in his paper "On the Causes of the Solar-Diurnal Variation of Terrestrial Magnetism," takes in one place (p. 443), for an example, the case of "an ordinary electric circuit, say of a circular shape, and horizontal, and heat it by causing some source of heat, such as a lamp, to travel slowly around it with a definite rate of progress." He goes on to say that no current due to the heating will take place. So it would generally be thought. If, however, the experiment be even roughly tried, at all events with an iron or nickel wire, the contrary takes place. An account of the experiments, &c., which I have made on this subject, was read before the Royal Dublin Society on March 24, and will, in the course of time, be printed in the Society's *Proceedings*. Though there is a current in a wire on causing a heated portion to travel along it, it seems unlikely from the nature of the phenomenon that it could in any way be inferred that the higher air would similarly affect a current under the sun's heating. FRED. S. TROUTON

Physical Laboratory, Trinity College, Dublin

Do Migratory Birds Return to their Old Haunts?

MUCH evidence has been given by naturalists to prove that birds of passage return to their old haunts. The following, I think, may be of interest to some of the readers of your valuable paper. For the past two springs a cuckoo gifted with a decidedly peculiar note has visited this neighbourhood. Within the last fortnight it has again arrived. Its song consists of three clear distinct notes, cuck—coo—coo, the second note being a semitone above the last. This it never varies. We all know that towards the end of its sojourn the cuckoo suffers from hoarseness, or, as the country people say, "changes its tune." Although this bird suffers in a similar way, yet it still maintains its peculiar song—three notes. As far as I can ascertain, it does not wander beyond the same limits—from the park here to a little hill about half a mile distant. I think these facts not only conclusively prove that the cuckoo returns to its old quarters year after year, but that it also restricts itself during its stay in this country to the same locality. F. C. TAYLOR

Summerleaze, East Harptree, May 13

The Poison of the Stinging-Nettle

IN the interesting article in your issue for May 6 (p. 5), on "Plants and their Defences," there is one sentence on which I should like to be allowed to offer a few remarks. It runs thus:—"This fluid [of the stinging-gland of the stinging-nettle] is generally conjectured to be formic acid—a view based on the fact that this acid can be obtained from the nettle-plant by suitable means." Does this "conjecture" rest on any other basis than the similarity of the effect produced by the sting of the nettle and the bite of the ant? I am inclined to think not. Certainly the fact that formic acid can be obtained from the nettle-plant is not in itself a cogent argument, seeing that it has [illegible] cell-sap of living plants is a widely-spread constituent of the [illegible] harmony with the fact that the fluid contained in the [illegible] glands of the nettle has frequently, if not always, an alkaline reaction. It seems strange that we have at present no trustworthy observations on so interesting a question. Can none of our physiological chemists come forward and remove it from the region of conjecture? ALFRED W. BENNETT

St. Thomas's Hospital, May 13

What is Histioderma?

CAN any of the readers of NATURE inform me to what class of fossil organisms belongs the genus Histioderma? Mention of the name—but of the name only—is made by Sterry Hunt in this journal, vol. vi. (1872), p. 54, and by Hicks in the *Quarterly Journal* of the Geological Society of London, vol. xxix. part 1, 1873, table facing p. 42. It does not occur in Broun's "Index Palæontologica," in Pfeiffer's "Nomenclator Botanicus," nor in Scudder's "Zoological Nomenclator." The name is not to be confounded with that of Carter's genus, Histoderma, established 1874, for recent sponges. S.

Leyden, May 15

ON THE INFLUENCE OF FORESTS ON THE CLIMATE OF SWEDEN[1]

A VALUABLE Report on this subject has been prepared by Dr. H. E. Hamberg, and printed as an appendix to the Report of the Forest Commissioners of Sweden for the year 1885. The observations were commenced in 1876, on the principles established by Dr. Ebermayer in Bavaria, but Dr. Hamberg soon found that the mere comparison of the results obtained at the forest station with those yielded by its sister station in the open country was insufficient to bring out all the peculiarities of forest influence, and accordingly he added a third class of station, situated in a clearing in the forest itself (*öppen plats i skogen*). The various results of these observations are discussed in a very exhaustive manner, and we must refer those interested in the subject to the Report itself. The author's conclusions, however, are very interesting, and are reproduced here in full.

"Our researches do not allow us to determine whether the presence of the forests on the whole contributes to increase or diminish the quantity of heat in the atmosphere, that is to say, to raise or lower its temperature. In fact, we have been entirely unable to take into account either solar radiation or the radiation from the needles[2] and the points of the trees. Until we are able to ascertain the quantity of heat which escapes from these surfaces, and its relation to that escaping from other surfaces, it is quite impossible to determine with certainty the influence of the forest on such an important subject as the mean temperature, and must confine ourselves to approximate estimations. Among the various surfaces which are met with in Sweden the most important are assuredly water, bare ground or rock, soil covered by herbage, and finally forest. Neither the surface of the lakes and sea nor the bare soil of town streets have any resemblance to the forest: the climate of the latter bears no similarity to a maritime climate or a town climate. A forest may best be considered as an instance of vegetation on a gigantic scale, as is evident from the low temperature of the ground under the trees, and the freshness of the air in summer, especially in the evening and at night-time, thus affording evidence of active radiation. In this case the forest would be a source of cold rather than of heat. But here we are simply dealing with suppositions.

"From this point of view a forest is distinguished from all the other surfaces we have mentioned, in that it extends into a stratum of air lying far above that in which man lives and carries on all of his occupations which depend on

[1] "Om skogarnes inflytande på Sveriges klimat." From *Quart. Journal Roy. Met. Soc.* for April 1886, communicated by Mr. R. H. Scott, F.R.S.

[2] The forests dealt with were entirely of pines and firs.

climate, such as agriculture, &c. It should follow from a this that wheth[illegible] or a defect of heat, the one or the other should, thanks to the winds, be communicated to a greater mass of air, and be less sensible in the stratum close to the ground. The thermic properties of other surfaces are more immediately available in the lower stratum, and consequently, from the practical point of view, exert a greater influence on the temperature of the earth and of its immediate vicinity.

"If, then, we confine our consideration to that which from the practical point of view is perhaps the most important, the influence of forests on the state of temperature in the stratum in which man generally lives, in so far as this can be determined in the ordinary way by thermometers, I think that our reply for this country (Sweden) will be less uncertain, and it is as follows:—

"In the districts of our country which are open and are cultivated, during the annual interval of cultivation, a forest lowers the temperature of air and soil during evenings and clear nights, restricting the period of daily insolation, and thereby checks vegetation.

"The other influences of forests on temperature are either so slight that they possess no practical importance, as, *e.g.*, the moderation of cold in winter, or else are of such a character that they elude the ordinary mode of observation by thermometers. Among the effects of this nature we may mention the well-known fact that forests afford shelter against cold and violent winds to vegetation which would suffer from these winds, or to objects whose temperature is higher than that of the environment, as for instance the human body. It is in this last respect that the Swedish saying is true, namely, that 'the forest is the poor man's cloak.' In certain cases it may also yield protection against the cold air or fog which on cold nights comes from districts in the vicinity which are visited by frost. The advantages on the score of temperature derivable from the forest may therefore be considered to resemble that obtainable from a wall, a palisade, a hedge, or any object of that nature.

"On the one hand a forest, where it is close at hand, offers mechanical protection against cold and violent winds. On the other hand, it does injury either by retaining the solar heat required by crops, or by lowering the temperature of the soil during clear nights, and thus favouring the development of hoar-frosts. At a distance forests have no sensible influence on the climate of Sweden.

"If we wish to put these results to a practical application, it is impossible to say in general whether one should, or even could, clear the forest without injuring agriculture. But it appears that as regards the temperature, if we disregard the utility of forests in other directions, we might make extensive clearances without any prejudice to agriculture. It is certainly not a mistake to say that our best cultivated districts are the freest from wood, nor is it a mere chance that the harvests are, on the whole, more sure in the open country than in the forest. In the event of a bad harvest it is, as I well know, the wooded districts which have suffered most. At the same time I must at once admit that these provinces are also influenced by other powerful physical factors, possibly even more active than forests, such as an elevated situation, a bad soil, the presence of swamps, &c. But nevertheless it appears to me, after all that has been said in the preceding pages, that the forest has some bearing on the subject.

"At the present day, the words spoken 130 years ago by Pastor P. Högström, and at that time member of the Swedish Academy, are very generally applicable, inasmuch as it has been found that cultivation can to a great extent remove from a district its tendency to hoar-frost; this same result has frequently been obtained by draining or by clearing the forests, particularly those of deciduous timber, where the fogs, especially those which bring on [illegible] appear to have their origin and their aliment. On the contrary, a pine forest is an excellent shelter against cold, especially when it can stand between the country and marshes or surrounding districts where the cold has its rise. If, however, the forest interferes with sunshine and with wind, it should be cleared. It results, therefore, that while in some districts the clearing of a forest has been beneficial in averting hoar-frost, in others the result has been directly the opposite."

RESULTS DEDUCED FROM THE MEASURES OF TERRESTRIAL MAGNETIC FORCE IN THE HORIZONTAL PLANE, AT THE ROYAL OBSERVATORY, GREENWICH, FROM 1841 *TO* 1876

SIR GEORGE AIRY has recently published a valuable and extensive series of diagrams representing the diurnal changes in the magnetic forces in the horizontal plane at Greenwich between 1841 and 1876. In an introduction, the ex-Astronomer-Royal gives a short statement of the circumstances under which the magnetic work was undertaken at Greenwich, and the various changes which have taken place. With regard to the curves here brought together he writes as follows:—

The form of the curves, and the position of the points on them corresponding to hours of solar time, leave no doubt that the diurnal inequality is due mainly—and, as far as I can judge, entirely—to the radiant heat of the sun; and, it would seem, not to its heat on the earth generally, but to its heat on points of the earth not very distant from the magnets. In the hot months of the year the curve, though far from circular, surrounds the central point in a form which, as viewed from that central point, never crosses itself, and is, generally speaking, usually symmetrical with regard to E. and W. But in the cold months the space included in the curve is much smaller, in many cases probably not one-fifth of what it is in the summer months; and the curve often crosses itself in the most bizarre fashion, with irregular loops at these crossings. In the summer months there is a certain degree of symmetry; but here is, constantly, a preponderance on the west side, which leads me to imagine that the magnetic effect of the sun's heat upon the sea is considerably greater than the effect on the land.

To obtain some numerical basis for a report which, though undoubtedly imperfect, may convey some ideas on this wonderful subject, I have adopted the following course. I have confined myself to the months of June and July as probably the two hottest, and the months of December and January as probably the two coldest. In each of the curves applying to these months I have laid down a system of rectangular co-ordinates corresponding to the Greenwich astronomical meridian, and the line at right angles to the meridian (the geographical E. and W.). The extreme north ordinate and the extreme south ordinate were measured, and their sum taken, and interpreted by a scale of measure formed in accordance with the theory of the instruments, and this interpretation forms the "range of meridian force in terms of the mean horizontal force." In the same manner, the "range of transversal force" is measured. As the time of each two-hourly or hourly result is marked on the curve, there is no difficulty in fixing approximately on the solar times corresponding to the extreme N. and S. values and the extreme E. and W. values mentioned above. These are all the elements of the magnetic record which are included in the table.

MOVEMENTS ON THE SUN'S SURFACE

M. A. BELOPOLSKY, of the Moscow Observatory, states in *Astronomische Nachrichten*, No. 2722, some considerations of much interest regarding the solar

rotation. They are based upon a hydrodynamical investigation by Dr. Jukowsky, showing that in a liquid globe of which the angular rate of rotation increases from centre to surface according to a certain law, superficial currents set from the poles towards the equator, but take the opposite direction if the rotation be accelerated from surface to centre. These theoretical deductions have been experimentally verified by M. Belopolsky. A new criterion is thereby furnished as to the fashion of the sun's internal rotation. For both Spörer and Carrington have recognised that the motion of spots in latitude tends, on the whole, poleward; while the closing in towards the equator, with the progress of each epoch of disturbance, of the zone in which spots, faculæ, and prominences chiefly manifest themselves, is a well-recognised feature of periodical solar activity. This zonal movement is held to depend upon currents at considerable depths, but the drift of individual spots upon surface-flow; hence the sun's system of circulation is such as to indicate, according to Jukowsky's theory, rotation accelerated towards the centre.

The cause of this inequality is found by M. Belopolsky in the non-homogeneous character of the solar globe. Assuming that the variations of its density conform to the law adopted by M. Roche for the terrestrial spheroid ($\rho = \rho_0 (1 - \beta r^2)$), it follows that gravity must attain a maximum at a certain depth below the surface (this depth, in the case of the earth, is 1/6th of the radius). Under these circumstances the rate of rotation and amount of polar compression of successive solar strata must vary with gravity, and in the same sense. It is, moreover, highly probable that gravity and angular velocity will attain a maximum simultaneously. The ensuing frictional acceleration of the superimposed slower-moving layers is so conditioned as to lead directly to a law of surface-rotation identical with the empirical formula arrived at by Spörer from observation solely ($\xi = \omega + a \cos \phi$).

The minimum period of rotation for an interior solar shell, computed according to the foregoing hypothesis, is 21·3 days; the longest observed period for any part of the superficial globe is 27·5 days The mean of the two (24·4 days) differs very little from the period of 24·5 days deduced by Hornstein from magnetic observations. It is pointed out that Faye's *rationale* of the peculiar character of the sun's rotation implies for an inner nucleus the improbably short period of 2·2 to 3 days.

EDUCATION IN THE UNITED STATES[1]

THE work of education in the United States of America, as delineated in the Commissioner's report, is making steady progress and keeping pace with the great increase of population in that country, where are 266 cities with an average of 40,000 inhabitants, and a lowest limit of 7500. Various States are able to perceive that a more efficient course of education provided in them for the next generation is one of the greatest attractions to those earnest striving settlers who are the backbone of a growing country; and money and energy in increasing amounts are devoted to the purpose. The successful guidance of these powers to desired results depends largely upon the selection of capable district superintendents who will provide for the more careful selection and improvement also of teachers, and introduce the best methods and the best facilities of instruction; thus making common to the many what would have been confined to the extra intelligent few. The first use, therefore, to be made of liberal money votes is the provision of high-class inspectors, who can be secured only by higher salaries. One important duty of these officers arises from the system of establishing schools in every district being so perfect in all of the United States, that in Connecticut, for example, there are 158 school districts which have less than eight scholars in attendance during the year, and one case is quoted, not as being by any means unparalleled, of a school having only four scholars during the year, and for three months having one only, whose education consequently cost the district 60 dollars. In such circumstances the State inspectors can recommend the consolidation of several of these school districts into one. Where this cannot be done, it is not likely that an efficient, qualified teacher can be secured for each. Yet rather than this scattered population should grow up half taught, the New York superintendent of popular instruction recommends that a sufficient salary shall be made good out of State or general Government funds. It is the more necessary to meet this difficulty as population is not everywhere increasing. In Maine, for example, population has decreased, and the number of school districts has been reduced already.

In Massachusetts a greater number of scholars than the whole number of the school population (from 5 years of age to 15) were enrolled; but, on the other hand, Maryland and Virginia showed only 28 and 29½ per cent. attending, and what reports could be gained from Louisiana showed only 19 per cent. enrolled, and not above 13 per cent. attending. Of course, in such a State, there is the double difficulty of getting the coloured population to school and of raising the money to pay for it; poverty standing in the way of fair remuneration of teachers as much as the lazy ignorance of the blacks in that of regular attendance. Naturally half-day sessions have suggested themselves as being economical in every way, requiring only half the staff of teachers, and half the schools and school-furniture. But a danger in this system is lest the teacher should be overworked; and, where he is able to do so greatly increased work, it is fairly recommended that his salary should be increased accordingly. A great variety of work therefore presents itself to the inspectors, and much discretion and knowledge will be required to meet all emergencies.

It is easy to see that no New World organisations or ambitions are any match for the evils of population bred down to the point of a hard struggle for existence. These evils are developing in America as fast as they did in ancient cities. Truancy is increasing, and many children never attend school even in such a city as Providence, R.I. New York experience acquits employers of labour of any mischievous influence in the matter. It is curious to note that while in England charitable funds have been diverted gradually to the education of the more promising children, and School Boards have had intrusted to them the unpromising residuum, in American great cities the public schools take in all the former, while the benevolent are urged to take up the work of teaching the latter, for whom the regular course is too advanced. Free education, approved by certain States, can be more easily adopted in such a state of things, where the large ratepayers get large advantages, than in a country like ours, where payment made and advantage to be gained would be in exactly inverse ratio.

A very large proportion of the pupils in the primary schools are of the ages of 8 and 9, and the number who pass on to the secondary schools is about 40 per cent.; but not 1 in 12 of these reaches the higher standards of the secondary schools. Since also 60 per cent. never get beyond the elementary schools, the report urges how needful it is that the education given in these schools should be as complete in itself as possible, and not merely a preparatory step towards the "grammar" or secondary studies. The different proportions of arithmetic required in the different cases will force this upon the mind at once. Elsewhere in the report it is taken as an accepted rule that more cultivated fitness is required to teach a primary than a model school.

[1] "Report of the Commissioner of Education for the Year 1883-84." (Washington, Government Printing Office, 1885.)

The number of females attending the second-grade schools is equal to the number of males, and three-fifths of the teachers also in these schools are women. So many important institutions having for their main object the higher education of the sex have been opened in the United States that it is considered that the special examinations of females conducted for some time past under the auspices of the Harvard University may now be dropped. The number of them competing for scholastic honours on the same basis as the men is steadily increasing; "but," the report suggestively adds, "not rapidly enough to threaten any disturbance of existing social, domestic, or business relations."

One association for promoting the higher education of women reports that while the physique of lady students is higher than among women at large, yet that even that of the former is painfully low, and requires that measures should be taken against so dangerous a deterioration.

With regard to the co-education of men with women, a committee, appointed by a Western College to inquire into the subject, conclude by saying:—

"Joint education of men with women in the higher studies has now been tried in a sufficient number and variety of colleges, and for a sufficient length of time to prove that no special difficulties and evils grow out of it, and that it does away with the greatest difficulties and evils of the old monastic system. It makes college life and society more nearly human instead of only 'half-human.' The half-human ever verges first and last towards the bestial, whether in armies, on shipboard, in miners' camps, or in colleges, monasteries, or nunneries. It would be wise to humanise the colleges still more, rather than to begin the process of dehumanising them."

It is then urged that all lectures and studies should be conducted in as public a manner as possible, and attended by friends and relations of both sexes.

Kindergarten teaching is being carried out more largely, but is making its way more as a charitable institution than as a branch of education. Very appropriately it is becoming the ladies' charity; its work is found specially beneficial as the early beginning of a reformatory education for the purpose of overcoming inherited vicious propensities and physical infirmities. Most energetic efforts for this purpose are being made at San Francisco in particular.

Attention is specially called in this report to the desirability of teaching history so as to make the reading of it an intelligent study, attractive to its learners, who will fill up leisure time with its pursuit instead of, as of old, insisting on the laying to heart long tables of dates and dry facts, "killing the life out of the subject, disgusting the pupils, and giving them a dislike for historical reading."

Colleges of the highest class keep increasing in number, yet, nevertheless, the totals of teachers and pupils are small for the proud name each claims of "University." Some have resigned that title and devoted themselves to school work; but more fresh ones have sprung up which constitute a splendid force for future generations when their work, their wealth, and the population supporting them, have been multiplied. The fact of a superabundance of such institutions proves how highly learning even of the least utilitarian character is esteemed.

Perhaps traceably to temporary reasons, classics seem to be gaining rather than losing ground upon physical science at Harvard, a higher standard of instruction and attainment having been required in the latter. Technical schools, however, make steady, though not rapid, progress. Agriculture, mining, and building form so large a proportion of American employment that full attention can be given to these subjects with little hesitation. The bulk of their pupils are at once absorbed in further teaching, instead of putting into practice what they know, with their own hands.

The United States Commissioner of Education takes an annual survey of the whole educational world, and presents it to all who study his report, and when the subjects to be taught a different people like the 200,000,000 of British India are in it placed side by side with those which seem important in our own schools, a question suggests itself whether scientific teachings have not a better claim than the old knowledge to the title of "literæ humaniores." We see how local and confined are classical and historical studies, and of what common value to the whole human race are the elements of natural and physical science.

The free education which Texas and others of the United States are in favour of is not recommended by our Commissioners even in a country where it would interfere with so few vested interests as in India.

We are glad to know that a work on public libraries is progressing, which is intended to supplement the special report published in 1876.

There are 11,663 institutions in regular correspondence with the Bureau, and no one reading this report can fail to see the importance of a common centre of communication to so many and so various efforts to carry on the great work that will have such an influence over the next generation. A central nucleus, again, to this organisation must be a library, by reference to which inquiries from so many quarters on so many subjects may be answered. It is hard, therefore, to believe it a wise economy of a great nation to cut down the allowance for so permanent a part of the office as this from 1000 dollars to 500 dollars, which, nevertheless, has been done.

W. ODELL

COLLECTION OF HAIRS AFTER EARTHQUAKES IN CHINA

IN Dr. Macgowan's "Note on Earthquakes in China," republished in NATURE for May 6 (p. 17), I find the following passage:—

"The tremors that are experienced in Chehkiang, Kiangsu, and coterminous regions to the west, are sometimes followed by the appearance on the ground of substances that in Chinese books are styled 'white hairs.' When I first called attention to records of that kind that are found in local gazetteers, I suggested that they might be crystals precipitated by gaseous emissions, such as were once reported as occurring after an earthquake in south-west of the United States; from later descriptions of these 'horsetail-like' substances I incline to the opinion that they are organic, perhaps mycelium."

I think there can be little doubt that Dr. Macgowan's conclusion is well founded, and that the "white hairs" have no real connection with the earthquake.

In 1852, during one of the late Mr. Fortune's visits to China, he experienced the shock of an earthquake at Shanghai. He gives the following curious account in "A Residence among the Chinese" (pp. 4, 5), of the subsequent search for the hairs:—

"Groups of Chinese were seen in the gardens, roadsides, and fields engaged in gathering hairs which are said to make their appearance on the surface of the ground after an earthquake takes place. This proceeding attracted a great deal of attention from some of the foreign residents in Shanghai, and the Chinese were closely examined upon the subject. Most of them fully believed that these hairs made their appearance only after an earthquake had occurred, but could give no satisfactory explanation of the phenomenon, while some, more wise than their neighbours, did not hesitate to affirm that they belonged to some huge subterraneous animal whose slightest shake was sufficient to move the world.

"I must confess, at the risk of being laughed at, that I was one of those who took an interest in this curious subject, and that I joined several groups who were

searching for these hairs. In the course of my travels I have ever found it unwise to laugh at what I conceived to be the prejudices of a people simply because I could not understand them. In this instance, however, I must confess the results were not worth the trouble I took. The hairs, such as I picked up, and such as were shown me by the Chinese, had certainly been produced above the earth and not below it. In some instances they might readily be traced to horses, dogs, and cats, while in others they were evidently of vegetable origin. The north-eastern part of China produces a very valuable tree known by the name of the hemp-palm [*Chamærops Fortunei*, see Kew Report, 1880, p. 31], from the quantity of fibrous bracts it produces just under its blossoms. Many of these fibres were shown to me by the Chinese as a portion of the hairs in question; and when I pointed out the source from which such had come, and which it was impossible to dispute, my friends laughed, and, with true Chinese politeness, acknowledged I was right, and yet I have no doubt they still held their former opinions concerning the origin of such hairs. The whole matter simply resolves itself into this: if the hairs pointed out to me were the *true* ones, then such things may be gathered not only after earthquakes, but at any other time. But if, after all, these were not the real things, and if some vegetable (I shall not say animal) production was formed, owing to the peculiar condition of the atmosphere and from other causes, I can only say that such production did not come under my observation."[1]

W. T. THISELTON DYER

THE U.S. GEOLOGICAL SURVEY

THE American papers contain an announcement which will be received with some astonishment in Europe. A member of Congress, Mr. Herbert, of Alabama, has introduced a Bill into the House prohibiting the Geological Survey of the United States from expending any money for palæontological work, except for the collection, classification, and proper care of fossils and other material; and from composing, compiling, or preparing for publication monographs, bulletins, or other books except an annual report containing merely the transactions of the bureau and other routine official matter. It is further proposed to sell off the laboratories and other property of the Survey which after the passing of the Act would be no longer needed. Of course there may be official or departmental reasons for reorganisation or retrenchment of which the outside world is ignorant. But these reasons must be very serious indeed to justify such action as is proposed. If there is one scientific undertaking of which the United States have pre-eminently just reason to boast as a model to all civilised countries, it is their Geological Survey. For completeness of equipment it has no rival in the world, and already though it has only been seven years in existence its work both for excellence and amount has placed it in the very front of the scientific organisations of the time. Whether we look to its purely scientific achievements or to the importance of its practical work in mining and other economical departments, the crippling of the resources of the Geological Survey of the United States would be a calamity against which not only all lovers of science but all who are interested in the continued development of the natural productions of the great republic would energetically protest. We can hardly suppose that Mr. Herbert will have many supporters, and it is difficult to conceive from what possible motive he is acting. He calculates that if his Bill passes he will effect a saving of 250,000 dollars. He should try to find some branch of the public service where economy and retrenchment could be practised without seriously injuring the scientific credit and industrial progress of his country. And no doubt he could succeed in this search.

[1] "During a recent visit to the North-West Provinces of India, where earthquakes are not unfrequent, I could find no traditions such as that I have alluded to."

THE ROYAL SOCIETY SOIRÉE

THE President and Council of the Royal Society are to be entirely congratulated on the success of the reunion at Burlington House on the 12th inst. It was generally felt that the display of objects of interest was finer than any brought together for some years, and the general satisfaction expressed must have amply rewarded those upon whom the burden of the arrangements had fallen.

It is a little hazardous to say which was the most interesting object; but as an *actualité* the unpaired parietal eye of Sphenodon exhibited by Mr. Baldwin Spencer, fully described in last week's NATURE, perhaps bore the palm.

Next in biological interest came an exhibit by Mr. W. H. Caldwell including a complete series of the Ceratodus from the unsegmented egg to hatching. The complete exhibit illustrated early stages in development of the Monotremata—Ornithorhynchus and Echidna, the Dipnoid Ceratodus and some marsupial genera. The series were as follows:—

(1) Series of early stages of Ornithorhynchus, from a few hours after fertilisation to the newly-laid egg, of about the stage of a 36-hour chick; (2) series of early stages of Echidna, from just before laying to the newly-hatched fœtus; (3) various stages of young Echidna, from hatching up to 5 inches long; (4) complete series of Ceratodus, from the unsegmented egg to hatching; (5) stages of young Ceratodus after hatching; (6) series of about thirty stages, from segmenting egg up to birth of *Phascolarctos cinereus*; (7) ditto of *Halmaturus rufus*; (8) Specimens showing the arrangement of the embryonic membranes in *Macropus major*.

There were two exhibits of micro-organisms—one of micro-photographs of Bacteria, and another of certain micro-organisms themselves—by Mr. Cheshire. The former included enlargements, from negatives obtained with an oil immersion $\frac{1}{25}$ inch, of the following:—

Anthrax-bacillus, in tissue-sections and cultivations; hay-bacillus; bacillus of malignant œdema; micrococcus of pneumonia; tubercle-bacillus; bacillus of foul brood; *Bacillus megatherium*; *Clostridium polymyxa*; microbe of chicken cholera; comma-bacilli of Koch, Lewis, and Tinkler; Bacteria of putrefaction.

Mr. Cheshire exhibited (1) *Bacillus alvei* in sporulation; (2) *Bacillus alvei* spores in chain; and (3) spermatozoa of Apis forming in flocculent masses for packing in spermatophore.

Preparations illustrating the histological structure of the secretory tissues of certain plants, in which the substances secreted are of economic importance, were exhibited by Mr. W. Gardiner. Among these were hairs of leaf of *Flemingia Grahamiana*—wurras dye; laticiferous vessels of the stem of *Manihot Glaziovii*—ceara rubber; glands of the leaf of *Cinnamomum Camphora*—camphor.

In connection with biological inquiry may be specially mentioned Mr. Frank Crisp's demonstration of a new microscopic object-glass, by Prof. Abbe of Jena, an exhibit rich in hope not only for the future of microscopy, but also for astronomy. Eight of the ten lenses of this objective are made of a new kind of optical glass, composed of phosphates and borates without silex. The glass hitherto used contains as essential components only six chemical elements, while the new objective contains not less than fourteen. The secondary spectrum is by this means entirely removed, and only a small tertiary spectrum remains. The improvement in definition is especially marked

in the case of Bacteria and other minute micro-organisms.

As representing this last-named science we may specially mention a magnificent collection of the photographs of sun, stars, and planets which have recently astonished and delighted astronomers. The collection included specimens of the results recently obtained by Dr. Janssen, the Brothers Henry, Mr. Common, and Dr. Gill. Among these the star-photographs by the Brothers Henry, a photograph of a sunspot by Dr. Janssen, in which the minute structure of the penumbra and bridges of a large sunspot were exquisitely shown on a scale of something like 10 feet to the solar diameter, and two exquisite photographs of Saturn, enlarged eleven times by the Brothers Henry, excited the greatest wonder.

The Solar Physics Committee sent a collection of the daily solar photographs which they are now obtaining from India and the Mauritius to supplement the Greenwich series. These photographs are on scales of 12 inches or 8 inches to the solar diameter.

Mr. Norman Lockyer exhibited some photographs of spot spectra showing the widening of the lines and the reversal of H and K; and also some photographs illustrating the first results of a new branch of work recently undertaken at South Kensington, in which it is hoped eventually to obtain photographs of the spectrum of the chromosphere and prominences without an eclipse. The photographs showed that the bright lines H and K have already been caught. Mr. Lockyer also exhibited the new split-grating spectroscope recently described at the Royal Society; the green line of thallium or the red line of lithium being shown between the D lines.

Nor must we forget to mention a selection of drawings of the sun on a large scale from those now daily made at Stonyhurst College Observatory; these were exhibited by the Rev. S. J. Perry. Special care has been devoted to the faculæ, which are drawn with a red pencil, and their position is as accurately determined as that of the spots.

Mr. Howard Grubb exhibited a model of an equatorial and observatory which he has proposed for the 3-feet refractor for the Lick Observatory. All the required motions of the telescope, dome, and rising floor are effected by water-power (represented here by clockwork) governed by an electrical arrangement, the commutator being portable and carried by observer. By this arrangement the necessity of assistants, even in case of the largest sized instrument, is obviated, and the observer himself can, from any part of the Observatory, control all the motions of instrument and dome without using any physical exertion.

Even observatory clocks were not neglected. Dr. Leonard Waldo, of Yale College, U.S., exhibited a gravity escapement adapted for use in a precision clock, in which the escapement lifts the gravity arms with a gradually-increasing velocity, and with more certainty than in the ordinary forms; and a new astronomical clock.

Finally the Eclipse Committee of the Royal Society were represented by charts of the West Indies and of the Island of Grenada, showing the path of the total eclipse of August next, arrangements to observe which are now being made.

In pure physics the *pièce de résistance* was the colour photometer, for comparing the luminosity of colours and for testing the perception of colour, exhibited by Capt. W. de W. Abney, and Major-General Festing. The form exhibited was an improvement upon the original one, which was fully described in NATURE a little time ago.

Two exhibits by Mr. A. Stroh, also optical, may next be referred to. The first was an apparatus for showing stereoscopic effects on a screen; the next was an instrument for enlarging the angular division by means of reflectors, and thereby causing an object to be seen in exaggerated relief.

Electrical science was represented by the following new electrical apparatus, exhibited by the Electrical Power Storage Company: (1) various types of cells; (2) ring contact switches; (3) automatic switch, for closing the circuit when the dynamo is running at the required speed, and for breaking it in case of accident; (4) hydrometers, specially for use with the Company's cells; (5) pocket voltmeter for cell-testing; (6) automatic switch to cut out two or more cells when dynamo is started to keep constant electromotive force on lamps.

In addition to these there were the following, contributed by Messrs. Woodhouse and Rawson:—

(1) Assortment of incandescent lamps, showing the latest developments in connection with the manufacture of incandescent lamps. (2) Small arc lamp, giving 200 to 300 c.p. or more if required: specially designed for being connected upon the same circuit with incandescent lamps of ordinary c.p., and being run by the same dynamo. These lamps can be also wound for running in series. (3) Switch-boards, illustrating the universal system introduced by Messrs. Woodhouse and Rawson. (4) Electric-lighting switches and safety-junctions, for manipulating currents of from 200 to 500 amperes and upwards.

Mr. Pitkin exhibited some very interesting portable electric lamps intended for use in coal-mines and powder-magazines. A small teak box contains three or more accumulator-cells, which, when charged, give a continuous light for ten hours. In a modified form of the invention the lamp is detached from the box containing the accumulators, and is electrically connected to it by means of a flexible cord; by this arrangement a very convenient railway reading lamp is formed, as the box can be placed under the seat or on the rack, and the lamp itself either held in the hand, or hooked to the back cushions or to the button-hole of the coat of the reader in a convenient manner.

A new electrical influence-machine, having eight disks working within a glass case, was exhibited by Mr. Wimshurst.

Electricity applied to meteorology was represented by an electrical wind-vane and indicator exhibited by Mr. F. M. Rogers. This instrument enables the direction of the wind to be ascertained at any moment, and at any reasonable distance from the vane, within a house, observatory, or office. One vane will actuate several receivers, which are quite independent of each other. Should the vane remain for many hours upon any one point no waste of current takes place; the expenditure of such being limited to the momentary impulse required to effect change of direction upon the dial of receiver.

Messrs. de la Rue and Hugo Müller showed how the chloride of silver battery could be applied to electric lighting by a quantity arrangement. Instead of using a solution of chloride of ammonium simply, the solution, containing 2½ per cent. salt, is converted into a vegetable jelly, by dissolving in it Ceylon moss (Agar-Agar) to make a stiff jelly; this supports the zinc plate. The chloride of silver in *powder* is spread evenly on the bottom of the dish on which a piece of silver foil is placed.

One of the most interesting exhibits was by Mr. Conrad Cooke, C.E., who showed Dr. Auer von Weisegg's incandescence system of burning gas. A small Bunsen flame burning about 2½ feet of gas per hour gave a dazzling light of about twenty candles by suspending in it a gauze cylinder which had been impregnated with the salt of a rare earth (probably zirconium). Tested by the spectroscope, the light showed a large excess of blue rays as compared with an ordinary gas-flame.

Voltaic cells with solid electrolytes were exhibited by Mr. Shelford Bidwell.

Great excitement was caused among the chemists by the specimens of the new element germanium and some

of its compounds, from Prof. Winkler, of Freiberg, brought by Dr. Hugo Müller. These were :—

(1) Metallic germanium ; (2) germanium monosulphide, GeS ; (3) germanium disulphide, GeS_2 ; (4) crystallised germanium, obtained by the action of hydrogen on germanium sulphide.

Germanium is claimed to be the ekasilicium predicted by Mendeléeff in his periodic law.

	Mendeléeff's ekasilicium		Germanium
Sp. Gr.	5·5	...	5·469
Atom. weight ...	72	..	72·75
Atom. val.	13	...	13·3

Mr. G. J. Symons exhibited a small pocket thermometer as constructed by Immisch. This thermometer is actuated by a minute Bourdon tube. It is shaped like a watch, is water-tight, and nearly unbreakable.

A terrestrial globe showing magnetic meridians for the epoch 1880, and general distribution of the secular change of the declination, made for the Hydrographic Department of the Admiralty, was exhibited by Staff-Commander Creak, R.N. The approximate positions of the foci of greatest secular change of the declination and vertical force—except for the Arctic and Antarctic zones—are also shown. A consideration of these foci shows the general angular motion of the north or marked end of a freely suspended needle as regards secular change.

The fact that our space is nearly exhausted, although we have only referred to about one-half of the exhibits, well indicates the care taken to make the *soirée* a success. In conclusion we refer as briefly as possible to some of the remainder :—

Jordan's photographic sunshine-recorder, with specimens of observations, exhibited by Mr. J. B. Jordan, of the Mineral Statistics Branch, Home Office.

Original geological map of the Orange Free State, and section of part of Cape Colony, by the late G. W. Stow (unpublished), exhibited by Prof. Rupert Jones, F.R.S.

Specimens of daily synchronous charts of the North Atlantic for the period of thirteen months, from August 1882 to August 1883 inclusive, now in the course of preparation by the Meteorological Office, exhibited by the Meteorological Council. The specimens show the meteorology of the North Atlantic on three summer and on three winter days.

New and interesting plants, exhibited by the Director of the Royal Gardens, Kew.

Nolls' apparatus for demonstrating secondary growth in thickness of stems ; Hopfe's Collectiones Phytomicrotomicæ, exhibited by Prof. Bayley Balfour, F.R.S.

Collection of stone-headed arms, implements, &c., from New Guinea, exhibited by Mr. H. B. Brady, F.R.S.

Diagrammatic sections showing the geological structure and physical features of parts of Arabia Petræa, and Palestine, exhibited by Prof. Edward Hull, F.R.S., Director of the Geological Survey of Ireland : (1) from the sea-coast at Askalan by Jerusalem to the Jordan Valley at Jericho ; (2) from the table-land of Southern Judæa—across the Dead Sea to the Plains of Moab ; (3) from the Gulf of Suez, near Tor, by the Mountains of Sinai, to the Plateau of Badiet et Tih

Apparatus for measuring the luminosity of leaves, invented and exhibited by Dr. Gorham, to show that the white light reflected from leaves can be measured in *cents.* of a circle by the novel use of a *gray ring*, and that by putting this luminosity in the form of an equation its equivalents in colour are discovered, which, when placed in sectors on a circular disk and rapidly rotated on a wheel, are seen to match the colour of the leaf from which the luminosity has been originally reflected.

Specimens of miners' electric lamp, invented and exhibited by Mr. Swan.

Dr. Sohlberg's celestial globe of glass ; Dr. Schmidt's tellurium ; cosmographic clocks for showing universal time ; contoured map of the English Lake District, constructed by Mr. Jordan ; enlarged original photographs taken by Mr. Joseph Thomson in his recent journey up the Niger ; replica of Frankfort globe, of date 1520 ; two large diagrams—(1) Roraima, British Guiana, by Mr. Im Thurn, (2) a similar formation in the north of Brazil, by Mr. Wells ; collection of minerals from summit of Mount Roraima, exhibited by the Royal Geographical Society.

NOTES

It is with much regret that we announce the death of Surgeon-Major T. Lewis, Medical Staff, Assistant Professor of Pathology in the Army Medical School at Netley. Within the last few weeks the Council of the Royal Society decided to recommend Dr. Lewis for their Fellowship, in recognition of the importance of his various contributions to science Dr. Lewis had only just reached the forty-fifth year of his age at the time of his death.

The death is announced of Dr. E. Linnemann, Professor of Chemistry at Prague, which occurred on April 27. Among his papers a letter was found addressed to the Vienna Academy of Sciences containing a communication on a new chemical metallic element called austrium (Aus). This new element was prepared by the late Prof. Linnemann from orthite of arendal. The spectrum of austrium shows two violet lines ; the wave-lengths were found to be, for Aus α, $\lambda = 416·5$, and for Aus β, $\lambda = 403·0$. According to a note made by Prof. F. Lippich, of Prague, who communicated Prof. Linnemann's letter last week to the Vienna Academy, three not yet identified lines—$\lambda = 415·56$, $\lambda = 416·08$, and $\lambda = 416·47$—are shown in Ångström's atlas of the normal spectrum of the sun in the neighbourhood of the Aus α line ; the last of them might be supposed coincident with the Aus α line ($\lambda = 416·5$).

M. Chevreul, who on August 31 will be a centenarian, was on Monday afternoon presented by his colleagues of the Academy of Sciences with a bronze bust of himself, executed by Paul Dubois. Admiral Jurien de la Gravière, one of the senior members—his age being 73—made the presentation, and warmly complimented M. Chevreul on his long and distinguished career, which made France proud of him and of herself. M. Chevreul, who was much affected, made a brief acknowledgment of the honour done him.

The Swedish Academy of Sciences celebrated its centenary on April 5 last, having been founded by Gustavus III. on the eve of the French revolution.

The paper to be read at the ordinary meeting of the Society of Arts on May 26 will be "The Purification of Water by Agitation with Iron, and Sand Filtration," by William Anderson, M.Inst.C.E. On Tuesday, May 25, a paper on "Cyprus since the British Occupation," will be read by G. Gordon Hake, before the Foreign and Colonial Section. In the Indian Section, Capt. Richard Carnac Temple's paper on "Every-day Life of Indian Women, as Revealed in their own Sayings," will be read on May 21.

William Landsborough, whose name is known in connection with Australian exploration, died at Caloundra, near Brisbane, on March 15. His father was a Scottish naturalist of note. Having gone to Australia, Landsborough in 1860 discovered the head of the Thompson River, and in the following year traced the Gregory and Herbert Rivers to their sources. He then undertook to lead the expedition in search of Burke and Wills, and traversed the continent from the Gulf of Carpentaria to Melbourne. Subsequently he was appointed to a post in the public service of Queensland, and was voted 2000*l.* for his explorations in that colony.

A telegram from Catania of May 18 states that Mount Etna had been in eruption since 11 o'clock that morning. A very active discharge of vapour and cinders was proceeding from the western side of the central crater.

We are glad to learn of the formation of a Natural History Society at Yokohama. The marvel is that so long a time has been allowed to elapse before such a society was founded there or in Tokio, for probably there are no communities in the world in which the proportion of men of science is so high as here. But no doubt the local Asiatic Societies and the Seismological

Society absorbed much of the scientific work of the residents. The first meeting of the new Society was held at the beginning of last month, when Prof. Milne delivered a lecture on the geology of Japan, which is reported in full in the *Japan Weekly Mail* of April 3, and which is too comprehensive and detailed to lend itself to adequate treatment in a short note. We observe, however, that in speaking of the difference between the fauna of Yezo and of the other islands of the Japanese archipelago he suggests that the line between the two should be called Blakiston's line (from Capt. Blakiston, who first pointed it out), on the model of Wallace's line between the Javan and Australian fauna. Prof. Milne proposed this nomenclature a few years ago, and it is now adopted by some German publications. The difference, indeed, is not so great as that marked by Wallace's line, still it is of considerable importance. Thus in Japan we have the monkey, the sheep-faced antelope, the bear, and the pheasant; but on the other side of the straits the bear is a totally different one, the monkey, the pheasant, or the antelope is not found, and a totally different fauna exists. The suggestion of marking the dividing line with Capt. Blakiston's name is an appropriate one.

Dr. Trimen's Report for 1885 on the various botanic gardens in Ceylon, of which he is Director, is a very satisfactory document, as such reports from our colonial gardens generally are. It contains a considerable number of meteorological observations, and describes the arrangements made to carry out the scheme of a garden at Badulla, in the new province of Ceylon, for which funds have been voted. The usual report on the distribution of plants and seeds is given, and then comes a long list of additions to the collections of plants at the various gardens, which is followed by some interesting notes on economic plants and products. Under this head he makes various recommendations and suggestions which will no doubt be of the utmost value to perplexed planters who are assailed on all sides by dangers which are only too apparent, but which it requires scientific training and investigation to combat. Thus he advises that tea and cinchona should not be grown together, as it is only in such cases that the planter's scourge, *helopeltis*, does any appreciable damage to tea. On the other hand, he thinks that the diminution in the cultivation of cacao, through dread of *helopeltis*, is unreasonable, for the insect only attacks cacao grown in the open, and not that grown under the shade of trees, for example The Ceylon Herbarium has been arranged during the year in accordance with Dr. Trimen's "Systematic Catalogue of Ceylon Plants," and in the work of rearranging he was able to put together a series of notes describing about 280 additions to the flora of Ceylon, and 40 new species or varieties. That much yet remains to be done is obvious from the fact that during 1885 fifteen additional have been discovered in the island, many of which are striking plants. The Government has approved the formation of an exhibition containing specimens of the plants, grains, &c., of the island. Like many other colonial officials, Dr. Trimen has been busy during the past year preparing for the Colonial and Indian Exhibition, where the series of woods in the Ceylon section was prepared by him.

According to a letter received from Baron Schwerin, the well-known Swedish geographer, who is at present on a scientific mission to the Congo for his Government, he observed the great November meteor-stream on the 27th of that month, at 8 p.m., off Cape Palmas, on the Guinea coast (lat. 4° 29′ 9″ N., and long. 7° 44′ 16″ W.). He describes it as the most magnificent spectacle he ever beheld, "the whole sky being furrowed from nearly north to south by falling stars, looking like gigantic fireworks, in which thousands of 'star-rockets' were burnt off."

On the evening of April 27 a bolide was observed in several parts of the north-western provinces of Russia. A large ball of bluish colour was moving from south to north; in about two seconds it changed its colour into a bright electric-white, and suddenly broke in many pieces and disappeared.

In consequence of the ice breaking on the Volga, a considerable number of naphtha-barges were wrecked this spring, and about 2,000,000 poods, or 33,000 tons, of naphtha are now floating along the river. According to the observation of past years the presence of such a large amount of hydrocarbon in the water will produce a very disastrous effect upon Russian fisheries, not only along the Volga, but even on the northern coast of the Caspian Sea.

The tendency amongst modern Oriental scholars is to trace the origin of Chinese arts, sciences, and civilisation to Babylonia. The theory, however, is not accepted by many of the most eminent Chinese scholars. In the last number of the *China Review*, Dr. Edkins of Pekin gives his reasons for assigning a Babylonian origin to Chinese astronomy and astrology. Amongst these are the following:—Both peoples divided our day and night into twelve hours; the sun-dial is a Babylonian invention, and reached China at a very early date; the intercalary month belonged to the Accadians, and is found in the first sections of the Chinese Book of History. Geminus states that it was the dwellers on the Euphrates who discovered that, after 223 lunations or eighteen years, eclipses of the moon recur in the same order. The early use of the intercalary month by the Chinese implies that either they, or those from whom they derived it, knew this fact about lunations. Early Chinese astronomy, Dr. Edkins thinks, is too good to come from the Chinese of those days, and, on various learned grounds which he states, he concludes that the double hour, the astrolabe, the dial, the intercalary month, and the knowledge of the length of the year were all communicated from Babylon to China at different periods by land or by sea between about B.C. 2200 and B.C. 820.

The invitation to Norwegian sea-captains to make barometrical observations during the eclipse next August was not issued by the Norwegian Meteorological Society but by our Norwegian contemporary *Naturen*, to which the returns are also to be sent.

Messrs. Whittaker and Co., and Messrs. Bell announce in their new "Series of Hand-Books for Practical Engineers" Dr. Jul. Maier's book on "Arc and Glow Lamps." It will be a complete hand-book on the subject, especially relating to its practical applications, giving the latest results and improvements. The next volume in the same series will be Mr. Gisbert Kapp's "Electric Transmission of Energy," which is promised early next week. Mr. William Anderson has revised and added some new matter to his "Lectures on the Conversion of Heat into Work," which excited much attention when delivered last year at the Society of Arts. They will be published in book form by Messrs. Whittaker and Co.

The additions to the Zoological Society's Gardens during the past week include two Shaw's Gerbilles (*Gerbillus shawi*) from North Africa, presented by Mr. W. R. Ogilvie Grant; two Black-backed Jackals (*Canis mesomelas*) from South Africa, presented by Mr. F. Mosenthal; a Ring-tailed Coati (*Nasua rufa*) from South America, presented by Mr. T. P. Lymn; a Brown Condor (*Sarcorhamphus æquatorialis*) from Chili, presented by Mr. R. J. James; two Red Kangaroos (*Macropus rufus*) from Australia, two Grey Parrots (*Psittacus erithacus*) from West Africa, an Indian Rat Snake (*Ptyas mucosa*) from India, a Green Lizard (*Lacerta viridis*), European, deposited; an Indian Coucal (*Centropus rufipennis*) from India, an African Tantalus (*Pseudotantalus ibis*) from West Africa, four Black-tailed Godwits (*Limosa ægocephala*), European, a Spotted Eagle Owl (*Bubo maculosus*) from South Africa, purchased; an Eland (*Oreas canna*), four Chilian Pintails (*Dafila spinicauda*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

THE BINARY STAR α CENTAURI.—Mr. E. B. Powell, to whose observations and calculations we are largely indebted for our present knowledge of the orbit of this celebrated binary, has recently published new elements giving the period as 87·438 years, the time of periastron passage at 1875·447, and eccentricity = 0·544. These elements appear to satisfy fairly the recorded equatorial measures made from 1834 to 1885 (as well as most of the ancient observations), with which Mr. Powell has compared them; but all the available observations have not been used—for instance, the Sydney measures subsequent to 1877 have been omitted, as some influence appears to have operated to throw out these measures from accord with those taken at other observatories. Mr. Powell considers that the evidence is tolerably strong against the period of α Centauri being only some seventy-six years (as given by the Downing-Elkin orbit), but thinks that in six or eight years, if careful measures be taken, the point will be settled as to whether the period is about seventy-six years or exceeds eighty-six years.

A NEW BELGIAN OBSERVATORY.—The Cointe Observatory, attached to the University of Liége, has been founded at the instigation of M. Folie for the purpose of affording instruction to the students in astronomy and geodesy, as well as of furnishing original observations. M. Folie is Director of this Observatory, as well as of the Royal Observatory at Brussels. The Cointe Observatory is furnished with a meridian circle by Cooke, the object-glass of the telescope of which is of 6 inches aperture, and the circle 0·8 metre in diameter. The Observatory also possesses a 10-inch refractor by Cooke, of the optical qualities of which M. Folie speaks in the highest terms. The astronomers attached to this institution are MM. L. de Ball and P. Ubaghs, the former of whom observes with the 10-inch equatorial, and the latter with the meridian circle. The Observatory is destined, we hope, to do good work in both these departments of astronomy.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 MAY 23–29

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on May 23

Sun rises, 3h. 59m.; souths, 11h. 56m. 29·6s.; sets, 19h. 54m.; decl. on meridian, 20° 37′ N.: Sidereal Time at Sunset, 11h. 59m.

Moon (at Last Quarter on May 25) rises, 23h. 33m.*; souths, 4h. 8m.; sets, 8h. 48m.; decl. on meridian, 16° 38′ S.

Planet	Rises	Souths	Sets	Decl. on meridian
	h. m.	h. m.	h. m.	° ′
Mercury	3 25	10 37	17 49	13 12 N.
Venus	2 35	9 4	15 33	5 4 N.
Mars	12 10	18 54	1 38*	7 54 N.
Jupiter	13 24	19 42	2 0*	2 53 N.
Saturn	6 13	14 25	22 37	22 48 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

May	h.	
23	20	Jupiter stationary.
29	13	Venus at greatest distance from the Sun.

Variable Stars

Star	R.A.	Decl.			
	h. m.	° ′		h. m.	
U Cephei	0 52·2	81 16 N.	May 25,	3 18	*m*
U Monocerotis	7 25·4	9 32 S.	,, 28,		*M*
U Ophiuchi	17 10·8	1 20 N.	,, 26,	2 16	*m*
			,, 26,	22 24	*m*
X Sagittarii	17 40·6	27 47 S.	,, 26,	2 25	*m*
			,, 29,	0 0	*M*
U Sagittarii	18 25·2	19 12 S.	,, 26,	2 25	*M*
			,, 29,	21 40	*m*
β Lyræ	18 45·9	33 14 N.	,, 28,	21 30	*m*
R Lyræ	18 51·9	43 48 N.	,, 28,		*m*

M signifies maximum; *m* minimum.

Meteor Showers

The *Draconids*, radiant R.A. 280°, Decl. 54° N., and the *Cygnids*, R.A. 301°, Decl. 37° N., are due this week. Meteors from radiants near α Ursæ Majoris, R.A. 175°, Decl. 64° N.; in Lyra, R.A. 273°, Decl. 34° N.; and in Lacerta, R.A. 329°, Decl. 48° N., have also been observed at this season.

GEOGRAPHICAL NOTES

THE Kermadec Islands, which have during the past week been occupied by the orders of the Home Government by Admiral Tryon, Commander-in-Chief on the Australian station, are a group of rocky islets about 600 miles to the north-north-east from the North Island of New Zealand, and lying on the steamer route from Christchurch or Wellington to Fiji. They are due east of Norfolk Island. The principal islands of the group are Raoul or Sunday Island, the position of which is put at 29° 12′ S. and 178° 15′ W. It is described as about 12 miles in circumference, rugged and very steep, without an anchorage. It is said to be covered with wood, and to be uninhabited except for a few white men, waifs and strays from the ocean, shipwrecked sailors, deserters, &c. The other islands of the group are Macauley, the Curtis Islands, Havre, and Espérance.

AT the last meeting of the Geographical Society of Paris a letter was read from M. Borelli, who is at present travelling in that part of Eastern Africa where M. Barral has been murdered. M. Brettes, referring to his explorations of the Grand Chaco between the Argentine Republic, Bolivia, Brazil, and Paraguay, said he had discovered a great salt lake which he proposed to call Lake Crevaux, and three rivers, hitherto unknown, the most important of which would be called Rio Lesseps. M. de la Grye stated the propositions adopted by the committee appointed to study the reforms necessary for the adoption of a common international orthography for maps. These were—(1) any change for European countries is recognised as impossible; (2) in Asia, Africa, and America it is proposed that the French *u* should be replaced by the diphthong *ou*, the value of the French vowels *a*, *e*, *i*, *o* remaining unaltered; (3) in the geography of the Far East the sound of the *u* with a diæresis is represented by *oe*, *g* and *l* are always hard, *ch* is reproduced by *sh*, amongst the gutturals the soft ones are represented by *gh*, the hard by *kh*; (4) as far as possible, by the aid of this common alphabet, the most generally used pronunciation of places, towns, rivers, mountains, &c., shall be reproduced. Prof. Ersler of Copenhagen described the results of his investigations into the cartography of Denmark from the time of Ptolemy.

THE French staff officers are busy with the continuation of the Paris meridian to Laghouat, about 4° south of Algiers. When this work is finished this line will be measured with precision from the Orkneys to this locality. The length determined will not be far from 30°, or about three times its original extension, which was 10°, from Dunkirk to Formentera.

TWO Finnish *savants*, Drs. Hammarström and Ehnberg, have just returned to Helsingfors from a scientific journey in Eastern Siberia and China, whence they bring valuable scientific collections.

NEWS received from Baron Schwerin, the Swedish scientific explorer on the Congo, informs us that he landed from the Liverpool steamer in December last at the mouth of the River Chiloango, whence he proceeded on foot through the districts of Cacongo and Cabinda to Banana. During the journey along the coast the Baron succeeded in making many valuable observations of the shore-lines or terraces on the gradually-rising coast, and of the effects of the tide on the plastic formation of the sandy fore-shore. He also paid special attention to the study of the great influence which ocean currents exercise on the direction of the flow of rivers in their lowest course.

COUNT SAMUEL TELEKY is organising, at Pesth, an Expedition for the exploration of Central Africa; the fitting out will be completed by the end of May, and 100 well-armed men will reach Zanzibar in the course of June. Capt. Hähnel, of the Austrian Navy, will take part in the Expedition, and two boats will be taken out in pieces. It is believed here that the Expedition will not confine itself to scientific explorations only.

THE three numbers of the *Journal* of the Geographical Society of Tokio for last year which have been recently published do not contain much of special interest to English students of geography, although the papers could hardly fail to instruct a Japanese audience, which can hardly be expected to be as familiar with the colony of Victoria, or with the progress of Russia to the southward, as Western readers. The report of the Japanese delegate to the Prime Meridian Conference at Washington is also printed. Of special papers there are two: one on the Bonin Islands, called Ogasawarajima by the Japanese; another on the area within which Mount Fuji is visible.

THE IRON AND STEEL INSTITUTE

THE Iron and Steel Institute held its meeting on the 12th, 13th, and 14th inst., under the presidency of Dr. J. Percy, F.R.S., in the Theatre of the Institution of Civil Engineers.

The President made some introductory remarks having reference to the papers about to be read. He had strong hopes that, from a scientific point of view, great results were likely to flow from investigation of the microscopic structure of iron and steel, as it was only by physico-chemical investigation that our present ignorance of the causes of many phenomena relating to metal would be lessened or dispelled. He was peculiarly glad to read Mr. Turner's paper, as he had had the honour, when first addressing the Institute, of suggesting the solution of specific problems relating to iron and steel which had been ably attempted by the author; he should be glad to see medals or rewards conferred on those who solved problems emanating from the Institute. He made special reference to Mr. C. P. Clarke's paper, which we hope to print *in extenso*. He had great pleasure in drawing attention to Sir Henry Bessemer's gift to the Institute of a series of specimens illustrative of the process universally known by his name, which he exhibited at South Kensington some time ago. The President very shortly referred to what Sir Henry had done for metallurgy, and called upon the members to join in cordially thanking him for his gift to the Institute, which was done with acclamation.

With regard to the prevailing depression in trade, he thought over-production was the main cause of the evil in question. Considering the enormous power the iron and steel trades, for instance, possessed for production, it was not surprising that over-production should take place. Besides, what had taken place in our own country had also occurred to a greater or less extent in Germany, Belgium, France, Austria, Russia, and especially the United States of America

British workmen had a special enemy to contend against in the fierce competition from abroad, where men labour for less wages and work longer hours. He hoped that the problem would be solved, not by our countrymen having to be paid less for their labour, but by the labourers in foreign countries rising to our level, when our trades would have less to fear from foreign competition.

Passing from the over-production of iron and steel, the President referred to the fact that the surface of the earth was limited, whilst the human race was constantly increasing, and as the world could only sustain a certain population, so portions of it could do no more; he was of opinion that what was really at the bottom of the troubles of Ireland was the sentiment of Irishmen trying to live where they could not gain their livelihoods, when there were millions of acres in our colonies which they could cultivate and be happy upon. Shortly referring to the Colonial and Indian Exhibition, the speaker concluded, a vote of thanks for his address being moved by Sir Isaac Lowthian Bell and seconded by Sir Bernhard Samuelson.

The Bessemer Medal for the year was awarded to Mr. Edward Williams, who was unfortunately prevented by ill-health from coming to the meeting to receive it.

There was a very large number of papers on the agenda, some of which had to be deferred. Amongst the papers read and discussed some were important not only technically but scientifically. Mr. P. W. Flower's paper on the origin and progress of the manufacture of tin plates is hardly of this character, but it is interesting both from an archæological and industrial point of view. Aristotle, Pliny, the Phœnicians, Herodotus, and Diodorus Siculus have all made reference to this manufacture. In more modern days we find it flourishing in Bohemia in 1620, from which country Yarranton introduced it into England about 1665, thus fortunately succeeding in benefiting the iron trade of Wales and the tin trade of Cornwall, which were both much depressed. Later on, the use of coal instead of charcoal, of vitriol for pickling-purposes in place of barley-meal, of Siemens's soft steel for charcoal iron, of Bessemer steel in place of puddled bar, have all had their influence on this industry. Ninety-six works, with 320 mills in all, work up about half a million tons of British steel and iron annually into tin plates. The production last year was over 7,000,000 boxes, of which probably 3,000,000 were used in the manufacture of 875,000,000 of 1 lb. canisters. "By means of these canisters Europe receives largely of beef from the Western prairies, salmon (in shiploads) from Oregon, mutton from the plains of Australia, fruits of all sorts from California, lobsters from Boston and Nova Scotia, oysters and peaches from Baltimore, sardines and green peas from France, pine-apples from Mauritius, apricots from Lisbon, milk from Switzerland, jams from Tasmania, and many other products of foreign soil, which complete the list of what the French have called *conserves alimentaires*."

Mr. Hamilton Smith, jun., in his paper on wrought-iron conduit pipes, refers to the method of hydraulic mining introduced in California in 1852. It may roughly be defined as the discharge of jets of water, actuated by gravity with a considerable head, against a bank of auriferous gravel, the water acting first as an excavator, and afterwards as a carrier of the washed material. The supply of water for these jets was at first conducted through hose made of heavy cotton duck cloth, which was strengthened by outer nettings of cordage when the pressure was large. In 1853 an ingenious miner laid in his main a line of pipe consisting of joints of ordinary stove-pipe, made of very thin sheet-iron lightly fastened together with cold rivets; the joints being united stove-pipe fashion. This pipe answered admirably, and in a short time all the hydraulic gravel mines in California obtained the pressure for their water-jets by means of thin sheet-iron pipes. As a protection against rust, each joint is immersed for several minutes in a bath of boiling asphalte and coal-tar; a little rosin is added when a glassy surface is desired, and sometimes a little fish-oil. After successful practice in the mines had demonstrated the advantages and capabilities of wrought-iron pipes, they were used for permanent conduits both for conducting water to mining districts across deep mountain gorges, and also for the supply of cities. San Francisco, a place of some 300,000 inhabitants, receives its water through two lines of such pipes, and a third pipe, many miles in length, and of large diameter, is now being laid for an additional supply.

"On a Neutral Lining for Metallurgical Purposes" was the title of a paper in which M. Ferd. Gautier, after describing various linings of an acid, basic, reducing, and oxidising character, refers to one in which chrome iron is the main constituent. From a physical point of view chrome ore is essentially refractory; heated in lumps it does not crumble to pieces, however high the temperature. In general metallurgy, where no alkalies in notable quantities are present, chrome iron is a refractory material of a specially neutral character, since neither acids nor bases act upon it. The chrome iron is employed shaped in pieces, and also as a mortar in combination with lime. The use of this material in the basic open-hearth process has been kept secret for some time; it was exhibited last year at the International Inventions Exhibition.

The President's paper on steel wire of high tenacity referred to experiments on the tensile strength and chemical composition of wires of various thickness. The mechanical tests were made at the request of the author by Col. Maitland, R.A., and the analyses by Sir Frederick Abel. The wire was of a very pure character, there being a percentage of total carbon 0·828, manganese 0·587, silicon 0·143, sulphur 0·009, copper 0·030, without a trace of phosphorus. The tensile strengths of the wires increased as their thickness diminished, as shown by the following table:—

Diameter in fractions of an inch	Tensile strength in tons per sq. inch
0·093	154
0·132	115
0·159	100
0·191	90

The difficulty in accounting for the increase of strength with diminution of diameter in wire-drawing is the circumstance that the density of the material diminishes during this process.

Mr. T. Blair's paper on certain necessary products of blast-furnaces, and Mr. Bauerman's note on a rare blast-furnace slag of the composition of gehlenite, were discussed together.

The paper by Mr. John Head on blow-holes in open-hearth steel brought about a very animated discussion. The blow-holes in steel, the author explained, are due to the contraction of the metal on cooling, or to the presence of imprisoned gases in its mass. Those of the first kind are removed by welding, when the steel is subjected to pressure. Those of the second kind the author maintains to be similar to what is technically known as "seedy boil" in glass, and may be removed in the manufacture of steel by not allowing the flame to touch the fused metal, just in the same way as they have been

got rid of in the manufacture of glass by the use of the radiating furnace. It was suggested, in explanation of certain mysterious failures which had occurred in steel, the possibility that in these cases the gaseous blow-holes in an ingot may have sorted or arranged themselves in a series, thus forming a line of weakness in the plate or bar, which has failed along that line when subjected to a strain much below that which test-pieces from the same plate or bar would withstand. In conclusion the author had no doubt "that, by manufacturing open-hearth steel free from gaseous blow-holes, the metal produced would be much stronger and more reliable than that made by contact of flame, and the result would be a greater confidence in its use." In the discussion of this paper a unanimous verdict was given in favour of steel alike by representatives of the Admiralty, the Board of Trade, and Lloyd's Registry, who are the official judges of the metal, and by shipbuilders and boiler-makers who have found the material more trustworthy than the best iron. As regards the manufacturers, one acknowledged to the fact of there being a large difference in the total carbon, according as a sample was taken from one end or another of a large ingot, whilst another speaker had found the metal to be more regular if made in a radiation than a contact of flame furnace. As the author stated in his reply, the users were evidently even better satisfied with the material suoplied them than the makers, which is certainly a favourable sign.

Mr. F. W. Webb's paper on the endurance of steel rails added further testimony to what had already been said in favour of steel. In 1876 the London and North-Western Railway put down 31,391 tons of iron and steel rails together, twelve months after which iron rails entirely disappeared, whilst the estimated requirements for this year are only 11,600 tons. The small quantity of rails required for renewals account in some measure for the depression in the steel-making trade. On the other hand, if steel sleepers are found to answer, and the author sees no reason why they should not, 45,000 steel sleepers having been put down on the London and North-Western line, and giving every satisfaction, orders for steel sleepers should in great measure make up for want of orders for rails.

Dr. H. C. Sorby drew attention to the application of very high powers to the study of the microscopical structure of steel, having employed a power of 650 linear which, being about ten times that used in his previous researches, opened out a new field for research. The chief facts were best seen in the case of an ingot of steel of medium temper. On fracture, comparatively large crystals were visible, radiating from the surface to the interior. When a properly-prepared microscopical section was viewed with a moderate power, it was easy to see that, after having crystallised out from fusion at a high temperature, these large crystals broke up on further cooling into much smaller ones. What was now seen with very high powers was that these smaller crystals finally split up into alternating very thin plates. Taking all the facts into consideration, it appeared as though a stable compound of iron with a small amount of carbon existed at a high temperature, which at a lower broke up into iron combined with a larger amount of carbon, and into iron free from it. If these two products had not differed so much in hardness, or if the alternating plates had been considerably thinner, or if definite plates had not been formed, such a compound structure would never have been suspected. It has probably never been specially looked for in other substances, and might exist without being visible, even with the highest and best magnifying powers. To give a good idea of the size of the plates, he would refer to what was seen in a longitudinal section of medium steel forged from an ingot 3 inches in diameter down to a bar 1 inch square. When broken, it showed a very fine grain, and when a prepared section was examined with a moderate power, this grain was seen to be due to crystals often about 1/1000 inch in diameter, which were not drawn out or distorted, as they would have been if they had existed previously to final cooling after hammering, and as they were distorted if the steel were hammered at a lower temperature. Examined with a power of 650 linear, these crystals only 1/1000 inch diameter were seen to contain something like 60 of the alternating plates, and even this extremely delicate structure showed little or no trace of distortion. Of course it was impossible to separate and analyse such thin plates, and reliance must be had on induction to furnish a knowledge of their nature. His reason for concluding that the hard plates contained combined carbon was that they were not seen in iron free from carbon; they increased in amount with increase of carbon, and were seen to the greatest perfection when there was a considerable amount in a combined state.

Mr. Thomas Turner's paper on the constituents of cast-iron is an attempt now made for the first time to systematise in some measure our knowledge of the constituents generally present in cast-iron, to estimate the mechanical value of any given specimen of which the chemical analysis is known, and conversely, when any given mechanical properties are desired, to predict the most suitable composition for the material. In connection with this subject two opposite opinions have been advanced by different authorities, both of which found expression at the Glasgow meeting of the Institute. On the one hand, it was suggested that probably the best mechanical properties would be obtained in a cast-iron which contained if possible nothing but carbon and iron, all other elements being regarded as impurities. On the other hand, it was said that possibly very considerable quantities of other elements might be added, even upwards of 10 per cent., without rendering the metal unfitted for the founder's use. It might be, if chemically pure iron could be obtained, that the first suggestion would be correct, and possibly if the various constituents could be added in just such proportions as to neutralise each other's ill-effects, as under such circumstances they are capable of doing, then the second suggestion might likewise prove true. As a matter of fact, pure iron cannot be manufactured, and the ill-effects of large proportions of foreign substances cannot be neutralised. A cast-iron of tolerable purity can, however, be produced, from which, by variations in the proportions of the constant constituents, a metal of desired character may be prepared. The author treats in detail of the influence of carbon, manganese, phosphorus, silicon, and sulphur, all of which are invariably present in greater or less proportion. Of these, carbon is the most important constituent, and remarkable differences are produced by variations in the proportions of combined carbon and graphite. For the more ordinary cast-iron the amount of total carbon varies from about 3 to 3·8 per cent., a lower proportion being generally due to some irregularity in the working of the blast-furnace. The relative proportion of graphitic to combined carbon can only be affected in two ways—by difference in the methods of fusion after cooling, and by variations in the proportions of other elements present. Maximum general strength, that is, considerable crushing strength combined with high tensile strength, is obtained with not less than 0·4 per cent. of combined carbon, the metal being sufficiently soft to work with the tool; with more combined carbon the metal becomes harder, its crushing strength increases while the tensile diminishes. The amount of graphitic carbon depends upon the total and combined, but, in the majority of cases, 2·6 per cent. for crushing strength, 2·8 per cent. for general strength, and 3 per cent. for strength and softness, will be found best. It is to be remembered that any required proportion of combined carbon may be obtained by altering the amount of silicon on the one hand, or of manganese and sulphur on the other, the former diminishing and the latter increasing it. As regards silicon, the experiments show that, if high crushing strength is required, it can be obtained by a low percentage of silicon; if a high tensile strength is required the silicon should be somewhat higher, while for softness, smoothness of surface, and fluidity a still higher proportion is necessary. The author is of opinion that, although phosphorus is objectionable in wrought-iron and steel, it is not so in cast-iron, the specimens which possessed the highest average quality being all moderately phosphoric irons, averaging from 0·19 to 0·72 per cent., 0·3 per cent. being a very suitable average proportion for strong iron; the amount must be proportioned according to the object the founder has in view. A small quantity of sulphur is known to produce hard white iron, owing to an increase in the amount of combined carbon, acting therefore, when in small quantity, in a manner almost exactly opposite to that of silicon. Sulphur and silicon are to a considerable extent mutually exclusive of each other in cast-iron. Thus the addition of sulphur to siliceous iron causes the separation of graphitic matter containing silicon, while the addition of silicon to an iron rich in sulphur causes the separation of graphitic matter rich in sulphur, one part of sulphur neutralising the effect of from five to ten parts of silicon. From 0·2 to 0·75 per cent. of manganese appears to exercise no injurious effect in the majority of cases, and may even be beneficial. The author considers the following to be proved, that pure cast-iron, *i.e.* iron and carbon only, and cast iron containing excessive amounts of other constituents, would not be suitable

for foundry work; that the ill-effects of one constituent can at best be only imperfectly neutralised by the addition of another constituent; that there is a suitable proportion for each constituent present in cast-iron, depending upon the character of the product desired, and upon the proportion of other elements present; and that variations in the proportion of silicon afford a reliable and inexpensive means of producing a cast-iron of any required mechanical character which is possible with the material employed.

Krupp's hot-blast pyrometer, which was shortly described, consists of an arrangement by which the hot blast is drawn with a fixed proportion of cold air into a chamber, the temperature of which, being measured with an ordinary thermometer, gives that of the hot blast by calculation.

ON DISSOCIATION TEMPERATURES, WITH SPECIAL REFERENCE TO PYROTECHNICAL QUESTIONS[1]

IN bringing the subject of dissociation before the Royal Institution of Great Britain, the author proposed to confine himself to its influence on combustion and heating, that is to say, to its effects on combustible gases and the products of combustion, and on furnace work generally. His researches had been made for the most part in connection with large gas furnaces constructed according to his new system of working with radiated heat, or what may be otherwise called free development of flame. In the first or active stage of combustion the flame passed through a large combustion chamber (all contact with its surfaces being avoided), and parted with its heat by radiation only; while in its second stage the products of combustion were brought into direct contact with the surfaces and materials to be heated, by which means the remainder of its heat was abstracted. This, in a few words, was a description of the method of heating with free development of flame. In perfecting this system of furnace, the principle of which was in many respects the reverse of that generally accepted, both as regards construction and working, he had to examine into the accuracy of certain scientific theories which could not be brought into harmony with the actual results he obtained.

Adopting the generally-accepted theory of combustion, according to which a flame consists of a chemically-excited mixture of gases, whose particles are in violent motion, either oscillating to and from each other, or rotating around one another, it followed that any solid substance brought into contact with gases, thus agitated, must necessarily have an impeding effect on their motion. Motion being the primary condition of combustion, the latter would be more or less interfered with, according to the greater or less extent of the surfaces which impede the action of the particles forming the flame; in the immediate neighbourhood of such surfaces the combustion of the gases would cease altogether, because the attractive influence of the surfaces would entirely prevent their motion; farther off, their combustion would be partial, and only at a comparatively great distance the particles of gas would be free to continue unimpeded the motion required to maintain combustion. On the other hand, the surfaces themselves must suffer from the motion of the particles of gas producing the flame, for, however small these particles might be, they produce, while in such violent motion, an amount of energy which acting constantly would in time destroy the surfaces opposed to them, just as "continual dropping wears away stone." This circumstance fully accounted for the fact that the inner sides of furnaces, and the materials they contained were soon destroyed, not by heat, but by the mechanical, and perhaps also by the chemical, action of the flame. It would seem strange that the heating power of a large volume of flame should be so much interfered with by the contact of its outer parts only with the inner sides of a large furnace chamber, if there was not another cause besides imperfect combustion to reduce the heating effect of a flame which touched the surfaces to be heated. A flame when in a state of combustion radiated heat not only from its outer surface, but also from its interior by allowing the heat to radiate through its mass. In this manner every particle of flame sent its rays in all directions, but if the flame itself touched anywhere combustion ceased there, free carbon was liberated and produced smoke which enveloped that part and prevented the rays of heat of the other portions of the flame from reaching it.

[1] Lecture by Mr. Frederick Siemens at the Royal Institution, Friday, May 7.

The author had avoided for various reasons referring to the subject of dissociation until recently, although it had been brought forward by several writers, and used as an argument against his new system of furnace; as according to these writers it would appear to be impossible to produce such exceedingly high temperatures as he claimed to reach. He had long held the opinion that appearances of dissociation not being observable in furnaces heated by radiation, but occurring in furnaces in which the flame was allowed to come into contact with surfaces, must be due to the action on the flame of those surfaces at high temperature. He was led to this conclusion partly from his own observations, and partly from descriptions of dissociation observed by others, amongst whom was his brother, the late Sir William Siemens, who described a case of dissociation (see lecture delivered March 3, 1879, at the Royal United Service Institution, entitled "On the Production of Steel, and its Application to Military Purposes") which occurred in a regenerative gas furnace constructed according to their old views of combustion and heating. *The conclusion at which he had arrived was, that solid surfaces, besides obstructing active combustion, must also at high temperatures have a dissociating influence on the products of combustion.*

In order to obtain information on this subject he examined the laws and theory of dissociation, and endeavoured to bring the various results obtained by scientific authorities into agreement with one another, and with his own experience, but failed entirely in doing so. The temperatures of dissociation of carbonic acid and steam, the two principal gases forming the products of combustion when ordinary fuel was used, vary very much according to these observers, and the results he had obtained in practice were different from most of them. He hoped to prove that the temperature at which dissociation sets in is, in most cases, much higher than generally admitted; and that the authorities he was about to refer to had omitted in almost all the experiments they had made to take into proper consideration one element which was liable to alter materially the results obtained by them. *This element was the apparatus used for those experiments as regards its surface, form, and material.*

In considering the question of dissociation, he proposed to commence with Deville, who first discovered and called attention to the dissociation of gases at high temperatures. He made numerous experiments with various gases, and fixed certain temperatures at which he found that either complete or partial dissociation took place. Without going into details, he might mention that Deville required to use vessels and tubes of definite dimensions, material, and structure, in order to obtain the results stated. One experiment had to be made with a porous tube, another required the use of a vessel with rough interior surfaces, or containing some rough or smooth material. In this way Deville arrived at a great variety of results, and although he did not state that the rough surfaces, or porous tubes, or the solid material placed inside the vessels which he employed, had any particular influence on the temperature at which dissociation took place, yet it would appear that he could not obtain his results without having recourse to those means. Deville's results depended very much upon the various kinds of surfaces he used in his experiments, if they were not entirely brought about by them; these experiments, moreover, were of a very complicated nature, so he proposed to pass on to more modern authorities, whose experiments were of simpler character, and less open to objection.

The most important experiments which modified those of Deville were due to Bunsen. Bunsen observed the dissociation of steam and carbonic acid by employing small tubes filled with an explosive mixture of these gases, to which suitable pressure-gauges were attached. On igniting the gaseous mixture, explosion took place, and a high momentary pressure was produced within the tube; from the pressure developed, Bunsen calculated the temperature at which the explosion took place, and found that it varied with the mixtures employed. He records the circumstance that only about one-third of the combustible gases took part in the explosion, from which circumstance he concluded that the temperature attained was the limit at which combustion occurred. To prove this, Bunsen allowed the gases sufficient time to cool, after which a second explosion was produced, and even a third explosion when time was allowed for the gases to cool down again. Bunsen obtained much higher temperatures for his limits of dissociation than other physicists; these were for steam about 2400° C., and for carbonic acid about 3000° C. These temperatures were probably higher than

are reached in the arts, as materials used in furnace-building would not withstand such temperatures for any length of time; but still he must call attention to the circumstance that if the influence of the inner surfaces of the tubes on the combustion of the gases therein could be removed, the dissociation temperatures arrived at would be found still higher. He could not admit that Bunsen's explanation of the cause of the second and third explosions was quite satisfactory, as it was not the cooling of the gases alone which rendered the subsequent explosions possible, but also the thorough re-mixture of the gases by diffusion after each explosion. This he illustrated by means of diagrams which represented—

(1) A tube filled with an explosive gas mixture which was shown white.

(2) The same tube immediately after an explosion had taken place, with a white margin to indicate the unexploded mixture close to the sides, and deep red, towards the middle of the tube, the exploded gases. The white was shown as merging into deep red by degrees, because close up to the sides the surfaces prevented explosion or combustion altogether; nearer the middle partial combustion took place, whilst only in the middle of the tube the gases found sufficient space for complete combination.

(3) The same tube after the burnt and unburnt gases had mixed by means of diffusion, which was coloured light red.

(4) The same tube immediately after the second explosion, coloured light red at the sides, turning into deep red by degrees towards the middle.

(5) The same tube after diffusion has done its work a second time, coloured a deeper shade of red.

(6) The same tube after the third explosion, coloured nearly deep red throughout, but still a lighter shade on the sides.

In Bunsen's mode of determining dissociation at high temperatures we had only to deal with the obstruction which surfaces offer to combustion, leaving out their dissociating influence at high temperatures, which affected most of Deville's results. For that reason Bunsen arrived at much higher dissociation temperatures than Deville, and his mode of experimenting possessed the advantage that it might lead to a proper settlement of the question of temperatures at which dissociation would set in when taking place in a space unencumbered by surfaces.

By taking a narrow tube of about the same size as Bunsen used for his experiments, and a hollow sphere of the same capacity, in both of which Bunsen's experiment should be repeated, the real dissociation temperature, if no surfaces were present to influence the result, might be approximately calculated.

Bunsen's method of experimenting, according to his view of the matter, should form the foundation of further research to determine the dissociation temperatures of products of combustion. Even if means were found for eliminating the influence of surfaces, no known material at our disposal could withstand the very high temperature to which the vessels or tubes would be subjected if experiments were carried out according to Deville's method.

That the surfaces of highly heated vessels or tubes either produce, or tend to produce, dissociation, had been corroborated lately by two Russian experimentalists, Menschutkin and Kronowalow. These gentlemen found that dissociation of carbonic acid and other gases was much facilitated when the vessels used for the experiments were filled with material offering rough surfaces, such as asbestos or broken glass.

The lecturer's view of the theory of dissociation caused or influenced by surfaces might be given as follows. Increase of temperature producing expansion of gases would reduce the attractive tendency of the atoms towards one another, or, in other words, diminish their chemical affinity. In the same ratio as the temperature was increased the repelling tendency of the atoms must increase also, until at last decomposition, or what is called dissociation, took place. This being admitted, it would follow that the adhesive or condensing influence of surfaces on the atoms of the gas, which action would increase at high temperatures, would assist this decomposition by increasing the repelling tendency of the atoms.

Victor Meyer, who at first disputed the accuracy of the results obtained by the two Russian physicists, ultimately accepted them, thus confirming the results he had arrived at in practical work with furnaces. Thus the question might be considered nearly settled, the more so as Meyer was himself a great authority in questions of dissociation, having carried out many interesting experiments. Meyer, for instance, proved dissociation by dropping melted platinum into water, and found that oxygen and hydrogen were evolved from the steam produced. There could be no doubt on this point, but the question arose whether heat was the sole agent that brought about the dissociation of steam in this case. In the first place the dissociating influence of the highly heated surfaces of platinum on steam had to be taken into consideration, and, secondly, the chemical affinity which platinum had for oxygen, and still more for hydrogen. The same remarks applied to Meyer's experiment of passing steam or carbonic acid through heated platinum tubes, in which case he obtained only traces of dissociation, the temperature being much lower. Other experiments might be mentioned, but none led to a different conception of the question.

There is one other circumstance connected with dissociation, proved by experiment, which, however, required explanation. It was considered as a sure sign that dissociation was going on when a flame whose temperature was raised became longer; this it was said could only be accounted for by dissociation having commenced. He agreed with this conclusion, but the experiments by which it had been proved had been made, like others referred to, in narrow tubes or passages in which the dissociating action of the heated surfaces must come into play. It was not alone the heat to which the gases were raised that in these cases caused dissociation and increased the length of the flame, but also the influence of the heated surfaces in contact with the combustible gases, more especially if these gases contained hydrocarbons. The extension of the flame was also partly due to the obstruction which the surfaces offered to the recombustion of the dissociated gases through want of space. If the same flame were allowed free development in a space unencumbered by surfaces, as in the lecturer's radiation furnace, no such extension of its length would be observed; but, on the contrary, it would get shorter with increase of temperature. This action could be best observed in the regenerative gas-burner exhibited, whose flame became shorter the greater the intensity of the temperature, and therefore of the light, produced. On the other hand, flame might be extended almost to any length if conducted through narrow passages; this might be seen in regenerative furnaces, which would send the flame to the top of the chimney if the reversing valves were so arranged that the flame, instead of passing through the furnace chamber, was made to burn directly down into the regenerators. No proper combustion could then take place in the brick checkerwork of the regenerative chambers, and the flame would consequently continue to extend until cooled down below a red heat, being ultimately converted into dark smoke; thus in this case the extensive surfaces offered by regenerators would act both ways, by preventing combustion, and by assisting dissociation.

It would be understood that regenerative furnaces themselves offered special opportunities for making experiments, most questions, indeed, being best settled by the results obtained in actual work. If dissociation set in the consequences were seen in want of heat, reduced output, and in destruction of furnace and material. If the causes of dissociation were removed, a rise in temperature, increased output, longer furnace life, and saving of material ensued. Similar results might be obtained with other furnaces, but the beneficial action would not be so great as in the case of the regenerative furnace, because the intensity of heat obtainable in them was much lower.

After describing a new regenerative gas stove he had lately introduced, the lecturer referred to the better distribution of the radiated heat by its use; he found that a room warmed by means of a stove or open fire, such as described, was of a more uniform temperature than when warmed by an ordinary fire or by a gas and coke fire, such as his brother was engaged in introducing into this country shortly before his death.

This, in his opinion, was mainly due to the fact that a source of radiant heat of low intensity but of large surface, sending out its rays at various angles, heated an object in its vicinity very much more than was the case with a smaller source of radiant heat of greater intensity, whose rays struck the object from one direction only, notwithstanding that both sources radiated the same quantity of heat. This action was illustrated by means of two diagrams exhibited, which represented two rooms, the one heated by a small flame of high intensity, and the other by a large flame of low intensity, both radiating the same quantity of heat. In each room two objects, globes or spheres, were repre-

sented, the one close to, and the other at a distance from the source of heat. The object in the one room near to the source having the large heating surface was almost enveloped in rays, while that in the second received rays only in one direction, the former therefore being much more heated than the latter. This difference did not occur when the two globes at a distance from the two sources of heat were compared. The law that the rays of heat diminished in the inverse ratio of the square of the distance was only correct as regards small but intense sources of heat, whilst the decrease of radiant heat took place in a much higher proportion in the case of large sources of heat of low intensity. This clearly proved that for the purpose of warming rooms by means of radiation, it was important that the heat should be concentrated in an intensely hot focus, as was the case in nature, our earth being warmed in this way by the radiant action of the sun.

ON THE EFFECT OF HEAT IN CHANGING THE STRUCTURE OF CRYSTALS OF POTASSIUM CHLORATE

IT was observed some time ago by M. Mallard (*Bulletin de la Société Minéralogique*, 1882, p. 214) that certain crystals, such as boracite and potassium sulphate, have their crystallographic character profoundly modified by exposure to a high temperature, and that in the case of potassium sulphate a number of hemitrope plates are thus formed.

Now, potassium chlorate, while it does not belong to the same crystal-family as potassium sulphate, shows a still more inveterate tendency to produce twins (such as would assuredly drive a Malthus to despair). It was therefore an obvious inference that heat might produce a similar physical change in this substance, although I have not been able to find any account of the experiment having been tried. The decrepitation of crystals of potassium chlorate, when heated, has of course been noted; but the wreck of the crystal has been always rather inadequately explained as due to the vaporisation of included films of water.

A clear transparent crystal of potassium chlorate, from which the inevitable twin plate had been ground away so as to reduce it to a single crystal-film about 1 mm. in thickness, was placed between pieces of mica and laid on a thick iron plate. About 3 cm. from it was laid a small bit of potassium chlorate, and the heat of a Bunsen burner was applied below this latter, so as to obtain an indication when the temperature of the plate was approaching the fusing-point of the substance (359° C., according to Prof. Carnelley). The crystal-plate was carefully watched during the heating, but no decrepitation took place, and no visible alteration was observed, up to the point at which the small sentinel crystal immediately over the burner began to fuse. The lamp was now withdrawn, and when the temperature had sunk a few degrees a remarkable change spread quickly and quietly over the crystal-plate, causing it to reflect light almost as brilliantly as if a film of silver had been deposited on it. No further alteration occurred during the cooling; and the plate, after being ground and polished on both sides, was mounted with Canada balsam between glass plates for examination. Many crystals have been similarly treated with precisely similar results; and the temperature at which the change takes place has been determined to lie between 245° and 248°, by heating the plates upon a bath of melted tin in which a thermometer was immersed. With single crystal-plates no decrepitation has ever been observed, while with the ordinary twinned plates it always occurs more or less violently, each fragment showing the brilliant reflective power above noticed. Doubtless the decrepitation is due to the wrenching asunder of the hemitrope plates, caused by their unequal expansion by heat in different directions.

The following brief account will show the nature of the changes which the crystal has undergone:—

(1) Examined in common white light, the ordinary crystals of potassium chlorate reflect no more light, either superficially or internally, than a plate of glass, in whatever position they are viewed.

The altered crystals, when similarly examined, reflect little light at small angles of incidence, but at all angles greater than about 10° they reflect light with a brilliancy which shows that the reflection must be almost total. This reflective power does not seem to be materially greater at high angles of incidence. When the plate is turned round in its own plane, two positions are found, differing in azimuth by 180°, in which the crystal reflects no more light than an ordinary crystal under the same conditions. In these cases the plane of incidence coincides with the plane of crystallographic symmetry.

The reflected beam is slightly iridescent; and when the plate is held obliquely and examined with a magnifier, a striated faintly-coloured structure is observable, resembling that of watered silk or mother-of-pearl. The coloured bands always lie parallel to the plane of symmetry. When the reflected light is examined with a spectroscope, it is found to give a rather complicated spectrum containing numerous narrow absorption-bands. In some specimens these bands are fairly straight and regular, but in most cases they are rather wavy, and vary in thickness in different parts of their length, appearing somewhat like the interlacing twigs in a bundle of sticks. As the angle of incidence is increased, these bands move towards the more refrangible end of the spectrum, while others appear and join in the procession.

The spectrum of the transmitted light is, of course, strictly complementary to that of the reflected beam; and both of them strongly resemble the spectra given by some of the iridescent crystals described by Prof. Stokes (see NATURE, vol. xxxi. p. 565), and also by many sections of opal and mother-of-pearl, and by films of decomposed glass.

(2) When examined in a parallel beam of plane-polarised light, the ordinary crystals show little or no colour, unless held so that the light passes nearly in the direction of the optic axes, when the usual broad, rather faintly-coloured bands are seen. The altered crystals, on the contrary, give in all positions (except when the light passes through nearly normally, or when the plane of polarisation is either parallel or perpendicular to the plane of symmetry) a most complicated and brightly-coloured pattern, resembling that which is shown by many of the complicated macled crystals of amethystine quartz, which vary, like patterns on watered silk, with slight changes in the direction of incidence of the light.

(3) When examined in a micro-polariscope, in plane-polarised, highly convergent white light, the ordinary crystals show the usual isochromatic lemniscates surrounding the optic axes, which latter are themselves just visible at the edge of the field. In the altered crystals nothing of the kind is visible, only patches of colour distributed rather irregularly over the field, somewhat like those of certain of Nörremberg's mica-selenite combinations.

(4) When homogeneous (sodium) light was substituted for white light in the micro-polariscope (an expedient which is of great use in simplifying and giving definiteness to the phenomena shown by crystals), the remarkable nature of the structural change which heat had caused was much more clearly apparent. The ordinary crystals simply showed the usual multitude of curved isochromatic bands symmetrically arranged round the optic axes and filling the whole field. The altered crystals showed nothing of the kind; but a set of hyperbolas appeared—the form of the isochromatic curves of extremely high order which are given by biaxial crystals when the directions of the optic axes make a very large angle with the normal to the surface of the plate (see Verdet, *Œuvres*, vol. vi. pp. 172-175). These hyperbolas are not rectangular, thus proving that the optic axes do not lie in the plane of the plate (as in the case of cleavage plates of selenite); but they so nearly do this that I could not, even by immersing the plate in oil, satisfactorily determine their precise position. The bands are rather irregular and shifty, as is usual in composite macles; in some parts of a crystal they may appear as the central portions of a lemniscate-system.

(5) It seemed desirable to examine the effect of heat upon the crystal during its progress, so as to determine whether the change of structure takes place at the period of the formation of the reflective layer. For this purpose a polished plate of potassium chlorate was clasped in a copper holder (like that used for plates of selenite in Mitscherlich's well-known experiment), so that it could be placed in the field of the polariscope and examined while its temperature was gradually raised by the application of a lamp-flame to the outer extremity of the holder.

The ordinary set of isochromatic curves lasted nearly unchanged for some time as the temperature rose, but at a certain point they faded away like a dissolving view; and then out of the confusion there emerged the set of hyperbolas above mentioned, which grew in definiteness and regularity, but did not

otherwise alter until the field quickly became dark owing to the fusion of the crystal. This seems to indicate that the change in structure begins quite independently of the formation of the reflective layers, the latter being only an incident occurring at a particular stage of the cooling.

(6) It would seem that something of the following kind happens to the crystal. It is, of course, anisotropic in structure, and the effect of heat is to set up a molecular strain which at a certain point of temperature causes so strong a shearing action between nearly contiguous layers of the substance that whole rows of crystal elements lying between these layers are rolled over, as it were, by the "couple" applied to them, until they take up their "second positions of equilibrium," as M. Mallard would say (see his paper "Sur la Théorie des Macles," *Bull. Soc Min.*, December 1885, p. 467). If these latter positions were such as to bring the *obtuse* bisectrices (supplementary lines) into parallelism with a normal to the main plate, the occurrence of the hyperbolas above described would be fully accounted for. Such an action would be of the same general character as that which takes place in calc-spar when macles are being developed in it by Reusch's method; viz. by carefully compressing a crystal of it in a definite direction (*Pogg. Ann.*, vol. cxxxii. p. 445), I have succeeded by properly regulating the direction and amount of the pressure in making spar-macles containing numerous "planes of sliding" (*Gleitflächen*, as Prof. Reusch calls them), which reflect light with a pearly lustre, and almost as brightly as the potassium chlorate macles described above.

It has yet to be explained, however, why the intense reflective power does not show itself during the process of heating, when the tilting over of the crystals would certainly take place, and not until a particular stage of the cooling is reached. I am inclined to believe that this may be due to the substance acquiring a certain amount of plasticity at high temperatures, such as has been observed by M. Mallard in crystals of nitre under similar circumstances. This may prevent any loss of optical continuity until a certain critical point in the cooling has been reached; and at this point the displaced crystal elements suddenly part company with their unaltered neighbours, leaving a numerous series of parallel tubular cavities, precisely like those which are undoubtedly present in calc-spar macles formed by Reusch's method. The opposite sides of these parallelogrammatic cavities may be so near each other that the rays reflected from them may interfere, and give the colours of thin plates corresponding to a rather high order in Newton's scale. Although a large amount of light must escape reflection at any single cavity, yet if the transmitted rays encountered a large number of precisely similar and similarly situated cavities at slightly lower levels in the crystal, the sum of the partial reflections would produce an effect almost equivalent to a total reflection of the original incident ray, and a corresponding deficiency in the amount of light transmitted through the whole plate. The brilliancy of the colours in the light reflected from the well-known films of decomposed glass is accounted for in precisely the same way, and the successive separate films of glass can be easily seen under a microscope at the edges of the compound film, where they only partially overlap.

The fact that no brilliant reflection is observed in and near the plane of symmetry of the crystal may be due to the sides of the cavities in a given horizontal row not lying strictly in the same plane, but being slightly inclined alternately in opposite directions, so as to form a series of anticlinals and synclinals, or ridges and furrows like those of a roof. Thus a beam of light incident in the plane of symmetry would be reflected in directions lying a little to the right and left of this plane, and not in the plane itself. The satin-like appearance of the reflecting layers, already alluded to, would be fully accounted for by such a structure.

The changes above described seem of interest as bearing upon the cause of the strong iridescence of some crystals of potassium chlorate, about which I may have something to say in a future communication.

H. G. MADAN

Eton College, May 10

SCIENCE IN RUSSIA

THE last volume of the *Memoirs* of the Kharkoff Society of Naturalists (vol. xviii.) contains several papers of interest. All who have had to deal with Acarides, and are acquainted with the difficulties of their classification, will welcome the elaborate memoir, by M. Krendowsky, on the Hydrachnids of Southern Russia. It is not a mere description of forms, with a more or less happy classification, but an elaborate contribution towards the systematic arrangement of this imperfectly-known subdivision. The embryogeny of the Hydrachnids, and especially their larval phase, have received special attention, no satisfactory classification being possible without that preliminary study. It appears also from M. Krendowsky's researches that many Hydrachnids of Southern Russia are really temporary parasites on several insects, mollusks, and sponges, especially when young and in the state of six-footed larvæ. The Hydrachnids of South-Western Russia belong to thirty-five species (nine species each of *Nesæa* and *Arrenurus*, five of *Atax*, and four of *Limnesia*); the author has been led to revise the whole of the classification of the freshwater Acarides, and gives it complete, with analyses of each family, as well as of the very numerous genera.

Another paper of great interest is devoted by the same author to the estuaries of the Bug, Dnieper, and the smaller ones in the neighbourhoods of Kherson and Odessa. This paper is full of the most useful information as to the characters and geological history both of these estuaries and the *limans*, which are now shut off from the sea by their sand-bars, and have become mere elongated salt lakes.

Prof. Lewakowsky contributes to the same volume a paper on the Jurassic limestones of the Crimea, based especially on their micro-structure. It appears that they mostly contain very small debris of corals and rhizopods; they are not coral structures, as was supposed, but have much likeness to what Dana describes as beachsand-rock. Like the clay-slates of the same formation in the Crimea, they have been deposited in a wide basin which extended into Kherson and Ekaterinoslav, and they were composed of materials brought from the south, from a continent which occupied part of what is now submerged by the Black Sea. M. Genjouriste's researches into the microscopical structure of the coal of the Donez Basin are interesting inasmuch as they show that the prevailing materials for the formation of this coal were the higher vascular Cryptogams, and not Algæ, as was sometimes supposed by Russian geologists. Dr. M. Dybowsky's additional note on the Spongilla *Dorvilia stepanovii*, one of the most interesting discovered in Europe, contains a description of the structure of its gemmulæ, with the porous and "cirrous appendages." The note, as also the preceding papers, are accompanied by several plates.

SCIENTIFIC SERIALS

The Quarterly Journal of Microscopical Science, vol. xxvi. part 3, April 1886, contains a memoir on the leeches of Japan, by Dr. C. O. Whitman (plates 17 to 21). A short abstract of this important memoir has been given in our Biological Notes.—Contributions to the embryology of the Nemertea, by Prof. A. A. W. Hubrecht (plate 22). No. 1 is an account of the development of *Lineus obscurus*, Barr. These investigations, already published in the Dutch language, are fully detailed in this paper, and the plate gives the details of the principal results, combined into fifteen diagrammatic tracings. In one section the earliest developmental stages and the derivatives of the primary epiblast; in a second the hypoblast before the shedding of the primary larval integuments; and in a third the mesoblast, are treated of.—On the early development of *Julus terrestris*, by F. G. Heathcote, M.A. (plates 23 and 24). This is the first part of an essay on a subject not treated of by British naturalists since the days of Newport. It treats of the segmentation of the ovum, which shows a remarkable resemblance to that found in Amphipods by Uljanin. The formation of the blastoderm is such as is generally found in tracheate development. The cells, which at the conclusion of the blastoderm formation remain within the yolk, represent the endoderm. The mode of formation of the mesoderm almost exactly resembles that described by Balfour for spiders. In a future paper the author intends describing the further developmental stages of the embryo.—William A. Haswell, M.A., on the structure of the so called glandular ventricle (*Drüsenmagen*) of Syllis (plate 25). This organ is in reality a well-developed muscular gizzard, and contains no glands in its walls. The muscular elements of the organ present an embryonic character containing as they do a polynucleated core.—Arthur B. Lee, on Carnoy's cell researches (plate 26). While Carnoy's conceptions of the cell body do not materially differ from received views, the author of this paper thinks that sufficient attention has not been given to his labours on the

question.—Prof. E. Ray Lankester, the Pleomorphism of the Schizophyta. A reminder of the simple fact that ten years ago Prof. Lankester called attention to the pleomorphism of the Schizophyta in a paper in this *Journal*, which attracted the deep attention of all those botanists who had taken any interest in the subject.

Journal of Anatomy and Physiology, April 1886, vol. xx., part 3, contains :—Dr. J. W. Frazer, on the action of infused beverages on peptic digestion. This paper is a continuation of one in the eighteenth volume of this *Journal*, and is based on the results of the same experiments, the difference being that the amount of peptones dialysed, instead of being estimated as the total organic matter, as was done in that paper, are here estimated by the amount of organic nitrogen.—W. A. Lane, some variations in the human skeleton ; asymmetry of skull, spinal column, &c., bifid ribs.—Dr. R. L. MacDonnell, case of bicipital rib.—Dr. R. W. Shufeldt, osteology of *Conurus carolinensis* (plates 10 and 11). The extermination of this parrot appears imminent. To this memoir there is appended a synopsis of the skeletal characters of this bird which exhibit many points of interest.—On a Navajo skull (plate 12), with a note by Sir Wm. Turner.—J. Bland Sutton, on the origin of certain cysts (plate 13).—Dr. J. Lockhart Gibson, the blood-forming organs and blood-formation : an experimental research (plate 14) (continued). Among the chief conclusions are the following: throughout life, nucleated red cells, derived from white corpuscles and colourless marrow-cells, are the only predecessors of the non-nucleated red blood-corpuscles. The transformation takes place in the bone marrow, spleen, and lymphatic glands ; the red bone marrow in extra-uterine life plays the more important part in the work, the spleen a subordinate one ; the lymphatics, while chiefly producing white, do also produce red corpuscles. Both colourless cells and nucleated red cells multiply by division in the blood-forming organs, and in these latter there are also to be found cells whose function appears to be to break down red blood-corpuscles.—Dr. E. E. Maddox, on the relation between convergence and accommodation of the eyes.—Dr. R. Robertson, a contribution to splenic pathology (plate 15).—Dr. F. Tuckerman, supernumerary leg in a male frog (*Rana palustris*) (plate 16).—Dr. D. Noël-Paton, the nature of the relationship between urea formation and bile secretion. Both these phenomena would seem to depend in large measure on the destruction of blood-corpuscles, and through this they necessarily bear a direct relationship to one another.—Prof. D'Arcy W. Thompson, on the hind limb in *Ichthyosaurus platyodon*, and on the morphology of vertebrate limbs.—Sir Wm. Turner, on the lumbar curve of the spinal column in several races of men (see also abstract of a memoir on this subject by Prof. D. J. Cuningham, NATURE, vol. xxxiii. p. 378).—Anatomical notes.

American Journal of Science, April.—On Lower Silurian fossils from a limestone of the original Taconic of Prof. Emmons, by James D. Dana. These fossils were recently found in the "sparry" or western limestone of the Taconic system, that is, the oldest limestone stratum of the system according to Emmons. They come from Canaan, New York, near the Massachusetts border, and several species have been determined by Prof. Dwight, notwithstanding the metamorphism of the rock. They include remains of Murchisonias, Pleurotomarias, Crinoids, Fenestellæ, a Trilobite, and probably some Brachiopods, showing that this limestone is not pre-Cambrian or Cambrian, but belongs probably to the Trenton or Lower Silurian age of the Eastern or Stockbridge limestone.—Preliminary report of S. W. Ford and W. B. Dwight upon the fossils obtained in 1885 from metamorphic limestones of the Taconic series of Prof. Emmons at Canaan, New York : A. Explanatory statement with reference to the palæontological investigations at Canaan, by W. B. Dwight. The authors are strongly inclined to the opinion that the limestones of Canaan, which have yielded these fossils, are of Trenton age.—On surface transmission of electrical discharges, by H. S. Carhart. A practical bearing of the experiments here described and illustrated is that there is no sufficient scientific basis for making lightning conductors of large surface, and that large sectional area is essential to ample conductivity.—The minerals of Litchfield, Maine, by F. W. Clarke. The paper contains a careful study and analysis of the elæolite, cancrinite, sodalite, hydronephelite (new species), albite, and lepidomelane from the numerous boulders of an elæolite rock scattered over the district between Litchfield and West Gardiner, in Kennebec County, Maine.—On the chemical behaviour of iron in the magnetic field, by Edward L. Nichols. A set of experiments with aqua-regia, nitric acid, hydrochloric acid, and sulphuric acid is described in illustration of the phenomenon that, when finely-divided iron is placed in a magnetic field of considerable intensity and exposed to the action of an acid, the chemical reaction differs in many respects from that which occurs under ordinary circumstances. The experiments are preliminary to a more complete investigation of the novel series of effects developed by them.—The inculcation of scientific method by example, with an illustration drawn from the Quaternary geology of Utah, by G. K. Gilbert. This paper is a reprint of the Presidential Address read before the American Society of Naturalists at Boston, December 27, 1885. It discusses, not the results nor the subject-matter of the several sciences with which naturalists are concerned, but their methods of investigation and their methods of teaching generally.—Nova Andromedæ, by Asaph Hall. The history of the discovery of the new star in Andromeda by Dr. Hartwig, of Dorpat, last August, its observation and gradual fading away, forms the subject of this paper.—On some new forms of the Dinocerata, by W. B. Scott. What appears to be a missing link between the two sub-orders of Amblypoda (the Coryphodons of the Wahsatch Eocene and the Dinocerata of the Bridger) is here described under the name of Elachoceras. It was discovered by the Princeton Expedition of 1885 in the Bridger beds of Henry's Fork, Wyoming, and represents a genus allied to Uintatherium, without upper incisors, and having six molars of the Uintatherium type and large upper canine tusks, but without nasal protuberances, and having only rudiments of the maxillary and parietal protuberances. The supra-occipital is pierced by two large venous foramina placed one on each side of the median line. In the same locality, but at a somewhat higher level, was found a large Uintatherium skull, undoubtedly representing a new species (*U. alticeps*) of that genus.

The American Naturalist for April 1886 contains :—On the ancestry of Nasua, by Saml. Lockwood.—On the mechanism of soaring (illustrated), by J. Lancaster.—The Stone Age in Vermont (illustrated), by Geo. H. Perkins.—On Grosse's classification and structure of the Mallophaga (illustrated), by Geo. Macloskie.—On traces of a cyclone which passed over Western Indiana more than 300 years ago, by Jno. T. Campbell.—On the mounting of fossils (illustrated), by F. C. Hill.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, April 1.—"On a New Form of Stereoscope." By A. Stroh.

Two optical lanterns are placed side by side, as for dissolving views. Two transparencies, photographed in the same manner as if intended for an ordinary stereoscope, are placed one in each lantern, and projected on a screen in such a position that they overlap each other as nearly as possible. The picture which is intended to be seen by the right eye may be placed in the right-hand lantern, and the other in the left.

Supported by suitable framework, and in front of the two lenses of the lanterns, is a revolving disk, portions of which are cut away, so that during its revolutions it obscures the light of each lantern alternately, or, in other words, so that only one picture at a time is thrown on the screen. A continuous change from one picture to the other is thus obtained.

In the same framework, and in convenient positions for the observers, two pairs of eye-holes are provided, one pair on either side of the apparatus. Behind each pair is also a rotating disk, and these disks are connected by suitable wheel-work or driving-bands with the one previously mentioned, in such a way that the three disks rotate together, and at the same rate. The two last-named disks are also so cut that they will obstruct the view through the right and left eye-holes alternately.

Finally, the connection between the three disks has to be so arranged that the time of obscuring the view through the right eye-holes, or the left eye-holes, shall coincide with the time when the light is shut off from the right or left lens of the lanterns respectively.

It is obvious that by this arrangement an observer can only see the picture projected from the left lantern with the left eye, and the one from the right-hand lantern with the right eye.

The rotation of the disks must be of such a rate that the alternate flashes of the right and left pictures on the corresponding

eyes follow in such rapid succession that the impression made by one flash does not diminish sensibly before the next flash on the same eye is received. The number of flashes for each eye which is required to produce an apparently continuous view, without any flickering effect, is from thirty to forty per second. As the disks are so cut as to produce two flashes for the right eyes and two for the left in one revolution, they must consequently be kept rotating at a rate of from fifteen to twenty revolutions per second.

The rotation of the disks is effected by a driving-wheel and band worked by a crank handle at the back of the apparatus.

The perspective effect obtained by the above arrangement is very perfect, the image of each object standing out in solid relief.

Considering that by this arrangement the two eyes never see at the same time, and that each eye views its picture after the other, it is interesting to find that the persistence of vision so completely bridges over the alternate interruptions to which it is subjected as to produce the effect of a continuous view.

The advantages claimed for this form of stereoscope are: that the pictures can be enlarged to such an extent as to appear equal to or even larger than the original objects from which they were taken; and that the eyes in looking at the pictures are not in any way subjected to strain by lenses, prisms, or reflectors, or by the difficulty which some persons experience in getting the two pictures to superpose. For each eye views its corresponding picture in exactly the same position it would see it in if it were looking at the original, since the two pictures are practically in the same place, which is not the case in any other form of stereoscope.

Although with the apparatus as here described only two persons can see the pictures at the same time, it would not be very difficult to construct it so as to be available for a greater number. The side disks above described only serve to control one pair of eye-holes each, but by making them larger they would serve for two pairs each, thus accommodating four observers. By increasing the number of disks, the number of observers might be increased proportionately.

May 6.—"The Influence of Stress and Strain on the Physical Properties of Matter. Part I. Elasticity (continued). The Effect of Change of Temperature on the Internal Friction and Torsional Elasticity of Metals." By Herbert Tomlinson, B.A. Communicated by Prof. W. Grylls Adams, M.A., F.R.S.

The author has recently had the honour of presenting to the Society a memoir relating to the internal friction of metals when vibrating torsionally at temperatures ranging from 0° C. to 5° C. He now brings forward results which have been obtained in experiments on the effect of change of temperature on the torsional elasticity and internal friction of metals. The apparatus used and the mode of experimenting are fully described in the paper, so that it will be sufficient, perhaps, to state here that the vibration-period and the logarithmic decrement were very carefully determined at four different temperatures between 0° C. and 100° C., and that the formulæ were worked out by the method of least squares. These formulæ were given in tables.

From a consideration of the tables it may be gathered that:—

(*d*) The torsional elasticity of all metals is temporarily decreased by rise of temperature between the limits of 0° C. and 100° C., the amount of decrease per degree rise of temperature increasing with the temperature. To this may be added that the percentage decrease of torsional elasticity produced by a given rise of temperature is for most metals about twenty times the corresponding percentage increase of length.

(*e*) If we start with a sufficiently low temperature the internal friction of all annealed metals is first temporarily decreased by rise of temperature and afterwards increased. The temperature of minimum internal friction is for most annealed metals between 0° C. and 100° C.; for most hard drawn wire, however, the temperature of minimum internal friction is below 0° C.

(*f*) The temporary change, whether of the nature of increase or decrease, wrought by alteration of temperature in the internal friction of metals, is in most cases enormously greater than the corresponding change in the torsional elasticity.

Linnean Society, May 6.—Sir John Lubbock, Bart., President, in the chair.—Prof. H. Marshall Ward was elected a Fellow of the Society.—Mr. D. Morris exhibited a number of living beetles (*Pyrophorus noctilucus*) from the island of Dominica. These had been fed on sugar-cane during the voyage to England. On the meeting-room being darkened, the phosphorescent show of light emitted by the insects was very brilliant.—Dr. Chas. Cogswell drew attention to framed water-colour drawings of *Lettsomia aggregata* and *Fothergilla gardeni*, botanical mementos of the two distinguished physicians Lettsom and Fothergill.—Sir J. Lubbock's paper on forms of seedlings was, by request, adjourned, so as to give opportunity for discussion of Mr. Romanes's communication.—Mr. G. J. Romanes then read his paper on physiological selection: an additional suggestion on the origin of species. A full account of this paper will appear in a future number.—Thereafter the two following papers were read in abstract:—Descriptions of new species of Galerucidæ, by Joseph S. Baly.—On some new species of the genus *Metzgeria*, by Wm. Mitten.

Geological Society, April 21.—Prof. J. W. Judd, F.R.S., President, in the chair.—Henry Fisher, Frederick Edwin Harman, Henry Johnson, Edward Alloway Pankhurst, and Henry Woolcock were elected Fellows of the Society.—The following communications were read:—On a certain fossiliferous pebble-band in the "Olive group" of the eastern Salt Range, Punjab, by A. B. Wynne, F.G.S. The principal object of this paper was to oppose the views recently published by Dr. Waagen as to the age of certain Boulder-beds in the Salt Range of the Punjab. By that author these beds had been considered contemporaneous with each other, and assigned to the epoch of the Coal-measures, in consequence of the discovery by Dr H. Warth of Carboniferous fossils, especially Australian forms of Conularia, in nodules restricted to a particular layer in the upper part of a Boulder-bed in the eastern Salt Range. Mr. Wynne adduced evidence to show that the fossils in question occur, not in concretions, as supposed by Dr. Waagen, but in pebbles evidently derived from an older series; and consequently there was no proof that the Boulder-bed in question was older than the Cretaceous Olive-beds with which it had hitherto been associated. The principal Boulder-beds in the Salt Range were then briefly noticed; those beneath the Carboniferous Limestone west of the Indus, those near Amb and Sakesir peak, associated with the "purple sandstone," "Obolus-beds," and "speckled sandstone," and those in the eastern portion of the Salt Range, amongst the beds of the "Salt pseudomorph zone" and "Olive group" being successively passed in review, and their relations to overlying and underlying strata explained. It was shown that Boulder-beds and conglomerates containing pebbles and boulders of the same crystalline rocks are not confined to one horizon. In conclusion, the resemblance of the rock, of which the pebbles containing Conulariæ, &c., were formed, to that forming some of the "magnesian sandstone" and "Obolus-beds" was pointed out, and it was suggested that the pebbles in question may have been derived from representatives of those beds formerly existing to the southward.—On the phosphatic beds in the neighbourhood of Mons, by M. F. L. Cornet, For.Corr.G.S. These beds are situate in the province of Hainaut, near the town of Mons (Belgium); the workings have increased of late years, and in 1884 yielded 85,000 tons of phosphate. They occur in the Upper Cretaceous, which is exceptionally well developed in the district, filling a trough in the Carboniferous rocks, and itself denuded for the reception of Tertiary and Quaternary beds. Omitting all Cretaceous groups below the middle of the fifth stage, the following is the sequence of the Cretaceous beds which contain the phosphatic series:—C. Tufaceous chalk of Ciply, with the Poudingue de la Malogne at its base. D. *Brown phosphatic chalk of Ciply*. E. Coarse chalk of Spiennes. F. White chalk of Nouvelles. F is a pure white chalk with some flints, and contains *Belemnitella mucronata*, *Rhynchonella octoplicata*, *Terebratula carnea*, *Ananchytes ovatus*, &c.,—an horizon well known throughout North-Western Europe. Series E and D represent one geological horizon characterised by Ostreæ, Brachiopoda, &c., in great numbers, but also containing *Belemnitella mucronata*, and lying between two distinct planes of erosion. The brown phosphatic chalk (D), which forms the upper division of the series, is about 70 feet thick, and may be described as consisting of three parts; the upper is tolerably pure carbonate of lime, but in its lower portion becomes charged with brown granules mainly consisting of phosphate of lime; these continue to increase towards the central or main phosphatic mass, which is also highly fossiliferous in places. This central portion constitutes the main phosphatic beds, but the amount of phosphoric acid (dry) is not more than 12 per cent. Hence, it is necessary to increase the richness in phosphate of the deposit in order that it may be available for

conversion into a superphosphate. This may be done by mechanical means. But nature has already partially anticipated this process, and the result has been a deposit known as "rich phosphate," containing about 25 per cent. of phosphoric acid. This occurs in wide cracks and holes in the ordinary phosphatic chalk. It usually occurs as a fine sand-like powder, and is evidently the result of the action of carbonated waters upon the phosphatic chalk, whereby the amount of carbonate of lime is reduced. This is especially the case where the phosphatic chalk is not protected by the tufaceous chalk of Ciply, but is only covered by Tertiary or Quaternary beds. The author calculates that each square foot of the phosphatic basin, which he estimates approximately at 5 miles by 3, contains 355 lbs. of tribasic phosphate of lime. Finally, he intimates how the phosphatisation of the chalk may have been brought about.

Physical Society, May 8.—Prof. H. McLeod, F.R.S., Vice-President, in the chair.—Mr. W. A. Price was elected a member of the Society.—The following communications were read:—On a modified form of Wheatstone's rheostat, by Mr. Shelford Bidwell. A wire is coiled upon a non-conducting cylinder as in the ordinary forms of rheostat, one end of the wire being in contact with the brass axle of the cylinder. A screw is cut upon the axle, the pitch being equal to the distance between the consecutive turns of the wire, and this, working in a fixed nut, causes the whole cylinder to travel in the direction of its axis. A fixed spring bears upon the wire at a convenient point, and by the travelling motion of the cylinder this point of contact remains fixed in space, and the effect of turning the cylinder is to introduce more or less resistance between the spring and the brass axle. Binding screws on the base of the instrument are in contact with the nut and the bearing spring. Though this arrangement has several obvious advantages over the usual forms, Mr. Bidwell does not recommend it in cases where it is required to introduce a known resistance, but where it is important to adjust a resistance to a nicety, or to cause a continuous variation, it is of great use.—Prof. Perry, remarking upon the importance of being able to vary a resistance gradually, described an instrument he had used with advantage. A number of plates of gas-carbon are placed between two parallel copper plates, one of which is fixed and the other adjustable by a screw; by applying pressure by means of the screw the resistance between the plates can be varied uniformly and regularly from 2 to 10 ohms, beyond which point the increase is very rapid.—On a theorem relating to curved diffraction-gratings, by Mr. Walter Baily. In a paper read before the Society in January 1883 the author showed that if a plane be taken perpendicular to the lines of a curved diffraction-grating, and a normal to the grating be taken as the initial line, the equation—

$$\frac{\cos^2\theta}{r} = \frac{\cos\theta}{c} + \frac{1}{d}$$

(in which c is the radius of curvature of the grating, and d is an arbitrary constant), gives a curve having the property that if a point of light be placed anywhere upon it the curve is the locus of the foci of all diffracted rays whether reflected or transmitted. In the present investigation d is supposed to be greater than c, which allows of the source of light being at infinity. The points where the curve given by the above equation cuts the normal are called the normal foci. There are two of these, one relating to the reflected and the other to the transmitted light, the grating being supposed to consist of a number of opaque lines in space. It is then shown that if the grating be supposed to turn about the line in it intersecting the initial line, the normal foci will trace out two parabolas whose common focus is the origin, and common latus rectum is equal to the diameter of curvature of the grating, the parabola for reflected light being convex to the source of light, and that for transmitted light concave.—On some thermodynamical relations, part iv., by Prof. W. Ramsay and Dr. Sydney Young. The first part of this communication deals with Profs. Ayrton and Perry's criticisms upon the previous papers by the authors upon this subject. In the second part a brief review is given of the various attempts that have been made to represent the pressure of a saturated vapour as a function of the temperature.

Anthropological Institute, May 11.—Mr. Francis Galton, F.R.S., President, in the chair.—Mr. Galton read some notes on permanent colour-types in mosaic, in which he advocated the adoption of certain specimens of mosaic material as permanent specimens of standard colours for the description of tints of skin. The original paintings by Broca, as well as the lithographs from them, have already changed colour, and some more permanent standard is greatly needed. There can be no question as to the persistence of the colours of mosaic: some specimens in St. Peter's at Rome, that are more than a century old, have the appearance of being brand-new. The material is inexpensive, and as the variety of tints in the Vatican manufactory is very large, the flesh tints appropriate to European nations alone being about 500 in number, there would be no difficulty in selecting such a series as anthropologists desire.—Prof. Flower exhibited a Nicobarese skull, sent over by Mr. E. H. Man, together with some photographs of the natives.—Prof. Thane read a paper by Prof. A. Macalister on some African skulls and on a New Ireland skull in the Anatomical Museum of the University of Cambridge.—Dr. Garson reported that the correspondence as to an international agreement on the cephalic index had been brought to a satisfactory conclusion, and that the scheme advocated by him in his paper read before the Institute in February last had been accepted by sixty of the leading anthropologists on the Continent.—Dr. Garson read a paper on the skeleton and cephalic index of Japanese.

Entomological Society, May 5.—Prof. J. O. Westwood, M.A., in the chair.—The following were elected Fellows:—The Rev. E. N. Bloomfield, M.A., Mr. F. Fitch, Mr. A. J. Rose, and Mr. W. E. Nicholson.—Mr. J. Jenner-Weir exhibited a large spiny Lepidopterous larva from Western Africa.—Mr. Stevens exhibited *Apion sorbi* and other Coleoptera recently obtained in the Isle of Wight.—Mr. Crowley exhibited four specimens of *Leto venus*, a large moth belonging to the family *Hepialidæ*, from Natal.—Mr. Howard Vaughan exhibited a long series of *Cidaria immanata* from Kent, Surrey, Perthshire, Isle of Man, Isle of Arran, the Orkneys and Shetlands. He also exhibited *C. russata* from various localities in the south of England, and from Perthshire, Argyllshire, and the Islands of Arran, Lewis, and Hoy. Mr. Vaughan further exhibited varieties of *C. suffumata* from Dover and Darlington. Prof. Westwood commented on the interesting nature of the exhibition of *C. immanata*, and stated that he had never before seen such a wonderful collection of varieties of a single species.—The Rev. W. W. Fowler exhibited *Staphylinus latebricola* and *Quedius truncicola*, both from the New Forest.—The Secretary exhibited, for M. H. de la Cuisine, of Dijon, coloured drawings, life-size, of a variety of *Urania crœsus* and a variety of *Papilio memnon*.—Mr. G. Elisha exhibited specimens of *Antispila pfeifferella*, together with the cases, and the leaves mined by the larvæ.—Mr. J. W. Slater read a paper "On the Origin of Colours in Insects," in which he showed that the assertions of Mr. Grant Allen, that all brightly-coloured insects were flower-haunting species were incorrect, and that many brilliantly-coloured insects were carnivorous. Mr. McLachlan said that the physiological question in connection with colour had not been paid attention to; he thought that colour in insects was, to a great extent, dependent upon the circulation of fluids in their wings. The discussion was continued by Prof. Westwood, Mr. H. Goss, the Rev. W. W. Fowler, Mr. Jacoby, and Mr. Weir.

Victoria (Philosophical) Institute, May 3.—A paper by M. Maspero, describing his discovery of many Syrian geographical names in the lists of Thothmes III., was read. It was illustrated by a map, specially prepared by the author, as an aid to the inquirer in following his description of the Egyptian account of the events connected with each name. M. Maspero concluded his statements in the following words:—"Such are the observations which a long study of the lists has suggested to me. I have elsewhere given the justification of my transcriptions. I have endeavoured to bring to my identifications the same prudence that I have exercised in my transcriptions. The The names enumerated arrange themselves almost wholly in the districts that surround Megiddo; Qodshu, Damascus, and two or three other towns at most belong to countries comparatively remote. This result, to which the independent study of the lists has led me, arises clearly from the history of the campaign as the inscription at Karnak makes it known to us. In the year 23 (of his reign) Thothmes III. set out from Gaza, cleared Carmel, beat the confederates, including the prince of Qodshu, under the walls of Megiddo, besieged and took the town, then returned to Egypt without pushing farther on towards the north. The fall of Megiddo was decisive, for, as Thothmes III. has himself observed, 'every chief of the whole country [was shut up] in it, so that the capture of Megiddo was as good as the

taking of a thousand towns.' When the war was finished he 'reinstalled the chiefs in their dignity' on condition that they should pay tribute. The stress of the campaign fell thus on the plain of Esdraëlon: the Egyptian troops had long remained there, and had pillaged all the district round, not without pushing on to some distant points. On his return, when Thothmes III. built the pylon of Karnak with the booty of this campaign, he inscribed on the wall the names of the towns that he had sacked and which had unwillingly contributed to the completion of the edifice. The wall was large, and must be entirely covered. They took indiscriminately all the names of Galilee and Southern Syria that they knew, without troubling about the importance of the town itself: one name did as well as another for that matter." Sir C. Wilson, K.C.M.G., Mr. Boscawen, and others took part in the discussion.

Edinburgh

Royal Society, April 19.—Sir W. Thomson, Hon. Vice-President, in the chair.—Sir W. Thomson exhibited and described a new form of portable spring balance for the measurement of terrestrial gravity. In this instrument a metallic spring is used. The curvature of the spring when unweighted is such that, when one end is firmly clamped and a suitable weight attached to the other end, the spring becomes straight. When so arranged, the equilibrium of the spring and weight can be made as nearly unstable as is wished by simply tilting the instrument. Hence the apparatus can be made as delicate as necessary.—Mr. A. P. Laurie read a paper on the measurements of the E.M.F. of a constant voltaic cell with moving plates. Mr. Laurie determined the E.M.F. of a cadmium-iodine cell by drawing a large current from it, while the plates were kept moving. The value so got agreed with that given on open circuit as determined by the electrometer, thus showing that the fall of the current when the plates were not moving was due to alteration of the composition of the layers of liquid next the plate.—Mr. W. E. Hoyle read a note on the formation of Hectocotylus in Rossia.—Prof. Tait submitted a paper on some definite integrals.—Messrs. H. Rainy and R. D. Clarkson described the alterations in the electric conducting power of alloys at their melting-point.—The Rev. T. P. Kirkman submitted a communication on the reading of the circle, or circles, of a knot.

May 3.—Robert Gray, Vice-President, in the chair.—Dr. R. W. Felkin read notes on the Waganda, a Central African tribe.—J. Murray discussed the drainage-areas of continents, and their relation to oceanic deposits.—Dr. A. B. Griffiths read a paper on the vitality of the spores of parasitic fungi, and the antiseptic properties of ferrous sulphate.—Dr. R. Stockman discussed the action of benzoyl-ecgonin.

Paris

Academy of Sciences, May 10.—M. Jurien de la Gravière, President, in the chair.—On the formation of oxalic acid in plants (continued): *Amaranthus caudatus*, *Chenopodium Quinoa*, *Mesembryanthemum crystallinum*, by MM. Berthelot and André. Tables are given of the varying quantity of oxalic acid in the roots, stems, leaves, and flowers of these plants at different seasons of the year. The results throw great light on the essentially different physiological conditions of life and organisation in these various types of vegetation.—Observations of the comet 1886 *b* (Brooks II.), and of the new planet 258 (Luther), made at the Paris Observatory (equatorial of the West Tower), by M. G. Bigourdan. The new planet 258 was discovered by Dr. R. Luther at Düsseldorf on May 4 at 10 o'clock mean Düsseldorf time, when it occupied the position: 14h. 20m. R.A.; 9° 31′ Decl. When observed in Paris on May 7 it had the appearance of a planet of the twelfth magnitude.—Observations of the Brooks comets (1886) made at the Lyons Observatory, 6-inch Brunner equatorial, by M. Gonnessiat. Brooks I. appears like a diffused nebulosity with diameter of about 2′, and but slight central condensation. On May 4 the head of No. II. was bright narrow, and elongated in the angle of position 258°; tail visible for a space of about 12′, faint, and at its extremity spreading out towards the south.—Transformation of the horary angles and declinations to azimuths and heights, by M. Vinot. To supplement Warnstorff's tables, giving this transformation for the latitude of the Altona Observatory, the author has prepared others calculated for the latitude of Paris. They are presented to the Academy in the hope that, if preserved, they may enable other observers to dispense with long and tedious calculations.—On the employment of crusher manometers for the measurement of the pressures developed by explosive substances, by MM. Sarrau and Vieille. Two cases are recorded, in which the maximum pressure is accurately determined by the measured value of the crushing force.—Remarks on M. Ledieu's communication regarding marine engines, by M. Aug. Taurines. Attention is drawn to some errors in this communication presented on March 23, 1885, where M. Ledieu describes the dynamometric experiments made on board the corvette *Le Primauguet*, which he mistook for a simple aviso.—Note on certain sounds produced in vibrating metal plates by the discharges of static electricity, by M. E. Semmola. The conditions are described under which these sounds occur, but no theory is offered in explanation of the phenomenon.—Secondary electrolysis, by M. E. Semmola.—The island of Ferdinandea. the blue sun, and red after-glows of 1831, by M. A. Riccò, With a view to the elucidation of the crepuscular lights of 1883–84, the author gives a detailed account of the analogous phenomena which accompanied the appearance of the island of Ferdinandea in the Sicilian waters in the year 1831. It is pointed out that the atmospheric effects attending the eruption of Ferdinandea closely resembled those following that of Krakatão. But the ashes took no part in the production of the blue sun and red after-glows of 1831; consequently, the ashes of Krakatão would also seem to have had nothing to do with the similar light effects of 1883–84.—Note on the extraordinary halos seen at the Observatory of Parc Saint-Maur on March 29, 1884, and, with still more interesting light effects, on May 3, 1886, by M. E. Renou.—On products of decomposition of hypophosphoric acid: secondary hydrate, by M. A. Joly.—On the definite compounds of hydrochloric acid with the chloride of zinc, by M. R. Engel. The experiments here described have confirmed the theoretical view already advanced by the author, regarding the probable existence of one or more hydrochlorates of the chloride of zinc stable at the ordinary temperature.—On the combinations of quinone with the benzenic phenols, by MM. Ph. de Clermond and P. Chautard.—Action of the perchloride of phosphorus on the hydrocarburets, by MM. Alb. Colson and H. Gautier. It is shown that by means of the perchloride of phosphorus it is possible to substitute chlorine for hydrogen in the aromatic carburets. It thus becomes possible to prepare the symmetrical chlorurretted compounds in the fatty and aromatic series to the exclusion of the isomerous substances, which always accompany them in all other methods of preparation.—On the rancid element in butter, by M. E. Duclaux.—Note on sozolic acid (orthoxyphenylsulphurous acid), by M. Serrant. This acid, whose formula is $C_6H_4OH_{(1)}SO_2OH_{(2)}$, is described as even a more powerful antiseptic than salicylic and phenic acid. Being perfectly soluble, it may be taken inwardly without any inconvenience, and is rapidly and completely eliminated from the system.—On the position in the crab of the parasite *Sacculina carcini*, by M. A. Giard.

Berlin

Physiological Society, April 9.—Dr. Goldscheider spoke on the effect of menthol on the nerves of temperature. It was known that menthol (which for headaches has been extensively applied) generated a keen feeling of cold on being spread over the forehead. It was assumed that this feeling of cold resulted from the cooling of the skin consequent on evaporation. On the other hand, it was explained that the feeling of cold in the mouth produced by mouth washes containing mentha was due to an astringent effect of the mentha. The speaker had come to the conclusion that the two explanations referred to in the respective cases were neither of them correct. He made his experiments with a solution of menthol in lanoline, which was rubbed into circumscribed places of the skin. Measured with the thermometer, the places of the skin in question showed after the rubbing an increase of temperature of about 20° C., and yet for all that there was a quite decided feeling of cold. This feeling of cold was also observed when the place where the solution was rubbed in was protected against evaporation by a watch-glass. The feeling in question could proceed therefore only from a direct stimulation of the nerves of cold sensation. If of two places on the forehead exactly corresponding to one another, the one were rubbed with menthol salve and the other not, then bodies which before had produced no impression, as being indifferent, would now be felt as cold by the part of the skin where the rubbing was made, whereas there would be no perceptible impression at the other part. From these and several other experiments the speaker

concluded that the menthol exercised a specific influence on the nerves of cold, which were distributed with especial copiousness on the forehead. Menthol produced an effect on the nerves of warmth and the nerves of feeling of less amount than on the nerves of cold. A sensation of warmth after the rubbing in of menthol was obtained only at spots which were very rich in nerves of warmth. This was most easily obtained on the volar side of the lower part of the arm in the neighbourhood of the elbow joint. As analogous to his menthol experiments, the speaker called to mind how Prof. Herzen had quite recently communicated the observation that moderate pressure on the nerve-trunk produced a different effect on the cold feeling nerve-ends than on the warm feeling nerve-ends.—Prof. Albrecht, from Brussels, developed his views on the morphological significance of the auditory ossicles of the middle ear, of the external ear, and of the Eustachian tube. Respecting the auditory ossicles there had hitherto prevailed two views. There was, first, the German view, represented by Prof. Gegenbaur, according to which the joint between malleus and incus corresponded to the quadrato-mandibular joint of the lower vertebrates, incus answering to the os lenticulare, stapes to the os quadratum, and malleus to the os articulare. The second view was the English one, set up by Prof. Huxley, according to which all four auditory ossicles of the mammalia were homologous with the os quadratum. The speaker considered both views to be incorrect. As to the latter, the four auditory ossicles of the mammalia, seeing they lay between the fenestra tympanica and the fenestra ovalis, must in his opinion be the homologue of the columella of the reptiles, amphibia, and birds, which likewise extended from the fenestra tympanica to the fenestra ovalis. The columella itself was the homologue of the symplectico-hyomandibulare of the fishes. The auditory ossicles had nothing whatever to do with the quadrato-mandibular joint. The os quadratum of the lower vertebrates must, on the contrary, be sought for at an entirely different place, in the lower part, namely, of the pars squamosa of the temporal bone. At this place Prof. Albrecht had in point of fact observed in different cases fissures by which the superior part was separated from the zygomatic part, the proper os quadratum. The middle ear was, in the opinion of the speaker, divided by the columella into two sections, of which the anterior, the precolumellare, was, through the Eustachian tube, brought into connection with the larynx, and, through the anterior part of the tympanum, with the external organ of hearing. This whole section of the ear was, according to the view of Prof. Albrecht, the remains of a special pharyngeal gill-segmentation.—Prof. Flesch, as guest, communicated some results of his investigations into the peripherical nervous cells. The question of the histological diversity of the nerve-cells, which, by the labours of Stieda, had been solved in a negative sense, had again been taken up by Prof. Flesch. In order to a settlement of the question, he had applied himself to the peripherical nerve-cells and to different methods of staining. It was the colouring method with Weigert's hæmatoxylene and treatment with osmic acid which especially yielded beautiful results. The osmic acid had been used on quite fresh preparations, at most five to ten minutes after the death of the animal. The fact at once established itself that the nerve-cells, under precisely the treatment and under perfectly the same conditions of experiment, showed variations which were not artificial products. It was, first, possible to distinguish between stained and colourless cells. The former were mostly small, the latter large. The relation of the large pale cells to the small dark cells was a perfectly constant one, and that even in the case of different animals. In the peripherical ganglia the pale cells constantly amounted to 20 per cent., the dark to 80 per cent. In the spinal marrow, on the other hand, the number of the pale cells invariably amounted to about 40 per cent. On further investigation it came out that little colourless cells also occurred in small number. The occurrence of these differences among the nerve-cells under use of the most varied staining means and in various animals, especially, however, the determinate numerical relation of the various groups of nerve-cells in the peripherical ganglia and in the spinal marrow, were deemed by the speaker to be proofs that there was here a question of physiological variations. This difference might be of manifold significance. In the first place there might here be a question of various stages of development on the part of the nerve-cells—young, adult, and senile forms. In the second place the various forms might be the expression of a different nature on the part of the nerve-cells: one set being, possibly, motory, another sensory, and so on. In the third place and lastly, these various forms might, in a manner similar to what had been observed in the glandular cells, be the expression of different states of activity or of rest on the part of the nerve-cells. By way of arriving at a decision among these different possibilities, Prof. Flesch had had a series of experimental investigations undertaken which had not yet come to a conclusion. The probability, however, was that the experiments in question pointed to functional variations on the part of the nerve-cells which were the subject of investigation.

BOOKS AND PAMPHLETS RECEIVED

"The Journal of the Anthropological Institute of Great Britain and Ireland," May (Trübner).—"Bulletin of the Buffalo Society of Natural Sciences," vol. v., No. 1 (Buffalo).—"Bulletin of the U.S. Geological Survey," Nos. 15-23 (Washington).—"Bulletin of the U.S. National Museum," No. 23, by N. P. Scudder (Washington).—"Chemistry of the Gold-Fields," by J. G. Black (Horsburgh, Dunedin).—"The Monthly Weather Report of the Meteorological Office," December 1885 and January 1886.—"The Quarterly Journal of the Geological Society," May (Longmans).—"Report of the New York Meteorological Observatory, 1885."—"Manual of Operative Surgery," by W. A. Lane (Geo. Bell and Sons).—"Meteorological Record for Quarter ending December 31, 1885," by W. Marriott (Stanford).—"Quarterly Journal of the Royal Meteorological Society," April (Stanford).—"The Colloquial Faculty for Languages," second edition, by Dr. W. H. Walshe (Churchill).—"Proceedings of the Society of Natural History, St. Petersburg," vol. xvi.—"Annual Report and Proceedings of the Belfast Naturalists' Field Club, 1884-85" (Mayne and Boyd, Belfast).—"The Rotifera or Wheel-Animalcules," part iv., by C. T. Hudson and P. H. Gosse (Longmans).—"Engineering Education at Home and Abroad," by E. Mitchell (London).

CONTENTS

THURSDAY, MAY 27, 1886

A HAND-BOOK TO THE HISTORY OF PHILOSOPHY

A Hand-Book to the History of Philosophy. For the Use of Students. By Ernest Belfort Bax. Bohn's Philosophical Library. Pp. 405. (London: George Bell and Sons, 1886.)

THE task to which Mr. Bax has set himself in writing a short and at the same time intelligible account of the history of philosophy is anything but an easy one. The historian of philosophy finds himself in presence of an enormous amount of material, which has accumulated as system has followed system in what at first sight seems a bewildering succession. It will naturally occur to the reader to ask whether after all the history of superseded systems of philosophy is of much more than antiquarian interest, or whether at best its study can be expected to repay the necessary trouble.

There is a pretty widespread idea that philosophy, or "metaphysics," has led to nothing but disappointing failures in the past, and cannot from its very nature lead to any result of real value. This idea would appear to depend on a radical misconception as to the nature and scope of philosophical inquiry. Those in search of first principles in any department of science cannot fail to come across questions which they find cannot be solved by the methods which may be applied to the ordinary questions occurring within their science. Such questions occur naturally and necessarily in mathematics, physics, biology, art, &c., and in virtue of their similarity may be all classified as philosophical questions. As a familiar example we may take the question, which must necessarily come prominently before the physiologist, as to whether consciousness is a function of the body. It is pretty generally acknowledged that this question cannot be solved by experimental methods. The question is a philosophical one, and can only be attacked by the method of philosophy.

Let us see what this method is, and how it is to be applied to the case in question. Mr. Bax defines philosophy as the result of the endeavour to reconstruct the world according to its possibility. Applying the method here indicated we have to ask whether it is possible to conceive the world of our experience on the supposition that consciousness is a function of the body. This was substantially the question which Locke set himself to answer; and it was finally answered by Hume, who showed that the supposition in question led necessarily to its own annihilation. It remained for Kant and his successors to point the way to the only hypothesis consistent with the facts.

Such being the scope and method of philosophy, we may readily understand that its history is no mere record of an arbitrary series of speculations successively displacing one another, but never leading to any permanent result. Modern philosophy has centred round the discussion of the relation of matter and thought; and its successive systems form so many landmarks in the progress towards a solution of the fundamental questions involved in this discussion. Each system is doubtless more or less burdened with superfluities and errors of detail; and many philosophical works have been written by men famous in their day, but who failed to realise the true position of philosophical thought in their time, and thus cannot be assigned a permanent place in the history of philosophy.

Progress in philosophy is nevertheless just as well marked as in any department of science; and it is the special merit of Mr. Bax's hand-book that this progress is everywhere clearly brought into prominence.

There is one important respect in which the history of philosophy differs from that of any of the sciences, and which gives it a far greater relative importance. The detailed results of a science have a value of their own more or less independently of theoretical considerations or of other facts of the science in question. Thus the experimental results of chemistry have each a value independently of the truth of the vast majority of the other experimental results of the science, and even of the atomic theory itself. In philosophy, on the other hand, the conclusions arrived at are closely interdependent, and have no value apart from the general conception to which they belong, and the process by which that conception has been arrived at. It is as if the conclusions of chemistry were entirely valueless apart from the atomic theory and its correct application in detail. If this were so, it is evident that the history of the atomic theory and its application would be the first essential for the student of chemistry, instead of being what, as a matter of fact, many students of chemistry have only a very hazy notion of. Philosophical conclusions may be said to include the process by which they have been arrived at, so that a knowledge of the history of philosophy is in reality the basis of all study of philosophy. For this reason it will probably only lead to perplexity and disappointment to attempt the study of any philosopher without knowing the point at which he took up the work of his predecessors. Just as the individual organism shadows forth in its own development the forms assumed in the evolution of the stock to which it belongs, so the student of philosophy must repeat in his own mind the essential points in the historical development of philosophy.

A detailed criticism of Mr. Bax's work would scarcely be in place here. The book is on the whole an excellent piece of work. It is less of a summary, and much more readable, than the similar work of Schwegler, and for this reason will probably be preferred by English students. Due weight is as a rule given to the elements in any philosophical system which were of permanent value in influencing subsequent thought, while systems which were in reality anachronisms, however much stir they may have made, are passed over rapidly.

There are few positive blots in the book. One of these, cropping up in one form or other at various places, consists in the writer's persistent identification of the "anti-worldliness" of Christianity with "other-worldliness." As regards this and other kindred subjects the candour of Mr. Bax's expressions of opinion will, however, thoroughly commend themselves to the reader.

J. S. HALDANE

ELECTRICITY TREATED EXPERIMENTALLY

Electricity Treated Experimentally. By Linæus Cumming, M.A. (London: Rivingtons, 1886.)

THOSE who are acquainted with Mr. Cumming's "Introduction to the Theory of Electricity" will welcome most heartily a new and excellent little work from his pen. The book before us is on "Electricity Treated Experimentally"; and it is highly to be commended. It is admirably clear and concise, and at the same time the information is full and is well arranged; while the multitude of excellent illustrations and the open double-leaded type make the little book very pleasant and satisfactory reading.

The portions devoted to magnetic and electric measurements, both electro-static and electro-kinetic, are, as we should expect from the author, clear and full; while the descriptions of the various measuring instruments are very satisfactory. An excellent account is also given of Faraday's experimental investigations in electro-statics and electro-magnetism, and of those of Ampère in electro-dynamics.

The least satisfactory portion of the book is the chapter headed "Current Induction." This chapter, even making all allowances for its necessary brevity, requires very considerable improvement and amendment. The descriptions given of dynamo-electric machines are very far from adequate, even to the extent of making little or no distinction between a magneto-electric machine and a so called "dynamo." Under the heading "Siemens Dynamo" there is a description and diagram of the old Siemens shuttle-wound armature; and Fig. 218, which is a diagram of a Gramme magneto, shows the soft iron of the armature cut away almost to nothing to make space for the armature. The information given with respect to the incandescent lamps and incandescent lighting also requires improvement to make it suitable for the present day; and the description of the telephone and of experiments to illustrate the action of it are not satisfactory. Some of these instruments it is perhaps unnecessary to treat of in a book of this class; but if they are dealt with at all the treatment must be correct and not too meagre.

One or two other minor matters we cannot avoid mentioning. The first is the naming of the magnetic poles. It is greatly to be desired that strong efforts should be made by all teachers to get rid of the English "north" and "south." Most writers of importance are doing this now; either by adopting "blue" and "red" for *true* north and *true* south respectively, or else by using in full the designations "true north" and "true south." However this may be, the practice of marking the ends of a magnet + and − seems to us thoroughly objectionable.

Next we would call the author's attention to the fact that the rule which he has called Oersted's rule for finding the direction in which a magnet turns under the influence of a current is commonly, and we believe rightly, called Ampère's rule. But it would be of very great advantage if Ampère's rule were improved out of existence, and some such rule substituted as that "terrestrial currents *supposed* to correspond with terrestrial magnetism follow the sun." When the unfortunate student imagines himself lying on his face, or (?) back, with a current entering by his feet, or (?) head, and stretches out his right hand, or (?) left, to show the direction of the deflection of the magnet, the probabilities against his coming at the end of his imagining to a correct conclusion are considerable. It seems strange that such a rule should have held its place from Ampère's time till now.

Lastly, we miss the name of Cavendish and his proof (by means of the experiments of Faraday so well described) of the electro-static law of the inverse square of the distance. It is impossible, by means of the torsion-balance, to give anything but a rough proof of this great law. But Cavendish established mathematically that no other law than that of the inverse square of the distance will account for the whole electric charge being found on the outside of a closed conductor; while the experiments of Faraday established to minute accuracy this celebrated law of electric distribution. In searching for the name of Cavendish, too, an alphabetical index would have been of much assistance. It is sad for a reviewer to take up a book without an index! No book, unless it be a novel, should be without one. For small books it is easily made; for large books it is essential.

With these criticisms we must take our leave of Mr. Cumming's book; but we cannot do so without remarking once more that it is one of the pleasantest and most thorough little books on electricity and magnetism with which we are acquainted.

J. T. B.

OUR BOOK SHELF

Constructive Geometry of Plane Curves. With Numerous Examples. By T. H. Eagles, M.A. Pp. xx., 374. (London: Macmillan and Co., 1885.)

THIS book differs considerably from previous treatises on practical geometry. The author has made a serious attempt to improve the instruction usually given in his subject, and the result is that we have a text-book which will lend itself to class-teaching of a thorough and searching character.

Hitherto much time has been spent on constructions which furnish no mental discipline. In this treatise the proofs of the methods used are given or indicated in every case.

A valuable collection of examples is supplied at the end of each chapter. If a numerical result is involved, the answer is usually appended, and hints are given towards the solution of the more difficult examples.

Two-thirds of the book is devoted to conic sections, and herein we find methods of drawing these curves under almost any conceivable conditions; there are also chapters on reciprocal polars and the anharmonic properties of conics which will give the draughtsman some indication of the power of modern geometry and of its usefulness in practical application.

After a chapter on conics as derived from plane sections of a cone, we have about 100 pages devoted to various other curves which are of interest in mechanics or physics. Compared with the exhaustive treatment of the conic sections, the account of several of these curves is somewhat scanty.

We should like to see more space given to equipotential curves, for instance, and to have further exemplification of the methods of construction adopted by Rankine and Maxwell.

The book closes with an interesting chapter on the graphical solution of quadratic equations and certain trigonometrical equations.

Scientific Memoirs by Medical Officers of the Army of India. Edited by B. Simpson, M.D., Surgeon-General with the Government of India. (Part I., 1884). (1) On the relation of cholera to Schyzomycete organisms, by D. D. Cunningham. (2) On the presence of peculiar parasitic organisms in the tissue of a specimen of Delhi boil, by D. D. Cunningham. (Calcutta, 1885).

IN the first of these memoirs Dr. Cunningham makes some interesting additions to our knowledge of the presence and distribution of comma bacilli in the intestinal contents in cases of Asiatic cholera; on the occurrence of peculiar comma bacilli associated with the scum formed on tank water by Euglenæ; and on certain modifications in morphological and other characters in artificial cultivations of the choleraic comma bacilli.

The second memoir gives a minute description of the anatomical nature of the skin disease known as "Oriental sore" or "Delhi boil." This description is the more valuable as it is the first accurate account that we possess of the minute anatomy of this interesting malady. The value is enhanced by the discovery by Dr. Cunningham in the diseased tissue of a peculiar fungus bearing the characters of Mycetozoa or Myxomycetes, more especially of the subdivision of the Monadinæ; the distribution of this fungus is such that a causal relation of it to the disease process becomes highly probable.

The memoir is illustrated by numerous fine lithographs, many of them coloured. E. KLEIN

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

On the Thomson Effect as Expounded by Prof. Tait

AMONG modern expositions of the subject of thermo-electricity there is none so full, and on the whole so instructive to students, as that contained in Prof. Tait's "Heat." It is therefore the more important to call attention to what appears to me to be, to say the least, a very questionable statement there made. It refers to the Thomson effect.

Thomson's experiments were of the following nature. A metallic bar was surrounded with a hot-water jacket in the middle and with cold-water jackets at the ends, and there were two holes sunk in it for the insertion of thermometers, midway between the hot jacket and the two cold jackets. When the flow of heat had become nearly steady, a steady current of electricity was sent through the bar; and, after it had flowed for several minutes in one direction, it was reversed; then, after the same number of minutes, it was again reversed, and so on several times. It was thus found that, when the bar was of copper, the current made the temperature of the further thermometer higher than that of the near one (the words *far* and *near* being used with reference to the end at which the current entered). When the bar was of iron, the current made the temperature of the near thermometer higher than that of the further one.

Seeing that a current may be regarded at pleasure as the flow of vitreous electricity in the nominal direction of the current, or as the flow of resinous electricity in the opposite direction, Thomson summed up his results by saying that "the vitreous electricity carries heat with it" in copper, and "the resinous electricity carries heat with it" in iron. He also gave the name of "electric convection of heat" to the effect thus detected. It has since been called by others "the Thomson effect."

The experiments were instituted to test the truth of a conclusion of which he had previously given a theoretical proof—the conclusion that "in one or other of the metals, and most probably in both, there must be a thermal effect due to the passage of electricity through a non-uniformly heated portion of it, which must be an absorption of heat [a cooling] or an evolution of heat [a warming], according to the direction of the current between the hot and cold parts."

It may be taken to be an established fact that, in a uniform linear conductor along which a current is flowing, there is, in addition to the frictional heating, which is proportional to the square of the current, a warming or cooling effect proportional (at given temperature) to the steepness of the thermometric gradient at the point which is warmed or cooled, changing sign with the gradient, and vanishing at points of maximum or minimum temperature, where the gradient vanishes.

Now compare these effects with what happens when a stream of liquid flows through a pipe surrounded at alternate points in its length with hot and cold jackets, the average temperature of the water being the same as the average temperature of the pipe. It will carry heat from the hotter to the colder portions, thus cooling the hottest parts, warming the coldest parts, and at the same time carrying forward the points of maximum and minimum temperature. If, at each point of the pipe (supposed straight and horizontal), we erect an ordinate to represent its temperature, and call the curve of which they are the ordinates "the temperature curve," the effect of the flow of liquid on this curve will be twofold: (1) it will carry the temperature curve forward; (2) it will make the temperature curve flatter.

Thomson's experiments show that an electric current carries the temperature curve forward in copper, and backward in iron; but I am not aware of any evidence to show that it makes the temperature curve flatter.

The analogy between the Thomson effect and convection of heat by a liquid in a pipe therefore does not run on all fours, and must be used with caution.

Maxwell says ("Elec. and Mag.," p. 343, second edition), "positive electricity in copper, and negative electricity in iron, carry heat with them from hot to cold." The words "from hot to cold" are here added to Thomson's original phrase "carries heat with it," and the addition thus made is not in accordance with facts, for it implies that heat is taken away from the hot parts and given to the cold parts; whereas the fact is that heat is taken from parts where the temperature gradient is in one direction, and heat is given to parts where the gradient is in the opposite direction. If the statement be altered by a little transposition, so as to make it stand thus, "positive electricity in copper, and negative electricity in iron, going from hot to cold, carry heat with them," it will be scarcely distinguishable from Thomson's original statement.

Prof. Tait goes further, and says ("Heat," p. 170):—"After a series of elaborate experiments (described in the *Phil. Trans.* for 1855) [it should be 1856] Thomson found that:—

"*An electric current in an unequally heated copper conductor behaves as a real fluid would do*, i.e. *it tends to reduce differences of temperature. In iron it tends to exaggerate them.*"

The italics are Prof. Tait's.

I can find nothing in Thomson's paper to support the assertion that in copper an electric current tends to reduce differences of temperature, though the idea that it does so is naturally suggested by the analogy implied in the phrase "electrical convection of heat."

The statement that in iron the current tends to exaggerate differences of temperature, seems to be completely original on the part of Prof. Tait. It does not arise naturally out of Thomson's dictum, "resinous electricity carries heat with it in iron"; for if we think of resinous electricity as a real fluid flowing through iron, it would tend to equalise differences of temperature in that metal.

The two statements taken together suggest the following line of reasoning as conclusive against them both:—

Let there be the same initial distribution of temperature in a copper and in an iron bar, and currents in the same direction through both. Then the alterations of temperature at corresponding points in the two bars will have opposite signs. Any one who maintains that the warmest parts of the copper are cooled is therefore bound to maintain that the warmest parts of the iron are warmed. But there is precisely the same ground for maintaining that the warmest parts of the iron are cooled, and therefore the warmest parts of the copper warmed. Whatever vitreous electicity can do in copper, resinous electricity can do in iron. We are thus involved in a contradiction if we assume any finite heating or cooling at the hottest parts. And similar reasoning disproves any finite heating or cooling at the coldest parts.

The following formal investigation confirms the view which I have above expressed.

Leaving out of account frictional generation of heat (in other words, the effect which varies as the square of the current, let $\sigma d\theta$ be the heat generated in unit time by unit current in a uniform copper bar, in passing from a section where the temperature is $\theta + d\theta$ to one where it is θ (see Thomson's "Papers," vol. i. p. 246). Let x be distance along the bar in the direction of the current, and c the thermal capacity of unit length of the bar. The heat generated in a short length δx is $-\sigma\frac{d\theta}{dx}\delta x$, and the consequent rise of temperature in this portion is $-\frac{\sigma}{c}\frac{d\theta}{dx}$. This is in unit time. Hence, putting t for time, and v for $\frac{\sigma}{c}$, we have—

$$\frac{d\theta}{dt} = -v\frac{d\theta}{dx}.$$

If the limits of temperature are not very far apart, it is known that σ and c are sensibly independent of θ; hence v may be treated as a constant. The integral of the above equation is then—

$$\theta = F(x - vt),$$

where F is a functional symbol such that $\theta = F(x)$ expresses the original distribution of temperature. The interpretation is that the original "temperature curve" travels forward with velocity v without flattening or any other change of form.

Belfast, May 14 J. D. EVERETT

Scientific Nomenclature

SOME time ago Mr. "John O'Toole," in the columns of NATURE, waged war against that hideous monstrosity *Potential Energy*, and he very aptly summarised his case against this term by saying that it involves, by the very signification of words, "a double remotion from actuality."

A few months ago it occurred to me that to express what is intended by the potential energy of any system the term *Static Energy* is not only logically unobjectionable but specially fit for the purpose. Thus, if a string or a membrane is stretched, a wire bent and twisted, or, generally, a body strained in any manner, the work which it can do against resistance in returning from its state of strain to its unstrained condition is the *Static Energy* of the system in the strained condition. The work which a moving system can do in virtue solely of its motion is, of course, its *Kinetic Energy*.

Thus we have simply *Static Energy* and *Kinetic Energy*, and these terms have the further advantage of harmonising with the ordinary subdivisions of dynamics.

I may add that the term *Static Energy* has received the approval of all the mathematical physicists to whom I have submitted it. GEORGE M. MINCHIN

R.I.E. College, Cooper's Hill, May 19

Pendulum Oscillation

THE oscillations of a long pendulum are observed to describe an ellipse the axis of which tends to set itself at right-angles to the plane in which the pendulum was started. An explanation of the above phenomenon would much oblige. M. H. MAW

Walk House, Barrow-on-Humber, Hull, May 18

What is Histioderma?

Histioderma hibernica was described by Dr. Kinahan in 1858 as an annelid, and most writers who have since mentioned it have also regarded it as such. Dr. Haughton notices and figures it in his "Manual of Geology," Sir R. I. Murchison places it amongst annelids in his "Siluria," as also Dr. Bigsby in his "Thesaurus Siluricus." Writing from memory only, I think a description and figure will be found in W. H. Baily's useful compendium of Palæozoic British fossils, published a few years ago. The fossil is mentioned with more or less detail by numerous writers on Cambrian strata.

The above references will perhaps answer the letter signed "S.," but there still remains the question, What is Histioderma? It is apparently a fossil impression in the rocks of Bray Head, Wicklow, Ireland. Only one species and only one locality is, I believe, known. It seems to me very doubtful what kind of animal made the impression—whether an annelid, or otherwise. I am inclined to doubt if the rocks are Lower Cambrian, as geologists generally suppose, because the evidence seems to indicate that the Bray Head rocks had become hardened and raised into land before the oldest Lower Cambrians of Wales and England were deposited. I would therefore pass on the question and ask, What is Histioderma?

4, Cowper Road, Acton, London, W. A. RAMSAY

IN answer to the above question in your issue of May 20 (p. 53), I refer your correspondent to the *Proceedings* of the Geological Society of Dublin, where he will find (*Natural History Review*, vol. v., *Proceedings* of Society, p. 150) the original description of Histioderma by Prof. Kinahan. According to its author, Histioderma is the tube of a cephalo-branchiate annelid. J. VICTOR CARUS

Leipzig, May 23

Black Skin

IN a letter lately received from Mr. Flinders Petrie, who is now in Egypt, are some remarks on "Black Skin," which I think very interesting, and perhaps the readers of NATURE may think them so too, so I send the paragraph to you.

Belvedere, Kent, May 24 F. C. J. SPURRELL

"In considering the use of a naturally or artificially black skin, we should not look so much to the requirements of the surface, which is constructed to bear variations, and has the means of cooling and maintaining a proper temperature within itself, but rather we should consider the far more delicate tissues beneath. We all know how translucent flesh is to strong light, and it can hardly be doubted that the rays of a tropical sun would light up a white man's inside considerably; whereas black skin would stop out the solar energy of light, heat, and chemical rays effectually. Skin heat is of no importance, as perspiration can always keep that down. May not the oiling of the skin in hot countries be partly to make it reflective, so that it should absorb less heat? And may not the regard white races have for clothing be partly for the purpose of keeping the insides of their bodies sufficiently in the dark?"

Male Animals and their Progeny

CAN any of your correspondents inform me whether any of the male wild animals in foreign countries show any love for, or recognition of, their progeny? In this country amongst the domestic animals it does not seem to exist, save in the case of the gander, who carefully guards the goose while sitting, and attends to the goslings when hatched; but the cock pays no attention to the hen while sitting nor when with chickens, nor does the dog, the bull, the horse, nor the boar evince any sign of parental feeling under circumstances favourable to its development.

One peculiarity of geese is, I think, worthy of notice, for it is not possessed by ducks or fowls, who also live in flocks. If when goslings are hatched, they are permitted to run with a goose in company with the other geese of the flock, all chances of any eggs being laid by the other geese who have no goslings are over. P.

Birds and Mirrors

FOR six days I have been for two or three hours in the club reading-room, where t ere is a large mirror. During all this time a cock and hen sparrow have been flirting with and bowing to their images in the glass with evident pleasure—rushing along the mantel-shelf, flying to the top of the frame, or resting for a time, always apparently happy in contemplation, never showing disappointment. F. C. CONSTABLE

Sind Club, Karachi, May 2

SURGEON-MAJOR T. R. LEWIS

DR. TIMOTHY LEWIS, Surgeon-Major Army Medical Staff and Assistant Professor of Pathology in the Netley Army Medical College, whose death, which took place on May 7, we announced last week, was a native of South Wales, and received his medical education at University College, London, and graduated as M.B. at Burlington Gardens. He was selected about twenty

years ago, together with Dr. Cunningham, to proceed to India in order to undertake, under the auspices of the Indian Government, a prolonged study of the causation of cholera, especially in regard to its reputed relation to parasitic organisms. At that time little was known or thought about Bacteria, and the public mind had been aroused by Hallier's (now long-exploded) theory of a rice-fungus as the cause of cholera, just as more recently it has responded to Dr. Koch's invitation to believe in the comma-bacillus. Dr. Lewis and his companion were authorised to visit Prof. Hallier at Jena and Prof. de Bary (in those days not attached to French Strasburg!) for the purpose of acquainting themselves with methods of mycetological research before proceeding to India. A few weeks was all the time allowed them for this visit, and consequently they took little to India excepting their own conscientious habits of work and that modicum of knowledge of microscopic *technique* which was considered sufficient for the highest medical qualification in England in those days. Nevertheless these observers made most valuable and minute researches on the microscopic organisms present in the dejecta of cholera-patients, which were published by the Government of India. Dr. Lewis extended his researches into the general question of the presence of microscopic organisms in the blood and tissues of man in health and disease, and was led to some very interesting discoveries. His results were published from time to time by the Government of India, and were re-published as they appeared in the *Quarterly Journal of Microscopical Science*.

Dr. Lewis's work was always remarkable for the extreme care with which positive results were asserted, and for the fairness with which the researches of predecessors in the same field were considered and discussed. His most remarkable discovery was that of the little nematoid worm occurring in the blood of persons suffering from a form of chyluria and elephantiasis, to which he gave the name *Filaria sanguinis hominis*. This discovery was published in 1872. Some years later Dr. Bancroft discovered in Australia the adult worm from which the brood of minute blood-parasites is derived, and, still later, an unsuccessful attempt has been made by Dr. Patrick Manson to show that the young pass an intermediate stage of existence in the alimentary canal of gnats, which suck them in together with the blood of worm-infested persons.

It is no small thing in these latter days to discover a new human parasitic worm of great pathological significance, and it was in recognition of this discovery, as well as in view of his important contribution to the discussion of "the cholera-bacillus theory," that the Council of the Royal Society in last April selected Dr. Timothy Lewis as one of the fifteen candidates to be submitted to the Society for election in next June.

In regard to the question of the relation of Bacteria to cholera and similar diseases, Dr. Lewis had a vast store of both published and unpublished observation. With characteristic caution and modesty, he had refrained from dogmatising on the subject. Working for twelve years in Calcutta, with daily access to cholera patients, he was thoroughly familiar with the several different forms of Bacteria which are to be found in the alimentary tract and in the dejecta of choleraics. Unlike some of his recent successors in this line of research, Dr. Lewis was also familiar with the different forms of Bacteria which occur in the *healthy* human mouth and intestines, and in potable waters. He was therefore able to demonstrate immediately on the publication of Koch's figures of the so-called "comma-bacillus" that this *form* (asserted by Koch to be peculiar to cholera evacuations) was nothing more nor less than a *Spirillum* broken by manipulation, and commonly to be found in the mouth of healthy persons. The importance of this contribution to the controversy excited by Dr. Koch's statements cannot be too highly estimated. Its accuracy was universally recognised at once, and has never been called in question. Dr. Klein has since come to the conclusion that not only are organisms of the exact *form* of Koch's cholera-comma abundant in the healthy saliva, as shown by Lewis, but that some of these forms have precisely the same physiological conditions of growth, and precisely the same action upon gelatine as Koch considered to be characteristic of those obtained from cholera evacuations.

At the time of his death Dr. Lewis was carrying on in his laboratory at Netley an extensive series of culture and inoculation experiments, chiefly upon the Bacteria which occur in the alimentary canal of man.

Those who enjoyed his personal friendship valued Dr. Lewis for his warm-heartedness no less than for the rare combination of enthusiasm with caution in his work which gives his published results a very special value. It is perhaps some satisfaction to his friends to know that he had heard of the recognition of his merits by the Royal Society Council before the commencement of the attack of inflammation of the lungs which so rapidly ran to a fatal termination. E. R. L.

A SKETCH OF THE FLORA OF SOUTH AFRICA

UNDER this title we would draw the attention of botanists to a very able essay on the botanical regions of South Africa, contributed to the "Official Hand-Book of the Cape of Good Hope" for 1886, by Harry Bolus, F.L.S., an accomplished botanist, who has devoted many years to the investigation of South African plants.

That extra-tropical South Africa is one of the most varied botanical regions on the globe is a fact familiar to both botanists and gardeners, from the days of Linnæus,—who epitomised its richness in the expression, "Ex Africâ semper aliquid novi,"—and of the earliest cultivators of greenhouse plants, who were indebted to the Cape of Good Hope, far more than to any other regions of the globe, for what were, and till Japan and Australia eclipsed it, the prime favourites of the conservatory. There are those still alive who can remember the time when plant-houses were ornamented with little else than Cape Heaths, Pelargoniums (miscalled Geraniums), Polygalas, Proteas, Oxalis', Mesembryanthemums, Everlastings, Stapelias, Irideæ, and Cape bulbs innumerable, and when the illustrated horticultural serials of the day were either devoted to these, or contained figures of more of the plants of this than of any one other country. It is true that the cultivation of all but a very few of the heaths and geraniums has been abandoned for things of not greater beauty, and of far less interest, but this is due, not to want of appreciation, but in the case of some to their not being amenable to the treatment of the "soft-wooded" plants now in vogue, and of others to the fact that their flowering period—of the bulbs especially—is a very brief one, and that the flowers soon fade when cut.

To return to the little essay before us: the attempt to define the South African regions of vegetation is not a new one; it had been essayed by Meyer and Drège, Zeyher, Griesebach, and others, but not successfully; and the author of the sketch under consideration is the first who has succeeded in presenting satisfactorily the salient botanical characters of that flora, as affected by, or in correspondence with, geographical and other physical conditions; whilst he alone has given such vivid pictures of the vegetation of the different botanical regions he has defined, that any one with even an elementary knowledge of South African plants can fancy himself travelling over the ground.

The two dominant features of the South African flora are, the number of orders, genera, and species that it contains, and the limitation of great groups of these

within very narrow and well-defined areas. There are five of these areas, differing from one another in the aspect as well as in the composition of their floras more decisively than do any other five contiguous areas of similarly small extent on the surface of the globe. These five together have been estimated to contain the extraordinary number of 14,000 species of flowering plants! which are comprised under 200 natural orders (nearly three-fourths of the known orders of plants), and 1255 genera (one-sixth of the described genera of the whole world). Though possessing no truly Alpine region, it is by far the richest extra-tropical area on the globe in respect of genera and species, and is probably not surpassed by any tropical area of equal extent; a circumstance which may be taken together with the fact, that the vast proportion of the species are low herbs or small shrubs—trees being very rare both in species and individuals, and that there is not a single arboreous genus of more than a very few species. There is no dominant genus of trees like the Eucalypti of Australia and the Conifers of northern regions, or even the oaks or beeches of Europe, that monopolise great areas and determine the absence or presence of a multitude of plants of lower stature. The following are Mr. Bolus's regions:—

(1) *The South-Western Region.*—This (which might be called the Cape proper region) extends in a curve from near the mouth of Olifant's River along the coast facing the Western Atlantic, round by the Cape of Good Hope to Cape Agulhas, and thence to near Port Elizabeth along the coast facing the Southern Atlantic. Throughout its length—about 600 miles—it maintains a breadth of between 40 and 80 miles, never more or less, and is bounded on its landward sides by mountain-ranges attaining 4000 to nearly 8000 feet in elevation—of which the eastern run east and west, the western north and south. The surface is varied with bushy, grassy, sandy, and rocky tracts, of which some appear desolate from a distance, but on examination are found to swarm with genera and species. It is a region of small-leaved herbs and bushes—of Irideæ, Orchideæ, Rutaceæ, Ericeæ, Restiaceæ, Compositæ, Proteaceæ, Polygaleæ, Mesembryanthemums, Oxalideæ, Geraniaceæ, and Leguminosæ It is that whence all the Cape plants of the greenhouses of the last generation were derived. The climate is dry, temperate, and comparatively equable, with a winter rainfall which varies excessively from 24 inches at Cape Town to 60 in some of its own suburbs, but everywhere rapidly diminishing with distance from the coast and from the vicinity of Cape Town. The few forests are near the few rivers, and their trees rarely exceed 50 feet in height. As an instance of the endemic nature of its vegetation, the genus Erica forms one of the many conspicuous examples. It contains no fewer than 300 Cape species; all, or nearly all, are confined to this region, and various other genera contain upwards of 100 endemic species. The total number of flowering plants in this region is about 4500. No temperate area of the globe of its extent is nearly so peculiar or rich. California offers but a faint counterpart; and the restriction of the majority of the species of Cistus and Ulex to the Atlantic coasts of Europe offers an even fainter example of restricted distribution.

(2) *The Tropical African Region* (which might be called the Natal region).—Unlike the western temperate coast, the vegetation of the eastern temperate retains the characteristic features of that of tropical Africa. From Port Elizabeth northwards to Abyssinia there is no sharp delimitation of floras. This region is bounded on the east and south-east by the Atlantic, and stretches inland for from 60 to 100 miles to ranges of mountains of 5000 to 10,000 feet high, which bound it on the west. The surface is varied with bush and park, which, proceeding northward, give place here and there to extensive forests, and it is traversed by many streams. The herbage, and bush- and tree-foliage, are greener than in the south-western district, and the foliage larger. The rains are summer ones, the temperature rather higher than in the west, and much more so to the north. Though there is some overlapping of the vegetations of this and the Cape region proper in the neighbourhood of Port Elizabeth and Grahamstown, the transition from one to the other is wonderfully sudden. The 300 heaths may be said to disappear bodily, as do the Cape Rutaceæ, Proteaceæ, and Orchideæ. As suddenly appear giant Cycadeæ, Aloes, leafless succulent tree Euphorbias, with different tribes of Orchideæ, Leguminosæ, and Amaryllideæ, often grouped in striking assemblages of grotesque forms, whilst a palm, *Phœnix reclinata*, reaches lat. 33° S. Along with African types, Indian abound, both in genera and species, especially to the northward.

(3) *The Karroo Region.*—Returning now to the Western Atlantic coast of South Africa, from Olifant's River to the Orange River, and thence south-eastwards, bounding the Cape proper region on the north, extends a vast shallow basin about 2000 feet above the sea-level, except towards its western or littoral boundary. It forms a curve somewhat concentric with that of the Cape region proper, and extends a little further east into the tropical African region. It is about 700 miles in length, and from 30 to 70 in breadth from the bounding mountains on the north, which are the Nieuveld Bergen, to lower ranges on the south. The surface consists of sandy, stony, or fertile plains of vast extent, traversed by river-beds, and by the courses of torrents filled by summer thunderstorms, but dry in winter. Permanent water is scarce, and (as in California) sheep have denuded large areas of native vegetation. The climate is excessive; the rainfall, chiefly a summer one, from 7 to 14 inches according to locality. During the dry season the country is a desert, but after a shower it is suddenly, but transiently, transformed into a vividly-coloured garden.

"I was amazed on visiting that desert country, after the rains of June to July 1883, to see tracts, hundreds of acres in extent, covered with sheets of living fire or glowing purple, visible from several miles' distance, caused by the beautiful Compositæ in flower; and nothing is more curious than to see this luxuriance intermingled with the black or white branches of dead shrubs killed by previous droughts, standing like ghostly intruders on a scene of merriment and joy. These charming scenes pass away all too rapidly, and in a month or two little that is beautiful remains."

The only tree is the ghastly *Acacia horrida*, fringing the dry river-beds. Of the Orders of the Cape region proper, Ericeæ, Restiaceæ, Polygaleæ, Orchideæ, Proteaceæ, Rutaceæ, almost all are absent, and a variety of so-called succulent plants appear, especially innumerable species of Mesembryanthemum, together with Portulaceæ, Zygophylleæ, Crassulaceæ, Stapelias, Ficoideæ, and dwarf Euphorbias, besides which many other genera that are never succulent elsewhere, here have species with fleshy roots, stems, or leaves. The tree Aloes of the Eastern region are fairly well represented, but by different species, and the same remark applies to the Geraniaceæ of the Cape region. The curious Elephant's-foot (Testudinaria) is a characteristic plant, as are the two species of the parasitical Rafflesiaceous genus Hydnora, which extends to Abyssinia.

(4) A very singular region or subdivision of the Karroo region is called by Mr. Bolus the "Upper Karroo," or "*region of Compositæ*," which occupies an inland broad area north of the Karroo region, everywhere distant from the coast, about 400 miles from east to west, and 150 to 200 miles in breadth. It is a mountainous country, only partially explored, and supposed to have a mean elevation of 4000 to 5000 feet. Its prevalent features are that of a treeless, heathy tract, or dry elevated moorland, covered with shrublets of a dull hue. The climate is severe, the summer nights are always cool, sharp frosts are common,

and snows falls in winter. Compositæ abound. Of nearly 1000 species that have been collected in this region, 61 genera and 231 species are referable to this Order. Of Orchids only four species have been found; Rutaceæ, Ericaceæ, and Restiaceæ are all but absent, and Proteaceæ wholly so.

(5) *The Kalahari Region* is the name Mr. Bolus gives to the vast tract of country north of the Composite region, west of the Natal region, and south of the tropic of Capricorn. It is a desert country, with an extreme climate, a rainfall of summer thunderstorms, hot summers with cool nights, and frosts in winter. Its essential features are of a country clothed with grass in tufts, and isolated shrubs and trees, which form forests in the north, and are thus probably continuous with the forests of tropical Africa. The Cape flora has here disappeared, and with it we take leave of Mr. Bolus's very able and most interesting contribution to botanical geography, regretting that want of space prevents any notice being taken of the many valuable observations and comparisons that he has made relating to the affinities of the South African with the Australian flora, and other matters of scientific interest. J. D. H.

THE INTERNATIONAL COMMITTEE OF WEIGHTS AND MEASURES

THE Comité International des Poids et Mesures, which has its bureau at Sèvres, has recently issued its ninth Report to the contracting Governments. The Report gives an account of the work done by the Committee during 1885, and a statement of the probable expenses of the Committee for the current year. During the last year new instruments for the accurate comparison of standards of the metric system have been obtained at a cost of about 500*l*., making a total cost of about 7000*l*. for instruments supplied to the Bureau. For the present year the expenses of the Bureau are estimated at 100,000 francs (4000*l*.), the annual expenditure of the Committee being limited by the Convention to that amount, of which sum about 2650*l*. is for payments to various officers of the Comité. These expenses are divided amongst the twenty contracting Governments, the annual contribution of Great Britain and Ireland amounting to about 300*l*. The new instruments include a comparator for measures of length by M. Brunner; new mercurial thermometers by M. Tonnelot; an air thermometer by M. Golaz; a spherometer by M. Brunner; and other measuring instruments by MM. Oertling, Boudin, Alvergniat, Simmen, and the Société Genevoise.

In the fourth volume of the *Travaux et Mémoires* of the Bureau (Paris, Gauthier-Villars, 1885) reference was made by the Director of the Bureau to the work then in progress, and in the present Report a summary is given of the whole work done by the Committee during the past twelve months.

The work of the Director has specially included the verification of the lengths and coefficients of expansion of several standard metres, and the determination of the weights and specific gravities of several standard kilogrammes for different Governments and scientific authorities. A report is given on the comparison of the new kilogramme prototype (K. III.) with the old kilogramme des Archives, and also on the verification at Paris, by Dr. J. Broch and Mr. H. J. Chaney, of certain British standards. During the present year the Committee report that Dr. Broch will continue his researches on the influence of light on the defining-lines of standards of length, and M. Benoit will also carry on his experiments on the best means of comparing end-measures of length (*mesures à bouts*) with line-measures (*mesures à traits*). Dr. Thiesen will continue his interesting studies on balances and the verification of kilogramme standards. Dr. Chappuis, with the assistance of Dr. Guillaume, will also continue the experiments on the verification of thermometers.

It would appear that the Committee are carrying out the duties intrusted to them with all possible care and despatch. In the preparation of the alloy of platinum-iridium, of which the standards of the kilogramme and metre are made, extraordinary difficulties were originally encountered, owing mainly to the presence in the iridium of iron, rhodium, ruthenium, osmium, and other metals, and the Committee therefore obtained the assistance of MM. Stas and Debray. The report of these eminent metallurgists shows that the Committee are indebted to Mr. G. Matthey for the production of an alloy having the high purity and finish required for such work.

The Report of the Committee includes copies of its correspondence with the different contracting Governments. In an able note to the Japanese Minister, Dr A. Hirsch (the Secretary of the International Committee) explains the objects of the Metric Bureau, and it is gratifying to find that Japan has now joined the Convention. From the correspondence with this country it would appear that our Government will in due course obtain copies of the new metric standards.

The Committee announce that the vacancies caused by the death of Prof. Heer and the resignations of Gen. Wrede and Dr. Gould have been filled by the election by the Committee of Prof. Th. v. Oppolzer, Mr. W. H. M. Christie, and Prof. Thalèn.

The Report also includes copies of a correspondence with reference to a proposal made by the French Government to extend the functions of the Bureau to the determination and verification of electrical standards of resistance and light, for the use of the different Governments. The proposal appears to have gone no farther at present than asking the several contracting Governments whether they would have objection to the preparation of estimates showing the cost of a new or extended Bureau for electrical purposes. The replies of Belgium, Denmark, Spain, the United States, Italy, Servia, and Switzerland are attached, but, excepting Spain and Switzerland, it cannot be said that generally the Governments appear to have given the proposal the most cordial welcome.

THE WEATHER OF THE ICE SAINTS' FESTIVALS OF 1886

LAST year we chronicled (NATURE, vol. xxxii. p. 62) one of the most disastrous snowstorms that ever occurred at this season in Vienna, where, on May 15, there fell $5\frac{1}{2}$ inches of snow, and the cold accompanying the storm was so intense that several persons who were exposed to it were frozen to death. Over Austria and Hungary snow covered the fields and vineyards, and, the crops being in a somewhat advanced condition at the time, an incalculable amount of damage was done. But the festivals of the Ice Saints (May 11, 12, and 13) this year have been marked off for a wider and deeper remembrance by storms of wind, rain, hail, and snow in all the continents of the northern hemisphere, which, for number and destructiveness, are perhaps unexampled at this time of the year.

In the British Islands the cold acquired its greatest intensity on the five days from May 11 to 15, and was coincident with a system of pressures which appeared in the Channel, and thence proceeded in a north-north-easterly direction through the North Sea to the south-west of Norway, which was reached on the 15th. It necessarily resulted from this distribution of pressure that northerly and easterly winds prevailed in these islands, and temperature fell correspondingly low. Over that

large portion of the British Islands lying between lat. 52° and 58°, the mean temperature of the five days was at least 6°·0 under the average of the season, and in the central portion of this district, as at York and Barrow-in-Furness, it was 8°·3 under the average. On the other hand, in Jersey the defect from the mean was only 1°·5, and in Shetland 2°·5. At Barrow-in-Furness, on the 12th, the maximum temperature was only 39°·0. On this day snow fell over the higher districts of central and southern Scotland, in many places to the depth of several inches, and the cold was so intense that swallows and some other birds perished in thousands.

Meanwhile torrents of rain were being poured down continuously over wide districts of northern and central England. These rains were heaviest and most widespread on the 11th, 12th, and 13th, on each of these days upwards of an inch being recorded in many places. Mr. Scott states in the *Weekly Weather Report* that during these three days falls of 4·83 inches occurred at Churchstoke, Montgomeryshire; 4·36 inches at Hereford; 4·15 inches at Pershore, Worcestershire; 3·38 inches at Ross, Herefordshire; and 3·44 inches at Fassaroe, county Wicklow. Extensive and disastrous floods were the consequence. The railway traffic between Worcester, Hereford, and Malvern was suspended, and the whole of the united valley of the Severn and Teme, in the neighbourhood of Worcester, was laid under water. At Gloucester large numbers of the inhabitants were driven from their homes. The valley of the Trent presented the appearance of a vast lake, and the Midland Railway between Nottingham and Derby was submerged. Immense damage was done about Chester and along the estuary of the Dee, over the low grounds bordering the Humber, and over extensive tracts of Yorkshire.

A noteworthy feature of the storm is shown in the weather maps for the morning of the 13th, when the area of low pressure extending from west to east over the south of England was broken up, and showed three satellite cyclones with still lower pressures, having their centres the first to the west of Brest, the second over Somersetshire, and the third over Kent,—these all evidencing great, well-marked, and sharp local differences in the distribution of temperature and vapour. Further, a most remarkable satellite cyclone was seen near Perpignan, in the south of France, which in all probability represented the small cyclone or tornado that about seven o'clock of the previous evening wrought such dreadful havoc in Madrid, causing 32 deaths, seriously wounding large numbers, variously estimated from 320 to 520 persons, and wrecking hundreds of houses. The area swept over by the tornado was comparatively limited, but within that area, as in the lower Carabanchel, not a house was left undamaged and many were wholly blown down, and hardly any of the inhabitants escaped uninjured. The storm was preceded by a sultry atmosphere, a heavy bank of black clouds in the north, and torrents of rain, and as so frequently occurs with the tornadoes of America, many buildings would appear to have fallen to pieces from an explosive force from within as the tornado passed overhead.

On the 14th violent cyclones or tornadoes occurred at Krossen, near Frankfort on the Oder, at Linz on the Danube, and at Lonato, near Brescia, wrecking houses, and causing great destruction in other ways; and storms of less, but yet of considerable severity are reported from other parts of the Continent. It is to be hoped that some meteorologist of mark will undertake the discussion of these remarkable storms, so as to lead, if possible, to some knowledge of the peculiar meteorological conditions of Europe out of which they originated. In this discussion no little help will be given by the high-level observatories now established at so many points over Europe.

In the United States on May 11 a tornado passed over Kansas City, Missouri, destroyed the court-house and other buildings, and partly demolished the bridge over the Missouri River, and thence pursued an easterly course to Pennsylvania, killing in its course about 90 persons, and injuring 300. On the night of the 12th terrible destruction was done by tornadoes in Ohio, Indiana, and Illinois. From 4 to 5 inches of rain fell within three hours, fully twenty miles of the Little Miami Railway were washed away, and at Xenia, where the waterspout was most destructive, 36 persons were drowned, 80 injured, and more than 100 houses destroyed. On the 14th another series of tornadoes carried destruction through Ohio and Indiana; and on the 15th a tornado, which appears to have formed on Lake Erie, penetrated 100 miles into Ohio. Its path was about 500 yards wide, and it levelled everything with the ground; killed 41 persons, and injured several hundreds. These, with other tornadoes of less severity occurring in Kansas and elsewhere, are stated to be the most severe and destructive storms or tornadoes hitherto experienced in the United States, and the losses are estimated at about 5,000,000 dollars.

The detailed reports of these remarkable storms will no doubt be prepared and circulated with the fulness and satisfactoriness which characterise the work and publications of the United States Signal Office; and, considering the striking simultaneousness of occurrence of these American and European storms from May 11 to 15, Gen. Hazen would, if the Reports were accompanied and illustrated by international weather maps of the northern hemisphere from May 7 to 16, lay meteorologists under a deep obligation.

CRETACEOUS METAMORPHIC ROCKS

IT is now a good many years since Prof. J. D. Whitney announced the existence of Cretaceous sediments in California which had undergone metamorphism into various crystalline rocks. The attention of geologists is once more directed to that region by the recent work of the Geological Survey of the United States. Mr. George F. Becker, whose admirable monograph on the great Comstock Lode has reflected such lustre on that Survey, has more recently been intrusted with the investigation of the quicksilver deposits of California. This research when completed will form the subject of another monograph in the same great series of publications. But in the meanwhile some results of such startling importance in metamorphism have been obtained that Mr. Becker has published in the *American Journal of Science* a brief preliminary outline of them.[1] It appears that the inquiry into the nature and origin of the rocks in question has been carried on partly by examination of their structure and relations in the field, partly by chemical analysis and partly by the study of microscopic sections. In short, all the appliances of modern geology have been enlisted in the investigation.

The area embraced by the rocks which are stated to have undergone metamorphism is estimated at 3000 square miles. The rocks are determined by fossils to be approximately of Neocomian age. They consist chiefly of sandstone and arkose lying upon and probably for the most part derived from granite. Their quartz-grains are cemented in great measure with carbonate of lime, and there are likewise clastic fragments of orthoclase, plagioclase, biotite, hornblende, and other minerals of the granite. From ordinary unaltered fossiliferous sandstones gradations are traceable into varieties wherein a process of recrystallisation has been set up, but has ceased before the fragmental character has been wholly effaced. In this process one of the first stages is often the resolution of the clastic grains into crystalline aggregates from which new minerals are built up. Thus the quartz-grains have had their surfaces so altered that an envelope of

twinned felspar microliths has formed round them. These bodies lie approximately normal to the surface of the residual kernel, which they actually penetrate like pins set in a cushion. Zoisite also, which is present in nearly all the altered sandstones, as a product of metamorphism, sometimes pierces the quartz from outside. Augite and hornblende have likewise been developed, not infrequently as microliths, which, though grouped together in one common crystallographic outline, are not united.

Further stages of change are described as occurring in certain rocks where the fragmental character, though to the unaided eye still traceable, is found to have been replaced by an entirely crystalline structure, giving rise to rocks which must be classed with the diabases. These masses sometimes have their pyroxene in the form of diallage, and are destitute of olivine, but usually contain much zoisite and frequently also hornblende. Diorites of similar origin occur, sometimes with a predominance of hornblende as in true amphibolites.

The shales are silicified and intersected by innumerable quartz-veins, in which, or projecting from their walls, are frequently abundant zoisite crystals. Yet the metamorphism has not destroyed the microscopic fossils contained in the strata. But the most remarkable example of metamorphism cited by Mr. Becker is the conversion of these same felspathic sandstones into serpentine—an alteration referred to in Prof. Whitney's description of this region. He asserts that field observation conclusively proves the great mass of the serpentine, estimated altogether at more than 1000 square miles, to have been made out of the sandstones, either immediately or through an intermediate granular rock. Sections are seen where sandstone shades off into serpentine, and areas of highly-inclined sandstones pass along the strike into the same rock. In this conversion, the change begins along the cracks, working toward the centres of the included fragments, and producing a structure like that seen in decomposing olivine. The felspar fragments are corroded externally, their cracks being irregularly widened and filled with serpentine which sometimes projects as teeth into the clear felspathic mass. Even the quartz-grains have not been able to resist the alteration, but may be seen with their outer parts replaced by serpentine, which likewise penetrates their interior in long slender green needles. Apatite has also been replaced by serpentine, and the same transformation may be surmised in the case of mica and garnet. Mr. Becker states that chemical analyses and microscopic tests demonstrated that the serpentine diffused through the sandstones and forming the massive exposures is all the same mineral. He will no doubt in due time produce the detailed evidence on which his statements are founded. In the meantime he will of course be prepared for much scepticism and even for angry denial of his results. The careful elucidation of the problem he has attacked cannot but be of enormous service in throwing light upon the vexed question of metamorphism. He claims that the rocks which he has been investigating furnish a colossal example of regional metamorphism of which all the successive stages can be studied. Many thousands of square miles of rock have been subjected to such intense lateral compression that they have been utterly shattered, the average size of the unbroken lumps not being greater than that of an egg. In rocks thus crushed warm interstitial water would have potent chemical reactions. Warm basic solutions are believed to have first been produced, and to have converted the sandstones and some of the shales into holocrystalline compounds containing augite and hornblende. Serpentisation is supposed to have followed at a lower temperature, while silicification came last, as the solutions finally became acid Geologists will await with impatience the appearance of the monograph in which these conclusions are maintained.

THE COMPOSITION OF THE EDIBLE BIRD'S-NEST (COLLOCALIA NIDIFICA)

THE nature of the material which forms the edible bird's-nest has been the subject of some controversy. In 1817 Sir Everard Horne (*Phil. Trans.*, 1817, p. 337) suggested that it is the product of the activity of certain glandular structures which he figures in his paper, and which he associates with the gastric glands. In the "Origin of Species" (6th ed., p. 228) Darwin indorses the view of its being entirely a secretion by the bird, speaking of it as "inspissated saliva," and showing how the amount of saliva devoted to nest construction differs with different species. He mentions in particular a North American species which he says "makes its nest (as I have seen) of sticks agglutinated with saliva and even with flakes of this substance." Writing at about the same time, Bernstein (*Journal fur Ornithologie*, 1859, p. 111) connects the nature of the material with certain developments of the bird's salivary glands, which he says are noticeable during the nest-building season.

On the other hand, it has been maintained by many observers that the nest of this species of swift is constructed similarly to the North American species referred to by Darwin, the chief difference being that instead of sticks the bird uses certain algæ which are found in considerable masses on the walls of the caves which they frequent in the breeding season. Other algæ also have been suggested as those used.

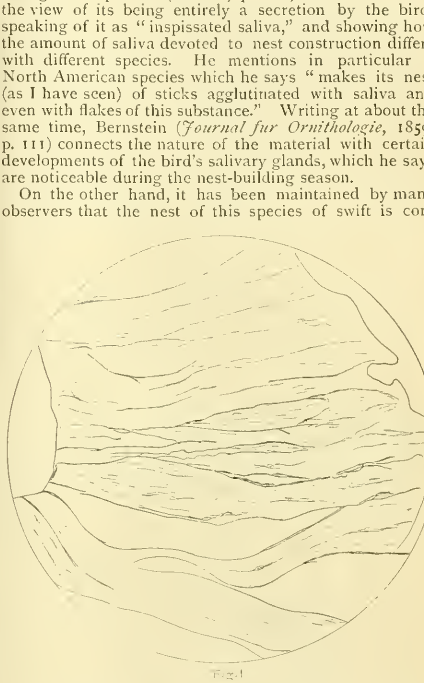


Fig. 1

Celebes nest. Lamellated structure (mag. 50 diam.).

It has further been supposed that the algæ are partially digested before being utilised, and that after regurgitation the material so acted on is worked up into the form of the nest.

A suggestion was made by Mr. E. L. Layard, H.M. Consul in New Caledonia, in a letter to NATURE, September 17, 1884 (p. 82), which seemed to reconcile the conflicting theories. He says that the first quality of nest which is produced early in the breeding season consists entirely of animal secretion, but that later on, if the first nests are destroyed, the birds cannot replace them by this secretion alone, and so use extraneous substances to help in the construction. The second and inferior qualities of nest are so formed differently from the first.

In a paper published in the *Journal of Physiology* (vol. vi. p. 40), I have given the results obtained from an

examination, both microscopic and chemical, of the nests used for soup at the Health Exhibition of 1884. Since then I have had, by the kindness of Mr. W. T. Thiselton Dyer, Director of the Royal Gardens, Kew, the opportunity of examining various specimens of the nests,

Fig. 2

Borneo nest. No alga present (mag. 200 diam.).

obtained from other places than the first-named, and differing from one another in quality.

The results of my experiments on these nests do not modify in any essential particular those which I had obtained before from the first ones examined. After prolonged soaking all alike became gelatinous in texture, and

Fig. 3

Celebes nest. Alga present (mag. 200 diam.).

then were easily seen to be made up of laminæ affixed by their faces to each other. Whether the observation was made by teasing with needles or by cutting sections, this laminated structure was very evident. Fig. 1 is a section of a nest from Celebes, enlarged about 50 diameters. The laminæ are fairly regular in disposition, and show no trace of any vegetable structure between them or in their substance. Here and there may be noted small granular bodies resembling epithelium cells. These are seen more distinctly in Fig. 2, which is a section of a nest of inferior quality from Borneo enlarged 200 diameters.

Another nest, also from Celebes, but marked as being of inferior quality showed the presence of algæ. A section of this, taken from the part where the greatest quantity was found, is shown at Fig. 3, the enlargement being again 200 diameters. The presence of the alga in this inferior nest seems at first to bear out Mr. Layard's suggestion, but an examination of the mode in which it is disposed in the nest-substance does not confirm his view of its being here even an agglutination of alga because the supply of saliva had failed. The alga-cells, though fairly numerous, are not in large quantity when compared with the amount of nest-substance, nor are they regularly placed in layers as would be the case if agglutinated as suggested. Their somewhat scanty amount and their irregular position would be better accounted for on the theory of their being accidental constituents. In many sections debris of one kind or another mixed with the secretion is not at all infrequent, small feathers being the most numerous. In the nest in question the alga was not present throughout, many sections showing none, others a little, the quantity varying very much. The amount found may perhaps be connected with the feeding of the birds, and result from debris of food remaining in the mouth, and so mixed up with the secretion next produced.

The nest-substance gave no micro-chemical reactions that could connect it at all with cellulose, so that it could not be formed by the partial digestion of the alga and regurgitation of the resulting matter. On the other hand, it did give very striking evidence of its close relationship with the body *mucin* described by various authors,[1] and well known as a product of the animal body. The reactions obtained with the first material used (that from the Health Exhibition) were confirmed completely by the experiments made upon the nests from Kew.

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NOTES

MR. ADAM SEDGWICK, M.A., Fellow and Lecturer of Trinity College, Cambridge, has been nominated by the President and Council of the Royal Society to be recommended for election by the Society on June 4, in place of the late T. R. Lewis, one of the selected candidates, who died soon after the selection was made.

SINCE our last week's note the eruption of Mount Etna has gone on increasing in violence. A correspondent of the *Standard* sends some valuable notes on the progress of the eruption. Under date Catania, May 19, 8.40 a.m., he writes :—"The eruption of Mount Etna assumed alarming proportions last evening at 5 o'clock. Earthquake shocks were felt in all the communes in the immediate vicinity of the volcano. At Zafferato, where the shocks appear to have been attended with upheaval of the soil, the disturbance is described as being so severe that the panic-stricken inhabitants fled from the neighbourhood. In the district of Bronte heavy showers of sand descended, and a gigantic column of thick black smoke was seen to emerge from the central crater of Etna towards the west." "10 a.m. An eruption has occurred near Nicolosi, to the north-west of Montegrosso, quite as severe in character as that of 1883. The lava has begun to stream down towards Nicolosi, accompanied by severe shocks of earthquake. 10.55 a.m. In addition to the

[1] Eichwald, "Ueber das Mucin besonders der Weinbergschnecke," *Annal. Chem. Pharm.*, cxxiv. 1865, pp. 177 to 211. Obolensky, "Ueber Mucin aus der Submaxillardrüse," Pflüger's *Archiv*, vol. iv. p. 336.

eruption which has taken place in Etna itself another one occurred about 2 a.m. in Prince of Naples Mount, situated about 9 kilometres north-east of Nicolosi. This is one of the 'red' mountains, formerly volcanic, but which has hitherto been regarded as exhausted. The lava is running in two streams towards the open country. Repeated and very severe shocks of earthquake are being felt in the vicinity of Etna. 11.55 a.m. A short time ago a severe shock of earthquake of an undulating kind was felt. The eruption of Prince of Naples Mount is increasing in severity. The lava is running rapidly in a double stream towards Nicolosi, to the great danger of the town. Panic amongst the inhabitants." "Nicolosi, May 19, noon. The eruption is assuming terrific proportions. The lava has advanced over 3 kilometres in 8 hours, continuing its course steadily towards Nicolosi. From the central crater, where the eruption is still very active, the lava is flowing towards Montegrosso." "Catania, May 19, 2 p.m. The eruption threatens the destruction of the western portion of the Etna forests. Last night several volcanic rents occurred between Nicolosi and Pedara. May 20. Eleven craters have been opened, of which three have assumed enormous proportions. The lava is advancing rapidly. In some places the stream is 200 metres broad. The central crater, however, emits only vapour and cinders." "Nicolosi, May 20. Three of the craters are raging fearfully, emitting huge stones to a considerable height, and the roar and tumult is terrible. The lava is advancing rapidly, but for the present it is uncertain what direction it may take, whether towards Nicolosi or Belpasso. Shocks of earthquake still continue, but they are less severe in character. The craters are situated behind Montezzano, in the valley between that place and Montenero. The people of the district assert that they can remember no eruption so sudden and alarming as this has been. The scene is indescribable. The streams of lava are in some places more than 200 metres in width. The central crater on the summit continues to vomit large columns of smoke; but from this, according to Prof. Silvestri, no danger is to be apprehended." "Acireale, May 20. Last evening, through the night, and this morning, many shocks of earthquake were felt at Piedimonte, Acisantangelo, Linguaglossa, and Acireale." "Catania, May 20. This morning, with a severe shock of earthquake, the volcanic orifice which was so active in 1883 was reopened." "Nicolosi, May 20. The activity of the central crater is increasing. Montegrosso is the principal site of eruptive force. Great alarm exists among the inhabitants, and the houses most likely to be in danger are being evacuated. The local authorities are on the spot." On Sunday the eruption had greatly diminished, but on Monday morning it broke forth with great violence, and a fresh crater sent out a stream of lava 150 metres wide and 23 deep towards Nicolosi. On Monday evening the news was very disquieting. The violence of the eruption was then greatly increasing, and Nicolosi seemed doomed to destruction. The noise at a considerable distance is described as resembling a continuous cannonade.

AT the meeting of the Paris Academy of Sciences on Monday, M. de Lesseps asked for the appointment of a committee to report on the alleged difference of sea-level on the two sides of the Isthmus of Panama. A similar objection to the Suez Canal had, he said, proved unfounded; and if the present objection were also disposed of, no locks would be necessary in the canal. On the motion of Admiral Jurien de la Gravière, who suggested that the tides might be higher on one side of the Isthmus than on the other, the question was referred to the navigation and astronomy sections. Meantime it is stated that the Technical Commission to whom M. de Lesseps had referred certain questions have affirmed unanimously that there is no insurmountable difficulty to the completion of the Canal according to the technical programme adopted by the Company—that is to say, that there will be no necessity to construct the Canal with locks, the level of the two oceans being the same.

THE committee for the celebration of the centenary of Arago will issue shortly an appeal for subscriptions to erect a statue to the celebrated astronomer on the Place St. Jacques. A new boulevard will be opened in the direction of the meridian for connecting Paris and Montsouris Observatories.

A COUNTY Scientific Society for Middlesex has been constituted, to which both ladies and gentlemen are eligible. The Right Hon. Viscount Enfield, Lord-Lieutenant of the county, has consented to be the President of the Society; and the Right Hon. the Earl of Crawford and Balcarrēs, the Right Hon. the Earl of Aberdeen, the Right Hon. the Marquis of Ripon, Prof. T. H. Huxley, Sir John Lubbock, Prof. W. H. Flower, Sir Frederick Abel, and Dr. Archibald Geikie having intimated to the Provisional Committee their approval of the scheme and their readiness to become Vice-Presidents of the Society, were at the meeting constituting the Society elected to this office. The following gentlemen were elected members of the Council of the Society, the list to be subsequently increased:—W. Lant Carpenter, Herbert Druce, J. N. Dunning, E. Fitch, G. Griffiths, R. B. Hayward, J. Logan Lobley, Rev. Dr. C. McDowall, Wm. Simpson, Rev. Dr. F. A. Walker, Rev. J. Crane Wharton, W. Mattieu Williams. It is intended that the Society shall hold monthly meetings (evening) from about October to May, and field-meetings during the summer months. Names of ladies or gentlemen desirous of joining may be sent to the Hon. Secretary, Mr. Sydney T. Klein, Clarence Lodge, Church Road, Willesden, N.W.

THE eleventh public annual meeting of the Sunday Society was held on May 22. Sir Henry E. Roscoe, M.P., F.R.S., took the chair as President for the year. Mr. Mark H. Judge read the annual statement, pointing out the progress that had been made during the year in accomplishing the objects which the Society has in view. Sir Henry Roscoe, in his Presidential Address, quoted copiously from the utterances of past Presidents and of eminent men in all departments on behalf of the opening of museums, art galleries, and similar institutions on Sunday. Having had the honour, Sir Henry said, to serve on the Royal Commission on Technical Instruction, he had had full opportunity of observing the effects of the Sunday opening of the numerous museums, industrial, artistic, and scientific, which exist in Continental countries, and he stated that after such observations the unanimous conclusion to which the Commissioners arrived was that the influence which the opening of these museums on Sunday exerted, not only upon the industrial progress of those countries, but upon the moral and intellectual condition of their peoples, had been in the highest degree satisfactory. And so forcible was the evidence on this head then brought before them, that the Commissioners placed a distinct recommendation at the close of their Report, to the effect that museums of art and science and technological collections in this country should be opened to the public on Sundays. They point out in their Report that, in respect to museums, the people of this country stand in a position of great disadvantage as compared with Continental nations, and that to the influence of these collections, as regards the direct bearing they have on art and industrial training, is due much of the abundance of art resource so advantageous to many Continental industries and manufactures. Almost all these Continental art galleries and museums are opened to the public freely on Sundays, and Sir Henry had yet to learn that opposition has been anywhere raised to this, or that any objectionable features have there been introduced. Sunday opening in the United Kingdom, so far as it has gone—and that is a long way—has had an effect the reverse of that

anticipated by its opponents: museums, galleries, and libraries have been open on Sundays in thirteen towns for some years, with such success that the only opposition in those towns is one which does not show itself in public. The town of Oldham is now trying the experiment of Sunday opening for four Sundays, to-morrow being the second Sunday; and Sir Henry had a letter from the Mayor of that important northern town, stating that on last Sunday crowds of well-behaved persons passed through the gallery, and that he is satisfied of the importance and good influence of the movement. Sir Henry concluded:—"We challenge the Sabbatarian party to be true to their own opinions, as we are to ours. We ask for more of what we say has been a blessing. They refuse their assent on the plea that Sunday opening *per se* is a curse. They show by their actions that they are only half believers in their own statements. We challenge them to take a vote on the direct question as to whether those museums and galleries now open should remain open or not." The meeting authorised the Committee of the Society to send a memorial to H.R.H. the Prince of Wales, requesting that in the interest of the community the Colonial and Indian Exhibition should be opened on a few Sundays by free tickets.

MR. EDGAR CROOKSHANK writes, with reference to our article on the Royal Society *soirée* last week, to say that by a slip the exhibition of micro-photographs and preparations of Bacteria were assigned to his friend Mr. Cheshire. The collection of photographs was exhibited by request, and represent many months' work in endeavouring to overcome the difficulties of obtaining satisfactory results with the use of high powers (1-25th, 1-18th, and 1-12th, o.i.). In many cases of preparations of Bacteria, such as *cover-glass preparations*, and especially the so-called *impression-preparations*, and particularly where there is much detail, Mr. Crookshank maintains that photography is the only satisfactory means for obtaining an accurate picture. In such cases photography excels in affording us absolute faithfulness.

THE hybrid trout reared at the South Kensington Aquarium from the ova of the sea-trout, *S. trutta*, impregnated with the milt of the common trout, *S. fario*, in December last, show unmistakable signs of hardihood. Comment has been made previously in this journal upon the subject, as the facts adduced evidence the capacity of Salmonidæ to give ova without descending to the sea. There has been a much less percentage of mortality not only amongst the ova but the fry. At the present time they are located in a special pond at the Delaford Park Fishery belonging to the National Fish Culture Association, where they continue to thrive exceedingly well. They were encouraged to feed after losing their sac sooner than other species of fry; and without exception are growing rapidly.

ON Saturday last 12,000 Severn salmon fry were turned into the River Dee. The ova were spawned by the Severn Fishery Board, and hatched out by the National Fish Culture Association. A large number of the same species will be deposited this week in the Severn, having been reared at South Kensington for this purpose.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*) from India, presented by Capt. Boyle; two Squirrel Monkeys (*Chrysothrix sciurea*) from Brazil, presented by Mr. George Liddell; a Pig-tailed Monkey (*Macacus nemestrinus*) from Java, presented by Mrs. F. E. A. Prince; three Speckled Terrapins (*Clemmys guttata*), seven Painted Terrapins (*Clemmys picta*), a Sculptured Terrapin (*Clemmys insculpta*), eleven Striped Snakes (*Tropidonotus sirtalis*), three Ribbon Snakes (*Tropidonotus saurita*), seven Dekays Snakes (*Ischnognathus dekayi*), four Grass Snakes (*Cyclophis vernalis*) from North America, presented by Mr. Samuel Garman, C.M.Z.S.; three Spanish Terrapins (*Clemmys leprosa*), European, presented by Mr. Cuthbert Johnson; a Quail (*Coturnix communis*), European, presented by Mr. Kenneth Lawson; four Menobranchs (*Menobranchus lateralis*) from North America, presented by Prof. Ramsay Wright; two Wild Ducks (*Anas boschas*), British, presented by Mr. G. Edison; a Common Viper (*Vipera berus*), British, presented by Mr. Percy E. Coombe; a Macaque Monkey (*Macacus cynomolgus*) from India, deposited; two Spotted Hyænas (*Hyæna crocuta*) from South Africa, two Ruffs (*Machetes pugnax*), three Viperine Snakes (*Tropidonotus viperinus*), European, two Lion Marmosets (*Midas rosalia*), a Spotted Cavy (*Cœlogenys paca*), two Ariel Toucans (*Ramphastos ariel*), two Crested Curassows (*Crax alector*), a Zenaida Dove (*Zenaida amabilis*) from Brazil, purchased; four Bernicle Geese (*Bernicla leucopsis*), European, received in exchange; a Gayal (*Bibos frontalis*), an African Wild Ass (*Equus tæniopus*), a Japanese Deer (*Cervus sika*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE HELIOMETER OF THE YALE COLLEGE OBSERVATORY.—From Dr. Elkin's report of the work done during the year ending June 1, 1885, we learn that the principal object of research has been the triangulation of the Pleiades, to which work the heliometer was devoted from September 1884 to March 1885. It was originally intended to confine the investigation to the stars measured at Königsberg and to carry out only one method of triangulation. The scheme has been extended, however, to include all the stars in the Bonn *Durchmusterung*, within certain limits, down to magnitude 9·2, making sixty-nine stars in all, and also to obtain a determination of the relative positions of the stars which should be strictly comparable with the Königsberg work, viz. measurement of distances and angles of position of the stars from Alcyone. The observations have all been reduced provisionally; the final reduction cannot be undertaken until the results of the meridian observations of the end stars of two zones serving to determine the scale value and zero of position have been received from the observatories which have consented to make them. Measures of the moon from neighbouring stars have also been made on thirty-six nights near the first and last quarters, and the diameter of the moon has been measured at opposition on seven occasions. Observations have also been made of the diameter of Venus, the outer ring of Saturn, and of Titan referred to its primary. It is now proposed to devote the heliometer to systematic investigations in stellar parallax, and, judging from the results which have been obtained by Gill and Elkin at the Cape, we may expect that most valuable work will be done in this direction with the heliometer at Yale College also.

ECLIPSE OF JUPITER'S FOURTH SATELLITE.—Mr. Marth pointed out some two years ago, in a paper read before the Royal Astronomical Society (*Monthly Notices*, vol. xliv. p. 241), the importance of observing those eclipses of the fourth satellite of Jupiter which commence or end a series, in order to obtain data for correcting the tables. A slight error in latitude shows itself very strikingly on such occasions in the duration of the eclipse. Such observations are unfortunately very rare, the Greenwich eclipse observations from 1836 to 1883 not affording a single instance of an observation which will assist in correcting the latitude. Mr. John Tebbutt, Windsor, New South Wales, noticing that two of the last eclipses of the cycle just closed were visible in New South Wales, drew attention to these facts in the *Sydney Morning Herald* for March 27. We learn from a communication in the number for April 1 of the same paper that Mr. Tebbutt was successful in his own observations of the phenomenon. The disappearance of the satellite took place at 14h. 38m. 34s. Windsor mean time, or 15m. 43s. *before* the time given in the *Nautical Almanac*. The reappearance, on the contrary, was 8m. 55s. *late*, the duration thus being 24½m. longer than the predicted time. Mr. Russell, at Sydney, noted that the satellite had already disappeared when he began to observe, quite six minutes before Mr. Tebbutt lost sight of it. Mr. Tebbutt was convinced, however, of the accuracy of his now observation.

A NEW COMET.—Mr. W. H. Brooks, Red House Observatory, Phelps, New York, discovered a bright comet on May 23, 15h. Greenwich mean time. Its position was as follows:—R.A. 11h. 55m., Decl. 8° 55′ N.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *MAY* 30—*JUNE* 5

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on May 30

Sun rises, 3h. 52m.; souths, 11h. 57m. 15·7s.; sets, 20h. 2m.; decl. on meridian, 21° 48′ N.: Sidereal Time at Sunset, 12h. 35m.

Moon (New on June 2) rises, 2h. 37m.; souths, 9h. 20m.; sets, 16h. 14m.; decl. on meridian, 7° 36′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	3 20	10 58	18 36	17 39 N.
Venus	2 22	9 5	15 48	7 43 N.
Mars	11 57	18 35	1 13*	6 44 N.
Jupiter	12 57	19 15	1 33*	2 50 N.
Saturn	5 50	14 1	22 12	22 46 N.

* Indicates that the setting is that of the following morning.

May	h.	
30	1	Venus in conjunction with and 1° 18′ north of the Moon.
June		
4	10	Saturn in conjunction with and 4° 1′ north of the Moon.

Variable Stars

Star	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·2	81 16 N.	May 30,	2 57	*m*
			June 3,	2 37	*m*
S Cancri	8 37·4	19 17 N.	May 31,	22 12	*m*
S Bootis	14 19·1	54 20 N.	June 2,		*M*
δ Libræ	14 54·9	8 4 S.	May 30,	1 16	*m*
U Coronæ	15 13·6	32 4 N.	,, 31,	3 34	*m*
R Herculis	16 1·1	18 41 N.	June 2,		*M*
U Ophiuchi	17 10·8	1 20 N.	May 31,	3 2	*m*
			and at intervals of	20 8	
X Sagittarii	17 40·4	27 47 S.	June 5,	2 20	*M*
W Sagittarii	17 57·8	29 35 S.	,, 2,	21 30	*m*
U Sagittarii	18 25·2	19 12 S.	,, 1,	21 40	*M*
β Lyræ	18 45·9	33 14 N.	,, 1,	2 25	*M*

M signifies maximum; *m* minimum.

Meteor Showers

The shortness of the nights at this season of the year greatly interferes with meteor observation, and no great periodical shower occurs at this time. Meteors from the following radiants amongst others have been observed. Near Cor Caroli, R.A. 206°, Decl. 39° N.; near ε Coronæ, R.A. 240°, Decl. 25° N.; near β Lyræ, R.A. 280°, Decl. 29° N.; near κ Cephei, R.A. 289°, Decl. 80° N.

Stars with Remarkable Spectra

Name of Star	R.A. 1886·0 h. m. s.	Decl. 1886·0 ° ′	Type of spectrum
407 Birmingham	17 14 2	2 15·4 N.	III.
D.M. + 17° 3241	17 20 49	17 1·1 N.	III.
Arg. Oeltzen 17681	18 1 7	21 16·3 S.	Bright lines
D.M. + 43° 2890	18 3 21	43 26·3 N.	III.
458 Birmingham	18 38 51	36 50·7 N.	IV.
464 Birmingham	18 43 43	8 2·0 S.	IV.
δ^2 Lyræ	18 50 30	36 45·2 N.	III.
R Lyræ	18 51 52	43 47·6 N.	III.
222*c* Schjellerup	18 58 18	5 51·2 S.	IV.

GEOGRAPHICAL NOTES

In his presidential address at the annual meeting of the Royal Geographical Society on Monday, the Marquis of Lorne, referring to the matter of geographical education, said that the interest excited in the subject by the Society's recent action has been so great, and the expectation that the Society will continue it by taking some positive steps towards encouraging improvements in the position of geography in schools and Universities is so general, that the Council have felt encouraged and indeed bound to carry the scheme further. The Educational Committee of the Society therefore made certain suggestions to the Council, which are now under consideration, and will probably be adopted. The principal of these suggestions relates to the appointment of a lecturer in geography to deliver courses where the Council may direct. In this matter the Council will take suitable steps to obtain the co-operation of the Universities of Oxford and Cambridge. In order still further to encourage the scientific study of geography at the Universities, the Committee suggest that a prize or travelling scholarship be given every alternate year to a student who has shown marked ability in geographical subjects, and who may desire to visit one of the less-known districts of Europe, or the Mediterranean or Black Sea shores, and any results be communicated to the Society. One or other of the annual grants which are at the Society's disposal might be devoted to this purpose. Another suggestion is aimed at reaching the intelligent middle and working classes through the medium of the University Extension Scheme. For this purpose a small annual grant is proposed. Another is that a medal be given by the Society to the student reported by the examiners to have done best in physical geography in the first part of the Natural Sciences Tripos (Honours Examination). And finally, in order that all classes of schools may be reached, it is proposed that prizes be offered for competence in geography to the students at the various training-colleges. "Here we reach the fountain-head of education, and if we can secure adequate attention to geography in the institutions which send forth yearly troops of teachers to our Board and elementary schools, the Society will have accomplished much. It is evident, then, that the Society has already accomplished a great deal. The mind of the public has been aroused and greatly enlightened on the subject; our best schools and Universities have expressed their willingness to co-operate as far as possible in carrying out improvements; and there can be little doubt that our proposed further action will bring results which the Council and all interested in geography have long desired."

In a paper contributed to the *Bulletin* of the Moscow Society of Naturalists (1885, No. 2) M. Smirnoff continues his most valuable delimitation of the vegetable zones of the Caucasus, which forms an introduction to a flora of the vascular plants of the region. He subdivides Transcaucasia into several zones, the central zone extending east to the meridian of Shemakha. This limit does not correspond to any orographical features, but separates from the remainder of Transcaucasia the region subject to the influence of the Caspian Sea. The littoral of the Black Sea in Transcaucasia is distinguished from the rest of the region by high winter temperatures. As far as lat. 44° N., and even at an altitude of 150 metres, the winter is as mild as in Provence or in Central Italy, only the setting in of warm weather in spring being a little later. But as soon as the chain is crossed we find a rapid decrease of the winter temperature, so that Yekaterinodar, on the northern slope of the chain, although only 20 miles further to the north and 1° of longitude more to the east than Novorossiysk, has an average temperature during December and January 4° lower. Baku and Lencoran have winters very much like those of the Venetian littoral, but Derbent shows a sudden decrease of 3°·5 of temperature in January, probably due to the influence of ice gathering in the northern part of the Caspian Sea, while Petrovsk, only 70 miles further north on the same coast, shows a further sudden decrease of temperature in January. The ranges of the monthly average temperatures of different places show a still greater difference of climate. Thus, on the Black Sea coast, south of lat. 44° N., and even at Kutais, the difference between the warmest and the coldest months does not exceed 18° to 19°; it is the same as at Trieste and Athens. But in Ciscaucasia it reaches 25°, and on the Caspian littoral it varies from 22°·4 at Baku to 29°·8 at Petrovsk. It is still greater on the Armenian plateau (30° to 35°); while in Central Transcaucasia it is generally less than 25°, and rapidly diminishes with the altitude of the place, reaching no more than 19°·5 at Shusha. These few data, together with a map of isotherms prepared by M. Smirnoff, give a broad general idea as to the climatic conditions of the Caucasus, and the consequent distribution of different regions of vegetation through the country.

TELESCOPIC OBJECTIVES AND MIRRORS: THEIR PREPARATION AND TESTING [1]

It would probably lend an additional interest to a technical subject such as I have to bring before you to-night, could I preface my description of the processes now employed in the

[1] Lecture given at the Royal Institution on Friday, April 2, 1886, by Mr. Howard Grubb, F.R.S., F.R.A.S.

construction of telescopic objectives by a short historical account of what has been attempted and achieved in the past, but time will not permit.

A very few words, however, on the history of glass manufacture are necessary.

As I pointed out last Saturday afternoon, Dollond's brilliant discovery, which afforded a means of achromatising objectives, rendered possible their construction of greater size and perfection than formerly, provided suitable material could be obtained. But the chromatic errors being removed, faults in the material hitherto masked by them were detected, and it was not until after many years that Guinand, a lowly but gifted Swiss peasant, succeeded in producing glass disks of a considerable size and free from these defects.

The secrets of his process have been handed down in his own family to M. Feil, of Paris (one of his descendants), and also through M. Bontemps, who for a time was associated with Guinand's son, and afterwards accepted an invitation from Messrs. Chance Bros. and Co., of Birmingham, to assist them in an endeavour to improve that branch of their manufacture. Only these two houses, so far as I am aware, have succeeded in manufacturing optical disks of large size.

Testing of Optical Glass.—Let me here say a few words respecting the testing of optical glass; I mean of the material of the glass, quite apart from the optician's work in forming it into an objective. When received from the glass manufacturer it is sometimes in this state, roughly polished on both sides, and sometimes in this, in which as you see there are small windows only, facets as they are called, polished on the edges. In case of lenses for telescopic objectives, it is always well to have them roughly polished on the sides, to avoid the chance of having to throw away a lens after much trouble and labour has been spent on it.

There are only three distinct points to be looked to in the testing of optical glass: (1) general clearness and freedom from air-bubbles, specks, pieces of "dead metal," &c.; (2) homogeneity; (3) annealing.

The first is the least important, and needs no instructions for detection of defects, any one can see these. The second is much more important, and much more difficult to test.

The best test for homogeneity is one somewhat equivalent to Foucault's test for figure of concave mirrors.

The disk of glass should be either ground and polished to form a convex lens, or if that be not convenient, it should be placed in juxtaposition with a convex lens of similar or larger size, and whose excellence has been established by previous experience.

The lens or disk is then placed opposite some small brilliant light, a small gas flame generally suffices, and at such a distance that a conjugate focus is formed at other side and at a convenient distance. When the exact position of this focus is found, the eye is placed as nearly as possible so that the image of flame is formed on the pupil. On looking at it with the eye in this position, the whole lens should appear to be "full of light"; but at the slightest movement to one side the light will disappear and the lens appear quite dark. If the eye be now passed slowly backwards and forwards between the position showing light and darkness, any irregularity of density will be most easily seen.

Of course, like everything else, some experience is necessary.

The rationale of this is very obvious. When the eye is placed exactly at the focus of a perfect lens, the image formed on the pupil is very small, and the slightest movement of the eye will cause the light to appear and disappear. If the eye be not at the focus, the pencil of light will be larger, and consequently it will require a much greater movement of the eye to cause the light to disappear. Now if any portion of the lens be of a different density to the general mass, that portion will have a longer or a shorter focus; consequently while the light flashes off the general area of the lens quickly, it still remains on the defective portions.

By imitating this arrangement and substituting a camera for the eye and forming the focus of a small point of light on the stop of the lens, I have succeeded in photographing veins in glass, and sometimes have found this useful as a record.

The third point—that of proper annealing—is easily tested by the polariscope.

For small disks the usual plan is to hold them between the eye and a polarising plane, such as a piece of glass blackened at back or a japanned surface, and look at them through the facets, using as an analyser a Nicol prism.

Larger sizes, which are polished on the surfaces, can be more easily examined. It is difficult to describe the appearances, but I will put a few disks into the lantern polariscope and endeavour to point out what amount of polarisation may safely be permitted in disks of glass to be used for objectives.

The composition of metallic mirrors of the present day differs very little from that used by Sir Isaac Newton. Many and different alloys have been suggested, some including silver or nickel or arsenic; but there is little doubt that the best alloy, taking all things into account, is made with 4 atoms of copper, and 1 of tin, which gives the following proportions by weight: copper, 252, tin, 117·8.

Calculation of Curves.—Having now obtained the proper material to work upon, the first thing necessary is to calculate the curves to give to the lenses, in order that the objective, when finished, may be of the required focus, and be properly corrected for the chromatic and spherical aberrations.

As this lecture is intended to deal principally with the technical details of the process, I do not intend to occupy your time for more than a few moments on this head, nor indeed is it at all necessary. In my lecture last Saturday I explained the principles of achromatism, and in many published works full and complete particulars are given as to the calculation of the curves—particulars which are sufficient, and more than sufficient, for the purpose.

Much has been discussed and written concerning the calculation of curves of objectives, and much care and thought has been bestowed by mathematicians on this subject, and, so far as the actual constructors are concerned, a certain amount of veil is thrown over this part of the undertaking, as if there were a secret involved, and as if each had discovered some wonderful formulæ by which he was enabled to calculate the curves much more accurately than others.

I am sorry to have to dispel this illusion. Practically the case stands thus. The calculation of the curves which satisfy the conditions of achromatism and desired focus is a most simple one, and can be performed by any one having a very slight algebraical knowledge in a few minutes, provided the refractive indices and dispersive power of the glass be known. Both Messrs. Chance and Feil supply these data quite sufficiently accurately for small size objectives. Speaking for myself, I am quite content to take the figures as given by these glass manufacturers for any disk up to 10 inches in diameter. If over that size, I grind and polish facets on the disk and measure the refractive and dispersive powers myself.

The calculations of the curves required to satisfy the conditions of spherical aberration are very troublesome, but fortunately these may be generally neglected.

Some years ago the Royal Society commissioned one of its members to draw up tables for the use of opticians, giving the curves required to satisfy the conditions of both corrections for all refractive and dispersive indices.

A considerable amount of labour was expended on this work, but in the end it was abandoned, for it was found that the calculation of these curves was founded on the supposition that all surfaces produced by the opticians were truly spherical; while the fact is, a truly spherical curve is the exception, not the rule. The slightest variation in the form or figure of the curve will produce an enormous variation in the correction for spherical aberration, and it was soon apparent that the final correction for spherical aberration must be left to the optician and not to the mathematician. *Object-glasses cannot be made on paper.* When I tell you that a sensible difference in correction for spherical aberration can be made by half an hour's polishing, corresponding probably to a difference in the first place of decimals in radii of the curves, you will see that it is practically not necessary to enter upon any calculation for spherical aberration. We know about what form gives an approximate correction; we adhere nearly to that, and the rest is done by figuring of the surface.

To illustrate what I mean. I would be quite willing to undertake to alter the curves of the crown or flint lens of any of my objectives by a very large quantity, increasing one and decreasing the other so as to still satisfy the conditions of achromatism, but introducing theoretically a large amount of positive or negative spherical aberration, and yet to make out of the altered lens an object-glass perfectly corrected for spherical aberration.

I am now speaking of ordinary sizes. For very large sizes it is usual to go more closely into the calculations; but I may

remark that it is sometimes possible to make a better objective by deviating from the curves which give a true correction for spherical aberration and correcting that aberration by figuring, rather than if the strictly theoretical curves were adhered to. So far, then, as any calculation is required, the ordinary formulæ given in the text-books may be considered amply sufficient.

Having now determined on the curves, we have to consider the various processes which the glass has to undergo from the time it is received in this form from the glass manufacturer to the time when it is turned out a finished objective.

The work divides itself into five distinct operations: (1) rough grinding; (2) fine grinding; (3) polishing; (4) centering; (5) figuring and testing.

(1) The rough grinding or approximate shaping of the glass is a very simple process. The glass is cemented on a holder, and is held against a revolving tool supplied with sand and water, and of a shape which will tend to abrade whatever portions are necessary to be removed to produce the required curves. These diagrams will illustrate the various operations.

(2) Fine grinding. The tools used for fine grinding are of this form, and are made of either brass or cast iron. I prefer cast iron, except for very small sizes. They are grooved on the face, in the manner suggested by the late Mr. A. Ross, in order to allow the grinding material to properly distribute itself.

If two spherical surfaces be rubbed together they will, as may be supposed, tend to keep spherical; for the spherical is the only curve which is the same radius every part of its surface. If fine dry abrading powder be used between, the same result will be obtained; but, if wet powder be used, the surface will no longer continue spherical, but will abrade away more on the centre and edge than in the zone between. It was to meet this difficulty that the late Mr. A. Ross devised the idea of the distributing grooves. The fine grinding process is the first of the series which calls for any skill on the part of the operator.

That the *modus operandi* of the grinding be the more easily understood, let me explain the principle of the process in a few words.

When two surfaces of unequal hardness are rubbed together with emery powder and water between the two, each little particle of the powder is at any given moment in either of these conditions: (*a*) embedded into the softer surface; (*b*) rolling between the two surfaces; (*c*) sliding between the two surfaces.

Those particles which become embedded in the softer material do no work in abrading it, and but little in abrading the harder. They generally consist of the finer particles, and are kept out of action by the coarser which are rolling or sliding between the surfaces. Further, those that are purely rolling do little or no work. The greater part of the work is performed by those particles which are facetted and which *slide* between the two surfaces.

As the grinder is always of a much softer material than the glass, there is much more friction between the grinder and these particles than between the glass and the same particles, and therefore they partially adhere to the grinder and are carried by it across the face of the glass. This being so, it is now easy to perceive what the best conditions for rapid grinding are. Not too little emery, for then there will not be enough of abrading particles; not too much, for then the particles will roll on each other and tend to crush and disintegrate each other instead of abrading the glass, but just sufficient to form a single layer of particles between the grinder and the glass surface.

In the grinding of the small lenses, I mean up to 5 or 6 inches diameter, it is usual to carry out the entire grinding processes by hand; above that size by machinery. Surfaces up to 12 or even 15 inches can be ground by hand; but the labour becomes severe, and for my part I am gradually reducing the size for which the hand grinding is used, as I find the machine work more constant in its effects.

The machinery used is the same as that employed for the polishing operation, and I shall describe it under that head further on.

In the fine grinding operation by hand, the glass is usually cemented on to a holder of this form, having (for smaller sizes) three pieces of cork, to which the lens is attached, and this holder being screwed to a spud or nose on top of a post screwed to the floor. The operator, having applied the proper quantity of moist emery powder between the grinder and the glass, proceeds to work the former over the latter in a set of peculiar strokes, the amplitude and character of which he varies according to circumstances, at the same time that he changes his position round the post every few seconds. . . .

Although, as I have shown, the harder material is abraded very much more than the softer, yet the softer (the grinder) suffers considerable abrasion as well as the glass, and the skill of the operator is shown by the facility with which he is able to bring the glass to the curve of the grinder without altering the curve or figure of the latter.

It is even possible for a skilled operator to take a lens of one curve and a grinder of, say, a deeper curve, and by manipulation to produce a pair of surfaces fitting together, and of shallower curves than either.

Measurement of the Curves.—In the early stages of grinding, gauges of the proper radius, cut out of sheet brass or sheet steel, are used for roughly testing the curves of the lenses; but when we get to the finer grinding process it is necessary to have something much more accurate.

For this purpose a spherometer is used. It is made in various forms, generally with three legs terminating in three hardened steel points, which lie on the glass, and a central screw with fine thread, the point of which can be brought down to bear on the centre of the glass. In this way the versed sine of the curve for a chord equal to diameter of circle formed by these points is measured, and the radius of curve can be easily calculated from this.

I do not find the points satisfactory for regular work. They are apt to get injured or worn, and for ground surfaces are a little uncertain, as one or other of the feet may find its way into a deep pit. This particular spherometer has three feet, of about half an inch long, which are hardened steel knife-edges forming three portions of an entire circle. In using this it is laid on the surface to be measured, and the screw with micrometer head is turned till the point is felt to touch the surface of glass. This scale and head can then be read off. The screw in this instrument has fifty threads to the inch, and the head is divided into 100 parts, so that each division is equal to $\frac{1}{5000}$ of an inch. With a little practice it is easy to get determinate measures to $\frac{1}{10}$ of this, or $\frac{1}{50000}$ of an inch, and by adopting special precautions even more delicate measures can be taken, as far probably as $\frac{1}{100000}$ or $\frac{1}{150000}$ of an inch, which I have found to be practically the limit of accuracy of mechanical contact.

To give an idea of the delicacy of the instrument, I bring the screw firstly into contact with the glass. Now the screw is in good contact; but there is so much weight still on the three feet, that, if I attempt to turn it round, the friction on the feet oppose me, and it will not stir except I apply such force as will cause the whole instrument to slide bodily on the glass. Now, however, I raise the whole instrument, taking care that my hands touch none of the metal-work, and that the screw be not disturbed. I lay my hands for a moment on part of the glass where centre screw stood, and thus raise its temperature slightly, and on laying the spherometer back in the same place, you now see that it spins on the centre screw, showing how easily it detects what to it is a large lump, caused by expansion of the glass from the momentary contact of my hand.

Flexure.—One of the greatest difficulties to be contended with in the polishing of large lenses is that of flexure during the process.

It may appear strange that in disks of glass of such considerable thickness as are used for objectives, any such difficulty should occur; but a simple experiment will demonstrate the ease with which such pieces of glass can be bent, even under such slight strain as their own weight.

We again take our spherometer and set it upon a polished surface of a disk of glass of about $7\frac{1}{2}$ inches diameter and $\frac{3}{4}$ inch thick. I set the micrometer head as in the former experiment to bear on the glass, but not sufficiently tight to allow the instrument to spin round. This has now been done while the glass, as you see, is supported on three blocks near its periphery. I now place one block under the centre of disk and remove the others thus, and you see the instrument now spins round on centre screw.

It is thus evident that not only is this strong plate of glass bending under its own weight, but it is bending a quantity easily measurable by this instrument, which, as I shall presently show, is quite too coarse to measure such quantities as we have to deal with in figuring objectives.

After this experiment no surprise will be felt when I say that it is necessary to take very special precautions in the supporting of disks during the process of polishing, to prevent danger of

flexure; of course if the disks are polished while in a state of flexure, the resulting surface will not be true when the cause of flexure is removed.

For small-sized lenses no very special precautions are necessary, but for all sizes over 4 inches in diameter I use the equilibrated levers devised by my father, and utilised for the first time on a large scale in supporting the 6-foot mirror of Lord Rosse's telescope. These have been elsewhere frequently described, but I have a small set here as an example.

I have also sometimes polished lenses while floating on mercury. This gives a very beautiful support, but it is not so convenient, as it is difficult to keep the disk sufficiently steady while the polishing operation is in progress without introducing other chances of strain.

So far I have spoken of strain or flexure during the process of working the surface; but even if the surface be finished absolutely perfectly, it is evident from the experiment I showed you that very large lenses when placed in their cells must suffer considerable flexure from their own weight alone, as they cannot then be supported anywhere except round the edge.

To meet this I proposed many years ago to have the means of hermetically sealing the tube, and introducing air at slight pressure to form an elastic support for the objective, the pressure to be regulated by an automatic arrangement according to the altitude. My attention was directed to this matter very pointedly a few years ago from being obliged to use for the Vienna 27-inch objective a crown lens which was, according to ordinary rules, much too thin.

I had waited some years for this disk, and none thicker could be obtained at the time. This disk was very pure and homogeneous, but so thin that, if offered to me in the first instance, I would certainly have rejected it. Great care was taken to avoid flexure in the working, but, to my great surprise, I found no difficulty whatever with it in this respect. This led me to investigate the matter, with the following curious results.

If we call f the flexure for any given thickness t, and f' the flexure for any other thickness t', then $\frac{f}{f'} = \frac{t'^2}{t^2}$ for any given load or weight approximately. But as the weight increases directly as the thickness, the flexure of the disks due to their own weight, which is what we want to know, may be expressed as $\frac{f}{f'} = \frac{t'}{t}$.

Let us now consider the effect of this flexure on the image. In any lens bent by its own weight, whatever part of its surface is made more or less convex or concave by the bending has a corresponding part bent in the opposite direction on the other surface, which tends to correct the error produced by the first surface. This is one reason why reflectors which have not this second correcting surface are so much more liable to show strain than refractors. If the lens were infinitely thin, moderate flexure would have no effect on the image. The effect increases directly as the thickness. If then the flexure, as I have shown, decreases directly as the thickness, and the effect of that flexure increases directly as the thickness, it is clear that the effect of flexure of any lens due to its own weight will be the same for all thicknesses; in other words no advantage is gained by additional thickness.

This has reference, of course, only to flexure of the lens in its cell after it has been duly perfected, and has nothing to do with the extra difficulty of supporting a thin lens during the grinding and polishing processes.

Polishing.—The polishing process can be, and is often, conducted precisely in the same manner as the grinding, except that the abrading powders used (oxide of iron, rouge, an oxide of tin, putty-powder) is of a finer and softer description, and the surface of the polishing tool is made of a softer material than the metallic grinder.

Very nearly all my polishing is done on the machine I shall presently describe; but before doing so, I will, with your permission, say a few words on the general principles of the polishing process. Various substances are used for the face of the polisher—fine cloth, satin or paper, and pitch. Pitch possesses two important qualities which render it peculiarly suitable for this work, and it is a curious fact that we owe its application for this purpose to the extraordinary perspicuity of Sir Isaac Newton, who we may fairly say was the first to produce an optically perfect surface, and that that material is not only still used for the purpose, but is, as far as I know, the only substance which possesses the peculiar qualifications necessary to fulfil the required conditions. With skill and care, moderately good surfaces can be obtained from cloth polishers; but it is easy to see why they can never be perfect. There is a certain amount of elasticity in cloth and in its "nap," and there is consequently a tendency to round off the surfaces of the pits left by the grinding powder, and to polish the bottom or floor of these pits at the same time as the upper surface. It is easy to show mathematically that any process which abrades the floors of the pits at the same time as general surfaces even in a very much less degree, can never produce more than an approximation to a perfect surface, and practice agrees with the theory. Paper is said to be much used by the French opticians. I can say nothing about it. I have tried it and failed to produce a perfect surface with it, nor indeed should I expect it. It is of course open to the same objection as cloth. Pitch possesses, as I said, two most important qualities which render it suitable for the work; the first, in its almost perfect inelasticity; the second, a curious quality of subsidence, which we utilise in the process.

If we watch with a microscope, or even a magnifier, the character of two surfaces during the process of polishing, the one with cloth, and the other with pitch, the difference is very striking. With the cloth polisher, the polish appears much quicker, and it would at first sight appear as if the same polishing powder abraded more quickly on the cloth than on the pitch polisher, but I do not believe that such is the case, for if we look at the surface with a magnifier we shall find that, while all the surface has assumed a polished appearance, the surface itself has retained some of the form of the original pitted character with the edges rounded off; while in the pitch half-polished surfaces the floors of the pits are as gray as ever, and the edges are sharp and decisive. In pitch polishing, too, a decided amount of polish appears very quickly, and then for many hours there appears to be little or no further effect. Suddenly, however, the remaining grayness disappears, and the surface is polished. The reason of this is very obvious. The polisher being very inelastic, polishes first only the tops of the hills, and has to abrade away all the material of which these hills are composed before it reaches the valleys or floors of the pits. When it does reach them, the proper polish quickly appears. The second quality of pitch, that of subsidence, is also most valuable.

Pitch can be rendered very hard by continued boiling. By pitch I mean the natural bituminous deposit which comes to us from Archangel, not gas-tar pitch. It can be made so hard that it is impossible to make any impression on it with the finger-nail without splitting it into pieces; and yet even in this hard condition, if laid on an uneven surface it will in a few days, weeks, or months subside and take the form of whatever it is resting upon. The cohesion of its particles is not sufficient to enable it to retain its form under the action of gravity; and as this condition is that which science tells us marks the difference between solids and liquids, we must, paradoxical though it may appear, class even the hardest pitch among liquid instead of solid substances.

Now how do we utilise this peculiar quality?

The polishing tool is made by overlaying a metal or wooden disk formed to nearly the required curves by a set of squares of pitch, and while these are still warm pressing them against the glass, the form of which they immediately take.

In the grinding process I showed you that the regulation of the abrasion was managed partly by the character of the stroke given, and partly by the local touches given to the tool by the stoning process.

In polishing we still retain the same facilities for modifying the stroke, and the same rules I gave apply generally to the polishing process as well as the grinding; but we have not got any process equivalent to that of the local stoning, and even if we had it would be useless, for this very quality of subsidence of the pitch would in a few minutes cause any part of its surface which had been reduced to come into good contact again; we must therefore look for some other means for producing more or less abrasion whenever we require it. This we effect by modifying the size of the squares of pitch in the various zones. Practically it is done in this way by a knife and mallet. Whenever the squares are reduced, the abrasion will be less.

This is a well-known method of regulation; but the rationale is, I think, not generally understood. It is generally explained that there is less abrasion because there is less abrading surface. I do not think this is the true, or at least the entire, explanation. In order to understand the action, you must conceive the pitch to be constantly in a state of subsidence, the amount of that

subsidence depending of course on the pressure placed upon it. Now, if we reduce the size of the squares in any zone while retaining the same distance from centre to centre of squares, we increase at first the pressure per unit of area on the pitch squares in that zone, and consequently the subsidence will be greater, and the action will not be so tight or severe on that zone.

I know of no substance but pitch and a few of the resins which possesses this peculiar quality except perhaps ice, and it is curious to think that the same quality which in ice allows of that gradual creeping and subsidence and consequent formation of glaciers with their characteristic moraines, &c., will in pitch help us to produce accurate optical surfaces.

Polishing-Machines.—The two best-known polishing-machines are those of the late Earl of Rosse and the late Mr. Lassell, the general forms of which were shown in these diagrams. Time will not permit me to enter into a minute description of their working, nor is it necessary, as both have been often described.

A few words, however, as to the different character of the strokes given by these machines may be interesting. The stroke of Lord Rosse's machine may be imitated in hand-work by the operator traversing the polisher or mirror in a series of nearly straight strokes, of about one-third the diameter of the glass, to and from himself, at the same time that he keeps walking slowly round the post, and instead of allowing the centre of polisher to pass directly over the centre of mirror, each stroke that he gives he slides a little (about one-tenth diameter) to one side and then a little to the other.

Mr. Lassell's stroke may be imitated by causing the polisher to describe a series of nearly circular strokes a little out of the centre, walking round the post at the same time; thus the centre of polisher will describe a series of epicycloidal or hypocycloidal curves on the speculum.

Many years ago my father devised a machine, figured and described in Nichols's "Physical Science," by which either of these motions could be obtained. He appeared to have got better results with Mr. Lassell's strokes, for he afterwards devised a machine which gave the same character of stroke, but over which the operator had greater control, and this machine has been used for many years with great success. Like all machines, however, which give a series of strokes constantly recurring of the same amplitude, it is apt to polish in rings It is impossible to obtain absolute homogeneity in the pitch patches, and if any one square be a shade harder than the general number, and that square ends its journey at each stroke at the same distance from the centre of speculum or glass, there will almost surely be a change of curvature in that zone. To avoid this I have made a slight modification in the machine, which has increased its efficiency to a great extent. I will now place in the lantern a model of this machine, and first draw you a few curves with the machine in its old state, and afterwards in its improved state.

In order to convey some idea of the relative quantities of material removed by the various processes, I have placed upon the walls a diagram which will illustrate this point in two distinct ways.

The diagram itself represents a section of a lens of about 8 inches aperture and 1 inch thick, magnified 100 times, and shows the relative thickness of material abraded by the four processes.

The quantity removed by the rough grinding process is represented on this diagram by a band 25 inches wide, the fine grinding by one $\frac{8}{10}$ inch wide, the polishing by a line $\frac{1}{50}$ inch wide, while the quantity removed by the figuring process cannot be shown even on this scale, as it would be represented by a line only $\frac{1}{100000}$ inch thick.

I have also marked on this diagram the approximate cost of abrasion of a gramme of material by each of the four processes, viz.:—

		£	s.	d.	
Rough grinding,	about	0	0	1	per gramme.
Fine grinding,	,,	0	0	$7\frac{1}{2}$	,,
Polishing,	,,	0	10	0	,,
Figuring,	,,	48	0	0	,,

Figuring and Testing.—By the figuring process I mean the process of correcting local errors in the surfaces, and the bringing of the surfaces to that form, whatever it may be, which will cause the rays falling on any part to be refracted in the right direction. When an objective has undergone all the processes I have described, and many more which are not so important, and with which I have not had time to deal, and when the objective is centred and placed in its cell, it is, to look at, as perfect as it will ever be, but to look through and use as an objective it may be useless. The fact is that when an objective has gone through all the processes described, and is in appearance a finished instrument, I look upon it as about one-fourth finished. Three-fourths of the work has probably to be done yet. True, sometimes this is by no means the case, and I have had instances of objectives which were perfect on the first trial; but this is, I am sorry to say, the exception and not the rule.

This part of the process naturally divides itself into two distinct heads:—

(1) The detection and localisation of faults—what may, in fact, be termed the diagnosis of the objective.

(2) The altering of the figures of the different surfaces to cure these faults. This may be called the remedial part.

It may be well here to try to convey some idea of the quantities we have to deal with, otherwise I may be misunderstood in talking of great and small errors.

I have before mentioned that it is possible to measure with the spherometer quantities not exceeding $\frac{1}{50000}$ of an inch, or with special precaution much less even than that; but useful as this instrument is for giving us information as to the general curves of the surface, it is utterly useless in the figuring process; that is, an error which would be beyond the power of the spherometer to detect, would make all the difference between a good and a bad objective.

Take actual numbers and this will be evident. Take the case of a 27-inch objective of 34 feet focus; say there is an error in centre of one surface of about 6 inches diameter, which causes the focus of that part to be $\frac{1}{10}$ of an inch shorter than the rest.

For simplicity's sake, say that its surface is generally flat; the centre 6 inches of the surface therefore, instead of being flat, must be convex and of over 1,000,000 inches radius. The versed sine of this curve, as measured by spherometer, would be only about $\frac{1}{250000}$, 4 millionths of an inch, a quantity mechanically unmeasurable, in my opinion.

If that error was spread over 3 inches only instead of 6 inches, the versed sine would only be about $\frac{1}{1000000}$. Probably the effect on the image of this 3-inch portion of $\frac{1}{10}$ inch shorter focus would not be appreciable on account of the slight vergency of the rays, but a similar error near edge of objective certainly would be appreciable. Until, therefore, some means be devised of measuring with certainty quantities of 1 millionth of an inch and less, it is useless to hope for any help from mechanical measurement in this part of the process.

If, then, no known mechanical arrangement be delicate enough to measure these quantities, how, it may be asked, are these errors detected?

The answer to this is, that certain optical arrangements enable us to carry our investigations far beyond the limits of mechanical accuracy. Trials of the objective or mirror as a telescope are really the crucial test, but there are various devices by which defects are detected and localised.

The best object to employ is generally a star of the third or fourth magnitude, when such is available, but as it frequently occurs that no such object is visible, recourse is had to artificial objects. The minute image of the sun reflected from little polished balls of speculum metal, or even a thermometer bulb is a very good object; polished balls of black glass are also used with good effect; but as the sun also is of somewhat fickle disposition in this country, we have frequently to have recourse to artificial light. Small electric lamps, such as this, with their light condensed and thrown on a polished ball are very useful. In fact, I am never without one of them in working order.

For the detection and localisation of errors it is very useful to be provided with sets of diaphragms which leave exposed various zones of the surface, the foci of which can then be separately measured, but a really experienced eye does not need them.

For concave surfaces, Foucault's test is useful. I shall not trespass on your time to explain this in detail, as it is described very fully in many works, in none better than in Dr. Draper's account of the working of his own reflecting telescope. This diagram will give an idea of the principle of the system, which is really the same as what I have described as useful for detecting want of homogeneity in the substance of the glass.

This system is extremely useful for concave spherical surfaces, but is not available for convex surfaces, and only partially available for concave parabolic surfaces.

The really crucial test is, as I said before, the performance of

the objective when used as a telescope; and the appearance of the image not only at the focus, but on each side of it, conveys to the practised eye all the information required for the detection of the errors.

If an objective have but one single fault, its detection is easy; but it generally happens that there are many faults superposed, so to speak. There may be faults of achromatism, and faults of figure in one or all the surfaces; faults of adjustment, and perhaps want of symmetry from some strain or flexure; and the skill of the artist is often severely taxed to distinguish one fault from the rest and localise it properly, particularly if, as is generally the case, there be also disturbances in the atmosphere itself, which mask the faults in the objective, and permit of their detection only by long and weary watching for favourable moments of observation.

It would be impossible in one or a dozen of such lectures as this to enumerate all the various devices that are practised for the localisation of errors, but a few may be mentioned, some of which have never before been made public.

For detection of faults of symmetry, it is usual to revolve one lens on another and watch the image. In this way it can generally be ascertained whether it is in the flint or crown lens.

With some kinds of glass the curves necessary for satisfying the conditions of achromatism and spherical aberration are such that the crown becomes an equi-convex and the flint a nearly plano-concave of same radius on inside curve as either side of the crown. This form is a most convenient one for the localisation of surface errors in this manner.

The lenses are first placed in juxtaposition and tested. Certain faults of figure are detected. Now calling the surfaces A B C D in the order in which the rays pass through them, place them again together with Canada balsam or castor-oil between the surfaces B and C, forming what is called a cemented objective. If the fault be in either A or D surface, no improvement is seen; if in B or C, the fault will be much reduced or modified. Now reverse the crown lens, cementing surfaces A and C together. If same fault still shows, it must be in either B or D. If it does not show, it will be in either A or C. From these two experiments the fault can be localised.

It often happens that a slight error is suspected, but its amount is so slight that it appears problematical whether an alteration would really improve matters or not. Or the observer may not be able to make up his mind as to the exact position of the zone he suspects to be too high or too low, and he fears to go to work and perhaps do harm to an objective on which he has spent months of labour, and which is almost perfect. In many such cases I have wished for some means by which I could temporarily alter the surface and see it so altered before actually proceeding to abrade and perhaps spoil it.

During my trials with the great objective of Vienna, I thought of a very simple expedient, which effects this without any chance at all of injuring the surface. If I suspect a certain zone of an objective is too low, and that the surface might be improved by lowering the rest of it, I simply pass my hand, which is always warmer than the glass, some six or eight times round that particular zone. The effect of this in raising the surface is immediately apparent, and is generally too much at first, but the observer at the eye end can then quietly watch the image as the effect goes off, and very often most useful information is thus obtained. The reverse operation, that of lowering any required part of the surface, is equally simple. I take a bottle of sulphuric ether and a camel's-hair brush, and pass the brush two or three times round the part to be lowered, blowing on it slightly at the same time; the effect is immediately perceived, and can always be overdone if required.

So far then for the diagnosis. Now for the remedy. When the fault has been localised, the lens is again put upon the machine and the polisher applied as before, the stroke of the machine and the size of the pitch patches being so arranged as to produce, or tend to produce, a slightly greater action on those parts that have been found to be too high (as before described while treating of the polishing processes).

The regulation of the stroke, excentricity, speed, and general action of the machine, as well as the size and proportion of the pitch squares, and the duration of the period during which the action is to be continued, are all matters the correct determination of which depends upon the skill and experience of the operator, and concerning which it would be impossible to formulate any very definite rules. All thanks are due to the late Lord Rosse and Mr. Lassell, and also to Dr. de la Rue, for having published all particulars of the process which they found capable of communication; but it is a notable fact that, as far as it is possible to ascertain, every one who has succeeded in this line has done so, not by following written or communicated instructions, but by striking out a new line for himself; and I think I am correct in saying that there is hardly to be found any case of a person attaining notable success in the art of figuring optical surfaces by rigidly following directions or instructions given or bequeathed by others.

There is one process of figuring which is said to be used with success among Continental workers. I refer to the method called the process of local touch. In this process those parts, and those parts only, which are found to be high, are acted upon by a small polisher.

This action is of course much more severe; and if only it were possible to know exactly what was required, it ought to be much quicker; but I have found it a very dangerous process. I have sometimes succeeded in removing a large lump or ring in this way (by large I mean 3 or 4 millionths of an inch), but I have also and much oftener succeeded in spoiling a surface by its use. I look upon the method of local touch as useful in removing gross quantities, but for the final perfecting of the surface I would not think of employing it.

In small-sized objectives the remedial process is the most troublesome, but in large-sized objectives the diagnosis becomes much the more difficult, partly on account of the rare occurrence of a sufficiently steady atmosphere. In working at the Vienna objective it often happened when the figure was nearly perfect that it was dangerous to carry on the polishing process for more than ten minutes between each trial, and we had then sometimes a week to wait before the atmosphere was steady enough to allow of an observation sufficiently critical to determine whether that ten minutes' working had done harm or good. It must not be supposed either that the process is one in which improvement follows improvement step by step till all is finished. On the contrary, sometimes everything goes well for two or three weeks, and then from some unknown cause, a hard patch of pitch perhaps or sudden change of temperature, everything goes wrong. At each step, instead of improvement there is disimprovement, and in a few days the work of weeks or months perhaps is all undone. Truly any one who attempts to figure an objective requires to have the gift of patience highly developed.

In view of the extraordinary difficulty in the diagnostic part of the process with large objectives, it is my intention to make provision which I hope may reduce the trouble in the working of the new 28-inch objective for the Royal Observatory, Greenwich.

Two of the greatest difficulties we have to contend with are: (1) the want of homogeneity in the atmosphere, through which we have to look in trials of the objective, due to varying hygrometric and thermometric states of various portions; and (2) sudden changes of temperature in the polishing-room. The polisher must always be made of a hardness corresponding to the existing temperature. It takes about a day to form a polisher of large size, and if before the next day the temperature changes 10° or 15°, as it often does, that polisher is useless, and a new one has to be made, and perhaps before it is completed another change of temperature occurs. To grapple with these two difficulties I propose to have the polishing-chamber under ground, and, leading from it, a long tunnel formed of highly glazed sewer-pipes about 350 feet long, at the end of which is placed an artificial star illuminated by electric light; on the other side of the polishing-chamber is a shorter tunnel, forming the tube of the telescope, terminating in a small chamber for eye-pieces and observer. About half-way in the long tunnel there will be a branch pipe connected to the air-shaft of the fan, which is used regularly for blowing the blacksmith's fire, and through this, when desired, a current of air can be sent to "wash it out" and mix up all currents of varying temperature and density. It may be found necessary even to keep this going during observations.

By this arrangement I hope to be able to have trials whenever required, instead of having to wait hours and days for a favourable moment.

Figuring of Plane Mirrors.—There is a general idea that the working of a plane mirror or one of very long radius is a more difficult operation than those of more ordinary radii. This is not exactly the case. There is no greater difficulty in figuring a low curve than a deep one, but the difficulty in the case of absolutely plane mirrors consists simply in the fact that in their figuring there

is one additional condition to be fulfilled, viz. that the general radius of curvature must be made accurate within very narrow limits. In figuring a plane mirror to use, for instance, in front of even a small objective, say 4-inch aperture, an error in radius which would cause a difference of focus of $\frac{1}{100}$ of an inch would seriously injure the performance. This would be about equivalent to saying that the radius of curvature of the mirror was about 8 miles, the versed sine of which, with the 6-inch spherometer, would be about $\frac{1}{50000}$ of an inch. Now what I mean to convey is this: that it would be just as difficult to figure a convex or concave lens of moderate curvature as a flat lens of the same size if it were necessary to keep the radius accurate to that same limit, *i.e.* one-tenth of a division of this spherometer.

Lick Observatory.—For the final testing of large objectives or mirrors, it is necessary to have them properly mounted, and in such a manner that they can be directed conveniently on any celestial object, and kept so directed by clockwork, to enable the observer to devote his whole attention to the testing. I had not intended touching at all on the subject of the mounting of telescopes, but I have been asked to call your attention to this model of a proposed observatory for Mount Hamilton, California, as it embraces some novel features, but I shall do so in only a very few words.

Most here are probably aware that a monster observatory is in course of erection in California, a large sum of money having been left for the purpose by a Mr. Lick. The observatory is already partly complete, and contains some excellent instruments of moderate size, the work already done with which warrants us to hope that the great 36-inch refractive about to be erected will be placed under more favourable conditions for work than any other large telescope in the world.

The 36-inch objective is at present in process of construction by the Messrs. Clark of America, but the mounting has not yet been contracted for.

Some years since, in a paper published in the *Transactions* of the Royal Dublin Society, I shadowed forth a principle which I considered should be adopted in great telescopes of the future. The trustees of the Lick Observatory having invited me to design an instrument for the 36-inch objective, I have put into practical form what I had before given but general principles of, and the design which this model illustrates is the result.

Whether this design will ever be carried out or not I cannot tell, but even as a proposal I trust it may be interesting enough to excuse my introducing it (somewhat irrelevantly perhaps) to your notice to-night.

The design includes the equatorial itself, with its observatory, dome, and provision for enabling the observer to reach the eye end conveniently.

The conditions I laid down for myself in designing this observatory were that it would be possible for the observer single-handed to enter the equatorial room at any time, and that, without using more physical exertion than is necessary for working the smallest-sized telescope, or even a table-microscope, he should be able to open the 70-foot dome, turn it round backwards and forwards, point the equatorial to any part of the heavens, revolving it in right ascension and declination to any extent, and finally (the most difficult of all) to bring his own person into a convenient position for observing. I say this last is the most difficult of all, for I think any who have worked with larger instruments will allow that there is generally far more trouble in moving the observatory chair (so called) and placing it in proper position than in pointing the instrument itself. In this instrument the "chair" would require to be 25 feet high, and with its movable platform, ladder, balance-weight, &c., would weigh probably some tons. Even if very perfect arrangements were made for the working of this chair, the mere fact that the observer, while attempting to make the most delicate observations, is perched upon a small and very unprotected platform 25 feet above the floor, and in perfect darkness, tends to reduce his value as an observer to an extent only to be appreciated by those who have tried it.

No matter how enthusiastic a man may be at his work, I would not put a high value on his determinations if made while in a position which calls for constant anxiety for his own personal safety. I would go even further still, and say that even personal comforts or discomforts have much to do with the value of observations.

I propose, therefore, that all the various motions should be effected by water-power. There are water engines of various forms now made, some of which have no dead point, and having little *vis inertia*, are easily stopped and started, and are consequently well adapted for this work.

I propose to use four of them: one for the right ascension motion of the instrument, and one for the declination; one for revolving the dome, and one for raising and lowering the observer himself; but instead of having anything of the nature of a 25-foot chair or scaffold, I propose to make the 70-foot floor of the observatory movable. It is balanced by counterpoise weights, and raised and lowered at will by the observer. Then the observer can without any effort raise and lower the whole floor, carrying himself and twenty people if desired, to whatever height is most convenient for observation; and wherever he is observing, he is conscious that he has a 70-foot floor to walk about on, which even in perfect darkness he can do in safety.

The valves and reversing gear of the water engines are actuated by a piece of mechanism, the motive power of which may be a heavy weight raised into position some time during the previous day by man- or water-power. By means of a simple electrical contrivance, this piece of machinery itself is under the complete control of the observer, in whatever part of the room he may be, and he carries with him a commutator of a compact and convenient form, with eight keys in four pairs, each pair giving forward and backward movements respectively to (A) telescope movement in right ascension; (B) telescope movement in declination; (C) revolution of dome; (D) raising of floor.

The remaining operation—opening of shutter—is easily effected without any additional complication.

It is only necessary to anchor the shutter (which moves back horizontally) to a hook in the wall and move the dome in the opposite direction by motion C; the shutter must, of course, be opened by this motion. . . .

It is very possible that there may be some here who have found what I have had to say on the subject of the figuring of objectives very unsatisfactory. They may have expected, naturally enough, that, instead of treating of generalities to such a large extent as I have done, I should have given precise directions, by the following of which rigidly any person could make a telescopic objective.

To those, however, who have followed me in my remarks, the answer to this will probably have already suggested itself. It is the same answer which I give to those who visit my works and ask what the secrets of the process are, or if I am not afraid that visitors will pick up my secrets. All the various processes which I have described up to that of the figuring are, I consider, purely mechanical processes, the various details of which can be communicated or described as any mechanical process can be; but in the last final and most important process of all there is something more than this. A person might spend a year or two in optical works where large objectives are made, and might watch narrowly every action that was taken, see every part of the process, take notes, and so forth, and yet he could no more expect to figure an objective himself than a person could expect to be able to paint a picture because he had been sitting in an artist's studio for the same time watching him at his work. Experience, and experience only, can teach any one the art, and even then it is only some persons who seem to possess the power of acquiring it.

A well-known and experienced amateur in this work declared his conviction that no one could learn the process under nine years' hard work, and I am inclined to think his estimate was not an exaggerated one.

True, it may be said that large objectives can be and are generally turned out by machinery, but what kind of an objective would any machine turn out if left to guide itself, or left to inexperienced hands?

At the risk of being accused of working by what is generally called the rule of thumb, I confess that conditions often arise, to meet which I seem to know intuitively what ought to be done, what crank to lengthen, what tempering is required of the pitch square; and yet if I were asked I should find it very hard to give a reason for my so doing which would even satisfy myself.

I may safely say that I have never finished any objective over 10 inches diameter, in the working of which I did not meet with some new experience, some new set of conditions which I had not met with before, and which had then to be met by special and newly-devised arrangements.

A well-known English astronomer once told me that he considered a large objective, when finished, as much a work of art as a fine painting.

I have myself always looked upon it less as a mechanical

operation than a work of art. It is, moreover, an art most difficult to communicate. It is only to be acquired by some persons, and that after years of toilsome effort, and even the most experienced find it impossible to reduce their method to any fixed rules or formulæ.

INDIAN CASTINGS AT THE INDIAN AND COLONIAL EXHIBITION

AT the last meeting of the Iron and Steel Institute Mr. C. Purdon Clarke, C.I.E., Keeper of the Indian Section, South Kensington Museum, read a paper "On Certain Descriptions of Indian Castings" as follows:—

The importation of partly manufactured material is at present exercising considerable influence over many of the native arts of Oriental countries and India. The supply of machine-made thread has doubled the village hand-looms in some districts of Madras, and gold thread from Germany has enabled the brocade weavers to compete with the imitation brocades sent in from Europe.

In some handicrafts, however, the supply of European material has produced a contrary effect. Iron and steel, bar and rod, have displaced an ancient industry, and sheet copper and brass have robbed the founder of half his work. Formerly the only means of producing sheet-metal was by hammering cast plates, an expensive method, only resorted to when thin flat coverings were required for wooden or other objects. For very large vessels, where weight was required to be kept down and strength maintained, hammered sheet was used; but generally the founder was employed, to save as much as possible the labour of forming the furnished castings which required but little beating out, trimming, and brazing.

In the case of a bowl, or flat jar with a narrow mouth, the founder would prepare a cast not unlike in shape and thickness that of an ordinary flower-pot saucer, from which, by constant hammering, the bulbous sides would be formed, projecting beyond the rim, which would remain of its first diameter and thickness. When finished, such a vessel would be nearly double the size of the first cast, and a remarkable example of the native knowledge of the composition of bronzes and annealing processes.

It is worthy of noting that the chief means of detecting modern from old Persian and Saracenic metal vessels is by examining the brazen joints, which in ancient vessels are rare. When not found, a close examination will show the vessel to be a thin casting, the ornamentation being by inlay, or chasing and hammering, which, being done after the cast is made, gives the reverse side the appearance of chased sheet metal.

So far as he could ascertain, there were three methods of casting practised in India. The first, by moulds in sand; the second, moulds in clay not unlike plasterers' piece-moulds; the third, clay moulds formed on a wax model, the *cire perdu* of Europe.

The first of these was well known in Europe, but the second was, he believed, now described for the first time. In preparing the mould, impressions of the various parts of the pattern are taken in clay, and these pieces when nearly dry are, after trimming, stuck neatly together, and kept in place by several layers of mud, in which some fibre is mixed. The mould when ready has but one vent, which, placed on the most convenient side, is carried up into a sort of bottle-neck. If the object is small, several moulds are attached together, and the vents united by a single short neck of clay, to which a crucible, inclosed in an egg-shaped ball of clay, is attached. The size of this crucible depends upon the exact amount of metal required to fill the mould or moulds; and this quantity being known by experience, the founder places it inside before closing up. No provision is made for the escape of air from the mould when the metal is poured in. The mould and crucible (now in one piece) is allowed to dry; and after several coats of clay, tempered with fibre, have also been well baked on by the sun, the furnace is prepared. This is simply a circular chamber about 2 feet 6 inches in diameter, 2 feet in height, with a perforated hearth and no chimney. Half filled with charcoal, a good heat is obtained by the use of several sheepskin bellows from beneath. When ready, as many moulds as the furnace will hold are placed in it, the crucible end of each being embedded in the fire. A cover is placed over, and the fire kept up until, upon examination, the moulds are found to be red hot. They are then taken, one at a time, and replaced in a reverse position, the crucibles being now above. The metal flows down into a red-hot mould and penetrates the finest portions of the surface without suffering from air or chilling. The fire is allowed to gradually cool, and when the objects are broken out of their clay covering, the metal is soft and malleable.

The third manner of casting (that by the use of a wax pattern which is destroyed in the moulding) was well known, but in one particular case the process had been carried further than would be at first believed, and of this he would now attempt a description.

The object produced is an anklet, a flexible ring about 4 inches in diameter, made from an endless curb chain. Such curb chain trinkets are common in India, and are generally made from thick silver wire rings interlinked and soldered one by one. In this example the anklet is of bronze, and consists of a complicated chain of forty-three detailed links, the whole being cast by a single operation. The first part of the process is the preparation of a pattern in wax, a delicate work, each link having to pass through four others, and to bear three small knobs or rosettes. These are in two instances but ornaments; the third, however, serves as a channel for the metal to enter each ring.

Then commences the most difficult part of the work, each ring having to be slightly separated, and this is effected by painting in a thin coat of fine clay until there is sufficient to form a partition. Other coats of clay are added until a thickness of about half an inch is attained, when a groove is cut round the upper side of the ring, and deepened until the row of knobs is bared. The wax is then melted out, and the mould attached to a crucible as before described. When cast, and the mould broken away, the chain comes out inflexible, being attached to a rod which runs round where the groove was cut. This is broken off, and the chain is complete.

Having been consulted respecting the trades to be represented in the Indian Courts of the Colonial and Indian Exhibition, he recommended amongst others a good brassfounder to be sent. Dr. Tyler, who was charged with the collection of these artisans, engaged one of the best he could find, but up to the present the foundry is not in working order.

One of these combined crucible moulds was submitted for inspection, with fragments of another, also a cast curb chain anklet; the author concluding by thanking the members for this opportunity of publishing an interesting process.

A NEW SPECTROMETER

IN equipping the Physical Laboratory of University College, Dundee, I felt considerable difficulty in deciding on a spectrometer for accurate work; it was easy to get a simple instrument for qualitative experiments and rough quantitative work, but it was only after consulting several friends and communicating with two or three firms that about two years ago I wrote to Mr. Hilger, in the hope that from him we might obtain an instrument capable of working to as high a degree of accuracy as would enable our best students in the laboratory to do advanced work. Considering that a second of arc is by no means an unusual limit of error in angular measurement, and that it is of the order 1 : 1,000,000, the whole circle being unit, we thought that while further capability in reading power would be more than counterbalanced by various indeterminate errors, yet it should be possible to obtain this accuracy with a suitable instrument.

Prof. Liveing was kind enough to give us valuable information about one of his own instruments, of which the plans were sent to us by Mr. Hilger for inspection; and Mr. Capstick and I finally decided to ask Mr. Hilger whether he could not arrange two microscopes on the instrument in place of the one which Prof. Liveing's has.

As a consequence, Mr. Hilger presented suggestions for a spectrometer which is now in this college, and is capable of reading directly to one second of arc and yielding reliable results. Its construction is very simple. The collimator stands on a heavy pillar by itself; and the circle, divided to five minutes of arc on a ring 15 inches in diameter, with six radial spokes, is carried on another pillar. The telescope, counterpoised, turns on the same axis, but does not touch the circle at any point; and the reading is managed as follows: from the telescope-bearing a double girder with a semicircular plan tied across its diameter by tubes of brass stretches horizontally above the semi-circumference of the divided circle; to this girder are fixed, at its ends,

two long focus microscopes, whose axes produced intersect the divided circle at the extremities of a diameter. They are read by means of a pointer and spider-line micrometer, whose head is divided into 300 parts, each of which represents one second of arc. The microscopes are carried at such a height that they easily pass the collimator, and they can be read in any position, and the light from the collimator passes clear under the girder.

J. E. A. STEGGALL

THE ABACUS IN EUROPE AND THE EAST

AT a late meeting of the Asiatic Society of Japan (reported in the *Japan Mail*), Dr. Knott read a paper on "The Abacus, and its Scientific and Historic Import." A portion of the paper described the various arithmetical processes connected with the *soroban*, the form of the abacus employed in Japan. The writer pointed out that in all arithmetical operations up to the extraction of the cube root, the *soroban* really possessed distinct advantages over ordinary ciphering. This in itself explained why the instrument, which in Europe is suggestive of an infant school, has in the East survived till the present day. The rest of the paper was a discussion of the peculiar position which the abacus, used in its widest signification, holds in the history of the progress of arithmetic and mathematics, and of science and civilisation generally. The following is an abstract of the argument, the ultimate object of which was to explain why the abacus had died in Europe but lived in China, and why the cipher system of numerals had grown up in India but not in China.

The abacus, as used in China and Japan, bears, on the surface of it, evidence of a foreign origin. The numbers are set down on it with the larger denomination to the left—a method which could hardly be believed to have been invented by the Chinese, who tend to work from right to left, and who have always named their compound numbers beginning with the higher denominations. The Chinaman says "one hundred forty-five," as the Englishman does; but the Englishman once said "one hundred five-and-forty," as the German still does; while in some of the Aryan languages of India, and in the Arabic of to-day, the number is named "five-and forty and one hundred." The Arab writes from right to left, so that, had the abacus been invented by such a people who, so to speak, both wrote and spoke inversely, it would have indicated the number as it does. In fact, the abacus could only have arisen in its present form amongst a people who either wrote and spoke directly, or wrote and spoke inversely. As a matter of history, the geographical home of the abacus is India, but, unless there is conclusive evidence to the contrary, we cannot regard it as an invention of Aryan Indians, who, although they wrote directly, spoke inversely. They probably borrowed it from the Semitic merchants, and these, with their inverse speaking and inverse writing, may have invented it, or perhaps received it from a direct-speaking, direct-writing people, such as the highly-cultured Accadians seem to have been. The abacus was evolved, no doubt, from the human hand, which, with its ten fingers, was the only counting-board used by primitive man. Its course of development is quite distinct from that of the symbolic representation of numbers. These latter we can trace through four stages, which may be called the pictorial, the symbolic, the decimal, and the cipher. The pictorial we find in the Egyptian hieroglyphic and the Accadian cuneiform; the symbolic in the hieratic, Phœnician, Hebrew, Greek, Roman, and the host of systems which grew up with the development and spread of alphabets and syllabaries, and the decimal in the simplification of these which live to-day in the Chinese and Tamilic systems. Once the decimal stage was reached, its general similarity to the abacus indications suggested bringing them into still closer correspondence. This took place amongst the Aryan Indians, who, along with their brethren of the West, very soon discarded the abacus for the, to them, more convenient cipher notation. With the Chinese and Tamils, however, no advance was made in this direction, a fact especially surprising in the case of the latter, who have lived in close contact with peoples that have long used the cipher system of numerals. One reason for the Chinese conservatism in so adhering to an unwieldy notation might be their vertical mode of writing, with which no very striking similarity between their symbolising of numbers and the abacus columns would appear. But this does not explain the conservatism of the Tamils, who write from left to right, nor yet the persistence of the abacus for centuries face to face with the Indian cipher system. The explanation is rather to be found in the system of nomenclature, which, being direct both among the Chinese and the Tamils, fitted perfectly with the abacus indications. For this reason the manipulation of the abacus in China and Japan is more rapid and certain than ciphering, and hence, there being no advantage for simple arithmetical operations in the latter, the cipher system did not develop in these countries, and even when introduced from the West in all its vigour could not displace "the rod and the beads." An Aryan Indian, with his inverse-speaking, could never work the abacus with the same facility as the Japanese unless he worked from right to left, a mode of procedure quite foreign to his nature. It is not so foreign to the Chinese and Japanese, however, to work from left to right, as shown in the formation of each individual ideograph employed in writing. Hence the abacus suited the Chinese language better than it did any of the Aryan languages in their original mode of number-naming. The influence of the notation which was developed from Semitic sources under the influence of the abacus, has in later times compelled many of the Aryan languages to assimilate as far as possible to the direct mode of numeration; but in the English *fifteen*, the German *fünfzehn*, and the French *quinze*, we still have the relics of the original inverse mode of naming, alike peculiar to Aryan and Semitic peoples.

In the course of the discussion which followed, it was mentioned that Chinese mathematics were first studied in Japan about 900 A.D., and that the Japanese ascend by powers of 10,000 in their treatment of larger numbers.

THE GAZETTEER OF RUSSIA[1]

WE have received the concluding fascicule of the "Geographical and Statistical Dictionary of the Russian Empire," published by the Russian Geographical Society, and edited by M. P. Semenoff. This monumental work, which was begun more than twenty years ago, has been now concluded in five large octavo volumes, and will remain for many years the most trustworthy and complete source of information for the geography of the empire, exclusive of Poland, but inclusive of the former Russian dominions in America. It may be regretted that the editor of the "Dictionary" has been diverted by so great a variety of geographical, statistical, and administrative work from this undertaking, and that therefore the last fascicule appearing twenty-three years after the first, the statistical information contained in the first fascicules and volumes has become out of date. But notwithstanding that, the "Dictionary" has not become old. Its value is not in the statistical data it contains; it is much more in the excellent geographical descriptions of the localities treated—that is, of each separate government of Russia, Siberia, Turkestan, and Caucasus—of the seas that border Russia, and their islands. Several articles are excellent and most complete monographs, and we need only mention those on the Amur, Caucasus, Sakhalin, the Northern Ocean, or Turkestan to remind geographers of these excellent descriptions of whole regions. The geology, the flora and fauna of each region have received much attention. These descriptions will not soon be old—they can be only completed.

At the end of each article there is, moreover, a most complete bibliography of the larger geographical works in which the place described in the article has been mentioned, as also of monographs dealing with it, and of newspaper articles. This bibliography is invaluable for the geographer. On the whole, the equally high standard of all geographical descriptions and the unity of conception in all of them—the whole being the work of the editor himself, assisted only by M. Zverinsky and very few occasional contributors—make this "Dictionary" occupy one of the first ranks among like publications. An appendix is promised, which will contain descriptions of such regions as the Thian-Shan, Ferganah, and Transbaikalia, which were much explored during the publication of the "Dictionary." They will embody all recent information.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CONVOCATION of the University of London met on Tuesday evening to consider the report of a Special Committee which proposed several important changes in the constitution of the

[1] "Geographitchesko-Statistitcheskiy Slovar Rossiyskoy Imperii,' P. P. Semenova.

University. After considerable discussion, resolutions were passed approving of the admission of certain educational institutions having one, or more than one, faculty of University rank as constituent Colleges of the University, of the establishment of a Council of Education, and of certain changes in the constitution of the Senate.

SCIENTIFIC SERIALS

THE most important paper in the *Journal of Botany* for April is the commencement of a Synopsis of the Rhizocarpeæ, by Mr. J. G. Baker, another of the series of this writer's exhaustive monographs of the families of Vascular Cryptogams outside the Ferns. The present instalment includes the genus Salvinia, in which three new species are described, and a portion of Azolla. In the May number we find a continuation of Mr. W. B. Grove's paper on new and noteworthy fungi, in which several new species are described, and one new genus of Sphæroideæ, Collonema. Mr. W. H. Beeby gives further particulars respecting the distribution of his newly discovered *Sparganium neglectum*, and Mr. Arthur Bennett an account of the distribution in Britain of the various species of Potamogeton, in addition to those contained in the second edition of "Topographical Botany."

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 20.—"Relation of 'Transfer-Resistance' to the Molecular Weight and Chemical Composition of Electrolytes." By G. Gore, LL.D., F.R.S.

In the full paper the author first describes the method he employed for measuring the "resistance," and then gives the numerical results of the measurements in the form of a series of tables.

He took a number of groups of chemically related acids and salts of considerable degrees of purity, all of them in the proportions of their chemical equivalent weights, and dissolved in equal and sufficient quantities of distilled water to form quite dilute solutions. The number of solutions was about seventy, and included those of hydriodic, hydrobromic, hydrochloric, hydrofluoric, nitric, and sulphuric acids; the iodides, bromides, chlorides, fluorides, hydrates, carbonates, nitrates, and sulphates, of ammonium, cæsium, rubidium, potassium, sodium, and lithium; the chlorides, hydrates, and nitrates, of barium, strontium, and calcium; and a series of stronger solutions, of equivalent strength to each other, of the chlorides of hydrogen, ammonium, rubidium, potassium, sodium, lithium, barium, strontium, and calcium. A series of similar liquids to those of one of the groups of acids, of equal (not of equivalent) strength to each other, was also included.

As electrodes, he employed pairs of plates of zinc, cadmium, lead, tin, iron, nickel, copper, silver, gold, palladium, and platinum; and separate ones formed of small bars of iridium.

He took each group of solutions, and measured in each liquid separately, at atmospheric temperature, the "total resistance" at the two electrodes, and the separate "resistances" at the anode and cathode respectively with each other, and thus obtained about seventy different tables, each containing about thirty-six measurements, including the amounts of "total," "anode," and "cathode" resistance of each metal, and the "averages" of these for all the metals.

By comparing the numbers thus obtained, and by general logical analysis of the whole of the results, he has arrived at various conclusions, of which the following are the most important:—The phenomenon of "transfer-resistance" appears to be *a new physical relation of the atomic weights*, attended by inseparable electrolytic and other concomitants (one of which is liberation of heat, *Phil. Mag.*, 1886, vol. xxi. p. 130). In the chemical groups of substances examined *it varied inversely as the atomic weights of the constituents, both electro-positive and electro-negative, of the electrolyte, independently of all other circumstances*; and in consequence of being largely diminished by corrosion of the electrodes, it appeared to be intimately related to "surface-tension." He suggests that corrosion may be a consequence, and not the cause of small "transfer-resistance." The strongest evidence of the existence of the above general law was obtained with liquids and electrodes with which there was the least corrosion and the least formation of films; those liquids were dilute alkali-chlorides, with electrodes of platinum.

This research is an extension of a former one on "Transfer-Resistance in Electrolytic and Voltaic Cells," communicated to the Royal Society, March 2, 1885. Further evidence on the same subject has been published by the author in the *Philosophical Magazine*, 1886, vol. xxi. pp. 130, 145, 249.

"A Study of the Thermal Properties of Ethyl Oxide." By William Ramsay, Ph.D., and Sydney Young, D.Sc.

A year ago a paper was communicated to the Society on the behaviour of ethyl alcohol when heated. A similar study of the properties of ether has been made, in which numerical values have been obtained exhibiting the expansion of the liquid, the pressure of the vapour, and the compressibility of the substance in the gaseous and liquid conditions; and from these results, the densities of the saturated vapour and the heats of vaporisation have been deduced. The temperature range of these observations is from $-18°$ to $223°$ C.

It is the authors' intention to consider in full the relations of the properties of alcohol and ether; in the meantime it may be stated that the saturated vapour of ether, like that of alcohol, possesses an abnormal density, increasing with rise of temperature and corresponding rise of pressure; that at 0° the vapour-density is still abnormal, but appears to be approaching a normal state; and that the apparent critical temperature of ether is 194° C.; the critical pressure very nearly 27,060 mm. = 35·61 atmospheres; and the volume of 1 gramme of the substance at 184 between 3·60 and 4 c.c.

Mathematical Society, May 13.—J. W. L. Glaisher, F.R.S., President, in the chair.—Mr. F. W. Watkin was admitted into the Society.—The following communications were made:—On Cremonian congruences contained in linear complexes, by Dr. Hirst, F.R.S.—Solution of the cubic and biquadratic equation by means of Weierstrass's elliptic functions, by Prof. Greenhill.—On the complex of lines which meet a unicursal quartic curve, by Prof. Cayley, F.R.S.—On Airy's solution of the equations of equilibrium of an isotropic elastic solid under conservative forces, by W. J. Ibbetson.—Conic note, by H. M. Taylor.—On the converse of stereographic projection and on contangential and coaxal spherical circles, by H. M. Jeffery, F.R.S.

Zoological Society, May 18.—Prof. W. H. Flower, F.R.S., President, in the chair.—Mr. C. W. Rosset exhibited a series of photographs taken during his recent visit to the Maldive Islands, and made some remarks on the zoological collections obtained during his expedition.—Mr. Philip Crowley, F.Z.S., exhibited some pupæ of nocturnal Lepidoptera which had been sent to him from Natal; and read some notes from his correspondent, which proved that they were subterranean.—Mr. Joseph Whitaker, F.Z.S., exhibited a specimen of Wilson's Phalarope, said to have been obtained at Sutton Ambian, near Market Bosworth, in Leicestershire.—A communication was read from Dr. A. B. Meyer, C.M.Z.S., containing an account of the known specimens of King William the Third's Bird of Paradise (*Rhipidornis gulielmi-tertii*), and remarking on a fourth specimen which had been recently obtained by the Dresden Museum.—Mr. Frank E. Beddard read a paper on some new or little-known Earthworms, together with an account of the variations in structure exhibited by *Perionyx excavatus*.—Mr. Sclater read a paper on the species of Wild Goats and their distribution. Mr. Sclater recognised ten species of the genus Capra, distributed over an area extending from Spain to Southern India, and from Central Siberia to Abyssinia.

Royal Meteorological Society, May 19.—Mr. W. Ellis, F.R.A.S., President, in the chair.—Mr. L. T. Cave and Rev. C. Malden, M.A., were elected Fellows of the Society.—The following papers were read:—The severe weather of the past winter, 1885–86, by Mr. C. Harding, F.R.Met.Soc. The author showed that the whole winter was one of exceptional cold, not so much on account of any extremely low temperatures experienced, but more from the long period of frost and the persistency with which low temperature continued. In the South-West of England there was not a single week from the commencement of October to March 21 in which the temperature did not fall to the freezing-point. In many parts of the British Islands frost occurred in the shade on upwards of 60 nights between the beginning of January and the middle of March, and during the long frost which commenced in the middle of February and continued until March 17 the temperature fell below the freezing-point in many places on more than 30 consecutive nights. At Great Berkhamsted, in Hertfordshire, frost

occurred on the grass on 73 consecutive nights from January 5 to March 18. The winter of 1885–86 was the only one in which there was skating on the water of the London Skating Club, in Regent's Park, in each of the four months December to March, since the formation of the Club in 1830, and there are but four records of skating in March during the 56 years, and none so long as in the present year. With regard to the temperature of the water of the Thames at Deptford, it was shown that the total range from January 8 to March 20 was only 6°, whilst from March 1 to 19 the highest temperature was 36°·5, and the lowest 35°. The temperature of the soil at the depth of 1 foot was generally only about 2° in excess of the air over the whole of England, and from March 1 to 17 the earth was colder than usual by amounts varying from 6°·3 at Lowestoft to 8°·5 at Norwood. The facts brought together showed that the recent winter was one of the longest experienced for many years, and that in numerous ways it may be characterised as "most severe."—Description of an altazimuth anemometer for recording the vertical angle as well as the horizontal direction and force of the wind, by Mr. L. M. Casella. The author describes an anemometer he has made which records continuously on one sheet the pressure, direction, and inclination of the wind.—Earth temperatures, 1881–85, by Mr. W. Marriott, F.R.Met.Soc. This is a discussion of the observations of the temperature of the soil at various depths below the surface, which have been regularly made at 9 a.m. at several of the stations of the Royal Meteorological Society during the past five years. The results show that the temperature of the soil at 1 foot at nearly all the stations in the winter months is almost the same as that of the air, while in the other months of the year the temperature of the soil is higher than that of the air at all except that of the London stations.—Note on the after-glows of 1883–84, by Mr. A. W. Clayden, M.A., F.R.Met.Soc. The author suggests that the after-glows were the result of the water-vapour erupted from Krakatão, and that the dust and other ejecta played but a secondary part in the production of the phenomena.

Sydney

Linnean Society of New South Wales, March 31.—Mr. William A. Haswell, M.A., B.Sc., in the chair.—The following papers were read:—On certain Geckos in the Queensland Museum, by Charles W. de Vis, M.A. A new species of the very curious genus *Nephrurus* is described under the specific name of *lævis*, from its smooth lepidosis, as compared with the only other species, *N. asper*. A species of *Diplodactylus* (*D. venicauda*) is also described. Both lizards are from Northern Queensland.—Description of a new aphanipterous insect from New South Wales, by A. Sidney Olliff, F.E.S., Assistant Zoologist, Australian Museum. The remarkable parasite here characterised under the name *Echidnophaga ambulans* was found in large numbers on the head and breast of a porcupine anteater (*Echidna hystrix*). It differs from the *Pulex echidnæ* described by Denny from the same host in habit as well as in several important points of structure, and is, therefore, regarded as forming the type of a new genus. Unlike the majority of its allies this species does not appear to possess the power of jumping.—On a microscopic fungus parasitic on the Cucurbitaceæ, by E. Haviland, F.L.S. In this paper the author gives an account of his inquiry as to the origin of a disease which has caused much destruction to melon and pumpkin plants during the last three months, and which he has identified as the micro-fungus *Oidium monilioides*. As a preventative he suggests greater care in cultivation, and quotes various authors proving that old plants will thereby be sufficiently vigorous to resist the attacks of the fungus.—Jottings from the Biological Laboratory of Sydney University, by William A. Haswell, M.A., B.Sc., Lecturer on Zoology and Comparative Anatomy. On the myology of the flying squirrel (*Petaurista taguanoides*). In its muscular anatomy the flying phalanger nearly resembles the vulpine phalanger and the *Cuscus*, with a few special modifications, of which the chief is the presence of a peculiar "long femoro-caudal muscle."—Insects of the Fly River, New Guinea, "Coleoptera," by William Macleay, F.L.S., &c. This is the second paper communicated by Mr. Macleay on the insects collected during the recent expedition organised by the Geographical Society of Australia for the exploration of the interior of British New Guinea. The previous paper dealt with the Coleoptera up to the end of the Heteromera. The present one deals with the families *Curculionidæ*, *Brenthidæ*, *Anthribidæ*, and *Longicornia*, comprising in all 96 species, of which 31 are now described for the first time.—The Mollusca of the Pareora and Oamaru systems of New Zealand, by Capt. F. W. Hutton, Hon. Member Linnean Society, New South Wales. Capt. Hutton's paper is a contribution towards the correlation of the Tertiary rocks of Australia with those of New Zealand, and it enumerates 268 species of Mollusca from the Pareora and Oamaru systems, which are probably of Miocene and Oligocene age, of which 184 species are confined to the Pareora beds, 33 species to the Oamaru, while 51 species, of which a few are doubtful, are common to both.

Paris

Academy of Sciences, May 17.—M. Jurien de la Gravière, President, in the chair.—Presidential allocution on the occasion of the homage offered to M. Chevreul at the meeting of Monday, May 17, when that illustrious member and *doyen* of the Academy completed his hundredth year. In reply, M. Chevreul assured the audience that to be told his long career had been useful to science and his country was the greatest eulogium he had ever ambitioned.—Observations in reference to the quantitative analysis of the ammonia found in the ground: a reply to M. Schlœsing, by MM. Berthelot and André. The authors point out that the note recently published by them in the *Comptes rendus* was not intended to raise any discussion on M. Schlœsing's theories regarding the absorption of atmospheric ammonia by arable lands. Their main object was to explain a special precaution and a common source of error in the quantitative analysis of the ammonia present in the ground. Nor did they wish to deny that the ground receives in a general way a supply of ammonia from the atmosphere, although they did not consider that this fact had been fully demonstrated by M. Schlœsing's experiments.—Reply to M. Taurines's recent observations on the communication of March 23, 1885, regarding marine engines and the experiments made on board the *Primauguet*, by M. A. Ledieu. The author maintains the general correctness of his conclusions, which are unfairly stigmatised by M. Taurines as "theories conceived *a priori* and at times dangerous."—Remarks on the third volume of the Scientific Mission to Cape Horn, presented to the Academy by M. Mascart. This volume contains all the observations regarding terrestrial magnetism, and MM. Müntz and Aubin's analyses of the specimens of atmospheric air collected by Dr. Hyades. The researches on terrestrial magnetism were greatly aided by a continuous registering apparatus, which was set up by MM. Payen and Le Cannellier, and which worked satisfactorily the whole time the Mission remained in Orange Bay. Incidental reference was made to the subsequent death of M. Payen in France, and of M. Martial, commander of the Expedition, in China.—Elements of the orbit of Brooks's comet, No. 1, by M. Lebeuf. These elements, deduced from observations made at Kiel on April 30, and at Paris on May 4 and 8, are as under:—

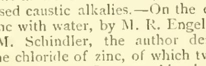
$$T = 1886 \text{ June } 7{\cdot}5158 \text{ Paris Mean Time.}$$

$$\left.\begin{aligned} \Omega &= 193^\circ\ 1'\ 29''{\cdot}5 \\ \varpi &= 33\ \ 42\ \ 7{\cdot}1 \\ i &= 87\ \ 47\ \ 34{\cdot}7 \end{aligned}\right\} \text{Mean Eq. } 1886{\cdot}0.$$

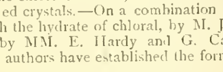
$$\log q = 9{\cdot}439104$$

—Observations of Brooks's comets made at the Observatory of Algiers with the 0·50 m. telescope, by M. Rambaud.—Measurement of the electric conductivity of the dissolved chloride of potassium, by M. E. Bouty. Between the temperatures of 0° C. and 30° C. the resistance of the solutions of the chloride of potassium is expressed with sufficient accuracy by the binomial formula $r_t = \dfrac{r_0}{1 + \alpha t}$. A table is given of the absolute values of the specific resistance r_0 and the relative values of the molecular resistance ρ_0, as well as the values of α.—On the atomic volume of oxygen, by M. E. H. Amagat. M. Wroblewski having recently announced that the atomic volume of oxygen was considerably below 16, the author points out that he had arrived at the same conclusion early in 1885. In his communication of March 2 of that year he stated that under a pressure exceeding 4000 atmospheres he had succeeded in obtaining oxygen with a density higher than 1·25 and at a temperature of 17°.—Observations on the deviation from the vertical on the south coast of France, by M. Germain. From four determinations obtained at Nice, Saint-Raphael, Toulon, and Marseilles, the author infers that on this seaboard the continent attracts the vertical, that is to say, repels the astronomic as opposed to the geodetic zenith, and that this attraction

appears to be exercised by a point situated to the north of Nice in the Alps.—On the barometric pressure of May 13, 1886, when at 4 o'clock in the morning the barometer fell to 737·37 mm., the lowest recorded in Paris since the year 1757, by M. E. Renou. This remarkable fall coincides with violent atmospheric disturbances in Madrid and other parts of Spain, in England and the United States. The stormy weather reached Italy and Germany on May 14, when the Jura and Chaux-de-Fonds were covered with snow.—Action of vanadic acid on the ammoniacal salts (continued), by M. A. Ditte. In this paper the author deals with a second group comprising the sulphate, chromate, iodate, borate, acetate, vanadate, perchlorate, carbonate, and hydrochlorate of ammonia.—On several double silicates of alumina, and of potassa or soda, by M. Alex. Gorgeu. The kaolin with which these silicates are obtained is that used at the Sevres works. This composition when dried at a temperature of 120° C. is almost exactly that of the silicate of hydrated alumina, $2SiO_2Al_2O_3,2HO$. Its action is described on the alkaline haloid salts, on the alkaline carbonates, and on the fused caustic alkalies.—On the combinations of the chloride of zinc with water, by M. R. Engel. Besides that discovered by M. Schindler, the author describes three other hydrates of the chloride of zinc, of which two may be obtained in large isolated crystals.—On a combination of phosphuretted hydrogen with the hydrate of chloral, by M. J. de Girard.—On pilocarpine, by MM. E. Hardy and G. Calmels. For this substance the authors have established the formula—



and for pilocarpidine—



—Researches on the composition of carotine, its chemical function, and its formula, by M. A. Arnaud. This is a carburet of hydrogen ($C_{26}H_{38}$) identical with the orange-red crystallised substance which the author has extracted from the leaves of various kinds of plants. This colouring-matter exists also in a great many fruits, and especially in the tomato, and may in fact be said to be universally present in the roots, leaves, and fruits of plants. It oxidises in the air even at the ordinary temperature, and especially about 70° C., and in solution this oxidation becomes extremely rapid.—Remarks on the bilobites, by M. Stan. Meunier. The author makes a fresh study of these interesting vestiges, without deciding the question whether they are mere animal footprints, as supposed by M. Nathorst, or real fossil algæ, as maintained by MM. Delgado and De Saporta.—Characteristics of the stem of Poroxylon (fossil gymnosperms of the Carboniferous epoch), by MM. C. Eg. Bertrand and B. Renault.—Account of a meteor recently observed on board the steamer *Algérie* in the Gulf of Smyrna, by M. L. Aubouy.

BERLIN

Physiological Society, April 30.—Dr. Wolffberg spoke on the Young-Helmholtz theory of the colour-sense, which he extended in the direction of assuming the existence of red-sensitive, green- and violet-sensitive ganglia in the central organ of sight-perception in the sphere of vision. These ganglia were connected with the red nerves, the green nerves, and the violet nerves, and by means of such nerves communicated with the retina. Seeing, however, that yellow, blue, and white were likewise psychically simple sensations, Dr. Wolffberg assumed specific ganglia for these as well, which, however, stood in connection with the red, green, and violet ganglia, the yellow ganglia being situated at an equal remove from the red and green, but at a further remove from the violet ganglia. Similar was his conception of the situation and connection of the blue and white ganglia. Regarding the sensation of black, he would speak in an address in the immediate future.—Dr. Uhthoff made further communications respecting the dependence of visual sharpness on the intensity of illumination. After an historical survey of the older experiments to determine the relation of visual sharpness to light intensity, he described the results of his own labours in this field. In the case of white light, he had communicated the relation on a former occasion (NATURE, vol. xxxi. p. 476). In the case of yellow light, the visual sharpness under low intensities increased just as rapidly with increasing intensity of light as in the case of white light. The curve, however, in the former case attained a greater height than it did with white, and then likewise proceeded parallel to the abscissa. With red light, on the other hand, the curve kept below the height reached with white light; it rose slower, moreover, and never became parallel. The curve of visual sharpness for green light lay still deeper than for red, and also rose persistently, though slowly. The curve for blue light lay deepest of all, and very soon became parallel to the abscissa of the light intensity. In the case of a green-blind person, the curves for white, yellow, and red were the same as in the case of the normal eye, as there was likewise a coincidence for blue. The curve for green fell almost coincident with the low curve for blue.

BOOKS AND PAMPHLETS RECEIVED

"Contra-Indications for Visiting the High Altitudes," with a Description of the Environs of Maloja, by Dr. A. T. Wise (Churchill).—"The Pictorial Arts of Japan," part ii., by Wm. Anderson (S. Low).—"Bees and Bee-keeping," part ix., by F. R. Cheshire (U. Gill).—"Fancy Pigeons," 3rd edition, part ix., by J. C. Lyell (U. Gill).—"British Cage Birds," part ix., by R. L. Wallace (U. Gill).—"Bicycles and Tricycles of the Year 1886," by H. H. Griffin (U. Gill).—"Mineralogical Magazine," March.—"Journal of Physiology," April.—"Proceedings of the Physical Society, St. Petersburg," vol. xviii. part 4.—"Bulletin de l'Académie Impériale des Sciences de St. Petersburg," vol. xxxi. No. 1.—"Chemical Atlas," part 1, by C. Peddie (Thin, Edinburgh).—"The Baths, Bathing, and Attractions of Aix-les-Bains," by Dr. W. Wakefield (S. Low).—"Bulletin of the United States Fish-Commission," vol. v., for 1885 (Washington).—"Causeries Scientifiques," by Hy. Vivarez (J. Michelet, Paris).—"Proceedings of the American Philosophical Society," April.—"American *versus* English Methods of Bridge Designing" (Japan Mail).—"Third Report on the Chemical Composition and Physical Properties of American Cereals, Wheat, Oats, Barley, and Rye," by C. Richardson (Washington).—"Memoir of Arnold Guyot, 1807 to 1884," by J. D. Dana.

CONTENTS

THURSDAY, JUNE 3, 1886

BRITISH FUNGI

Hymenomycetes Britannici—British Fungi (*Hymenomycetes*). By Rev. John Stevenson, Author of "Mycologia Scotica." Vol. I. Agaricus—Bolbitius. 8vo, pp. 372, with Cuts. (Edinburgh: William Blackwood and Sons, 1886.)

A QUARTER of a century ago, the number of persons in this country who made any pretence to study the fungi might have been counted on the fingers, and almost on the fingers of one hand. At that time Berkeley's "Outlines of British Fungology" had just appeared, but with it came no visible evidence of an increased number of students. An unfortunate desire to limit the volume to a definite size and price acted injuriously upon its contents. Half the book was a mere list of names without descriptions, and in the other half the descriptions were reduced to short diagnoses, quite insufficient except for those who were somewhat expert in the study. The young student struggling to get some knowledge of these obscure plants had no alternative but to fall back on the supplementary volume of Hooker's "English Flora" for the information he needed, oftentimes with much disappointment. Hence it is not surprising that only a few had the courage to persevere in a study for which there was no adequate text-book. It was not until 1871 that Cooke's "Hand-book of British Fungi" supplied what was required, and imparted a stimulus to the pursuit of that section of British botany, which has gradually increased in force, until at the expiration of another fifteen years, the "Hand-book" is out of print, and out of date, with a greatly augmented body of students looking anxiously for a new edition, or an entirely new work.

At this crisis, and under these circumstances, the work now before us has made its appearance, opportunely, and it is to be hoped satisfactorily, to fill a vacant place. No apology is offered, and none is required, where there is no rivalry, and a manifest necessity has been created by the flux of time. If the new work fulfils all the conditions of such a "Hand-book" of mycology as the student would require, there is a good and valid case in its favour. It must be conceded that although his previous "Mycologia Scotica" was little more than a localised catalogue, there was every reason to believe that the Rev. John Stevenson would bring practical experience and literary ability to his task, and acquit himself well in the production of a more elaborate work. In the result his friends have no reason to be disappointed. He has laboured conscientiously, and although in some things we do not agree with him, has accomplished a useful task.

It is hinted in the preface, although not clearly stated, what is the character of the book, namely, that it is practically a translation of Fries's "Monographia" in so far as the British species are concerned. There is no doubt that this was the best course to adopt, because there can be no two opinions of the value of Fries's observations, and the book in which they are written is very rare, and beyond the reach of the ordinary student. Still it would have been better not to have left this point in suspense, since a long detailed description which can be attributed without reservation to Fries is of infinitely more value than the most careful compilation would be. Unfortunately, any one who opens the book to consult it for the first time will at once conclude that the descriptions are the original production of the author, whose name appears on the title-page. We do not for a moment imagine that there was any desire to appropriate wholesale and take credit for the product of another man's brains, but unfortunately that *is* done sometimes in scientific books, and an honest author should be above suspicion.

Of the type, paper, and general appearance of the work, including the woodcuts by Mr. Worthington Smith, we have nothing to say except in strong commendation. But we cannot help quoting one sentence from the preface, which at least is original—"The tendency in recent years has been to multiply species unnecessarily, and ultimately many so-called species must disappear. The pruning-knife must be unsparingly used; but this must be the work of a Congress of Cryptogamic Botanists, not of individual authors." This quotation is made without intention of dissenting from it, but as a prelude to a statement of the fact that in the present volume two sub-genera and about fifty species (good, bad, or indifferent) which have been recorded as British, some on the authority of the Rev. M. J. Berkeley, and many of them figured, are entirely excluded without comment or apology. Was this "the work of a Congress of Cryptogamic Botanists or of an individual author?"

Some writers, and compilers, of the present day exercise a questionable originality in the correction, or alteration, of the orthography of generic names which have been in use for, perhaps, half a century. No useful purpose is served, except the gratification of personal vanity, and the multiplication of synonymy. We note, on p. 304, an instance of this kind, where *Psalliota* is written *Psaliota*. Without inquiring which is most accurate, or most elegant, surely its uniform use by Fries, in the previous form, since 1821, should have been sufficient to protect it from the "pruning-knife," and given it some title to usage in perpetuity. To such manipulators of names we would commend the following sentence from De Candolle's Commentary on the Laws of Botanical Nomenclature:—"In these kinds of questions, it must be borne in mind that the fixity of names is of superior importance."

We observe also two or three instances in which the orthography of specific names has undergone a change, but as it is just possible that these may be referred to typographical errors, and not to any intentional mutilation, we will accord the author the benefit of the doubt.

Some apology is made in the preface for a departure in the present work from the ordinary method of giving first a short diagnosis of the species, and afterwards a detailed description. "I am aware," it says, "that the departure from this method will touch existing prejudice; but it seems desirable to avoid repetition, to the extent of one-third, or one-half, in the account of each species, and thereby to secure space for fuller description. Moreover the *diagnosis* is not lost. From the arrangement which is adopted in printing, the student, if he is a student at

all, can at a glance pick it out for himself." We confess that we are not conscious ourselves of any prejudice which this arrangement touches inconveniently, since the diagnoses, by themselves, can be obtained in another form.

This first volume extends to the end of the genus *Bolbitius;* a second volume is proposed to complete the work, embracing all the British Hymenomycetes. Thus far we have descriptions of 822 species, corresponding to 485 which were included in the "Hand-book of British Fungi" in 1871, and 383 in Berkeley's "Outlines" in 1860; whilst all the European species included in Fries's "Hymenomycetes" up to the same point was 1271. Hence it would appear that two-thirds of the species enumerated by Fries as European have been found in the British Isles. This may not be absolutely accurate, since there are some included in the present volume which are not to be found in Fries, but the proportion is small and will not much affect the ratio. It is an interesting fact that the number of British species has been nearly doubled in fifteen years, which at least must be taken to indicate a larger number of observers and increased activity, for which there was doubtless some good and sufficient cause. Although coloured figures of upwards of 700 out of the 822 species have been published in this country since 1881, that would scarcely have been an appreciable factor in the result.

Criticisms of particular species would prove of little interest to any but practical mycologists, and therefore we forbear. In these times, when authority is held to have such slender claims, and independence of opinion is esteemed more highly than respect for the convictions of the old masters, it is a great consolation to encounter such an earnest and faithful disciple of the good old mycologist of Upsal as we meet with in the author of the book before us. Yet, notwithstanding this good trait, he has evidently a weak place in his human nature, without the tact to conceal it, and this is to be regretted, since rancour—like young chickens—comes home to roost.

M. C. C.

A MEDICAL INDEX-CATALOGUE

Index Catalogue of the Library of the Surgeon-General's Office, United States Army. Vol. VI. Heastie–Insfeldt. (Washington: Government Printing-Office, 1885.)

AMONGST the vast and rapidly-increasing mass of scientific literature it is a singular satisfaction to meet with a first-rate work such as this "Index Catalogue," which holds out good promise of being a clue to some parts at least of what is far too large for any single grasp. And if in any department of science it is more important than in another to trace generalisations to their foundations upon observations, and to have the facts before one, it is in medicine, which still contains so many dogmas whose foundations are not beyond attack, and so many observations in want of an adequate theory to explain them. In giving a clue to medical knowledge this "Index Catalogue" is in one respect at least, and in one very important respect, unique among its class; for under subject-headings such as, in this volume, hernia, hooping-cough, hydrophobia, hip-joint, hospitals, hygiene, insanity, &c., it gives a list not only of all the books and pamphlets in the library dealing with them, but also a list of the full titles of all the articles on them in all the periodical literature that it possesses, *Journals*, *Transactions*, *Reports*, *Reviews*, *Bulletins*, &c.; and when we reflect that the list of such periodicals taken in by the Surgeon-General's Office amounts now to at least 3005 (of which a very considerable proportion are weekly or monthly publications), such a careful classification of their separate articles would seem to be beyond all hope. However, the unexampled energy of Mr. J. S. Billings and his able assistants, which gives us every month the *Index Medicus*, has proved equal to this gigantic task, which it would have seemed to most men mere foolishness to attempt. The advantage to the student is immense; for in such periodical literature, by modern fashion, a great number of important facts in medicine lie buried, and there would hardly be a chance of finding them without some such help as is given us here. For though the literature of science is far less at present in bulk than the literature of some other subjects, most notably divinity, yet the literature of natural science, even in one of its many subdivisions, such as medicine, is paralysing in its profusion. To take as an instance the literature of a disease which, though just at present it is the fashion to talk much about it, is yet so rare that many doctors with considerable experience have never seen it, viz. hydrophobia, we find catalogued here not only 368 books dealing with it specially, but also the full titles of more than 1900 signed articles, not in the general but the medical press of the European languages, that have to do with it as well; and yet that is not a fifth part of what is catalogued under "Cholera" in Vol. III., and not a tenth of what is catalogued under "Fever" in Vol. V. The subdivision and arrangement of the masses of information so gathered together is admirable, and that, for subjects so difficult to deal with as hospitals and hygiene, which occur in this volume, is not a little to be proud of, and one that any student will appreciate. To the accuracy of every entry it would be absurd to pretend to testify on our own investigation, but frequent use of the five preceding volumes and some testing of this sixth volume leave us in little doubt that a very high standard was previously reached, and will be found to be maintained, and of course that is one of the points of cardinal importance in what is practically a dictionary of reference.

The Washington Library, or, as we should say more accurately, the "Library of the Surgeon-General's Office, United States Army," is one of the two or three largest collections of medical books in the world, and its growth has been astonishingly rapid. It was begun in 1830, and, after the first thirty years, in 1860, it contained only 350 volumes. To what size, at the end of the next thirty years, in 1890, we may see it grow we hardly venture to speculate; but in 1883 it stood at about 60,000 books and 66,000 pamphlets, and took in more than 2600 periodicals; and yet a careful critic last year estimated that for every hundred medical books that were in both the Washington Library and the British Museum there were also another hundred in each that were not in the other. If that be true, it would not astonish us to hear that for every hundred held in common there were fifty or more in the Bibliothèque Nationale at Paris, which were not to be

found in either of them. The largest medical specialist library in England, that of the Medical and Chirurgical Society, cannot claim to be more than half the size of the Washington Library, or to contain many books that are not to be found elsewhere; and it does not grow with all the rapidity of the New World.

For some time probably all seekers after the most difficult and most complete medical knowlege will have to turn to this "Index Catalogue," and will trust that Mr. Billings may be able to go on year after year putting forth his modest quarto of 1000 pages, until six or seven years may see him at the end of his great work.

A. T. MYERS

OUR BOOK SHELF

Mémoires de la Société des Sciences Physiques et Naturelles de Bordeaux. 3e série, tome i. (Paris: Gauthier-Villars, 1885.)

WE have frequently had occasion to direct our readers' attention to the high-class memoirs which this energetic Society puts forth. The volume before us is one of a kind that we should like to see brought out by our own scientific Societies. Under the title "Niels-Henrik Abel, sa vie et son action scientifique," it contains a full and most valuable sketch, by Prof. C. A. Bjerknes, of Christiania, of the writings and life of one of the ablest and acutest mathematicians of modern times. That the account is a full one will be evident when we say that the work occupies 365 octavo pages: it is a translation in French from the original memoir, and is further enriched by a considerable appendix. The labour of seeing the present form of the work through the press has principally fallen upon M. Houel, to whom the author warmly expresses his thanks. Abel was born at Findö, Christiansand, on August 5, 1802, and died near Arendal on April 6, 1829, and was interred at Froland.

The main body of the work consists of fifteen chapters, and the appendix occupies thirteen chapters more. His works, originally edited by M. Holmboe, the professor under whom he studied, were published in 1839, and quite recently a new edition was referred to in these columns. We give two or three extracts which show the appreciation of his powers amongst his contemporaries, an appreciation which has rather increased than decreased since his death. Jacobi writes of a *deduction* Abel had drawn as being "elle est au-dessus de mes éloges, comme elle est au-dessus de mes travaux." Legendre says, "il me tarde beaucoup de voir les méthodes qui vous ont conduit à de si beaux résultats; je ne sais si je pourrais les comprendre, mais ce qu'il y a de sûr, c'est que je n'ai aucune idée des moyens que vous avez pu employer pour vaincre de pareilles difficultés. Quelle tête que celle d'un jeune Norvégien!" Gauss expresses similar views, and on hearing of Abel's death, wishes for particulars of the life "de cette tête eminenmment distinguée." We could easily add other extracts from Prof. Bjerknes' admirable record of the distinguished Norwegian's life, which is a fitting companion to the before-cited edition of "the works," but forbear. Should any desire, with Gauss, to have his portrait, they will see here in the frontispiece the well-known, to some of us, lineaments.

Solid Geometry. By Percival Frost, D.Sc., F.R.S. Third Edition. (London: Macmillan and Co., 1886.)

IT would have been superfluous to recommend the third edition of Dr. Frost's "Solid Geometry," even if the third edition had been merely a reprint of the second. The book has now taken its position alongside the very best mathematical treatises in use, and requires "no bush." What we have got to do with, however, is no mere reprint: there is once again presented to us a notable increase of matter—much more than a cursory glance is likely to detect—and there has been considerable improvement generally. One change, greatly to the student's advantage, is the careful graduation of the problems at the end of each chapter, and the separation of them into groups. There is still a lack of references to original memoirs, and though, apparently, the author is conscious of it, he needs reminding that it is not sufficient merely to say that this or that is due to Cayley, Chasles, or any one else. Such incomplete statements serve only to give discoverers their dues; they do not assist the advanced and inquiring student.

We have pleasure in learning that an Appendix is about to be issued giving hints for the solution of the problems, but the pleasure is far more than counter-balanced by observing that the title-page bears no longer the words "Vol. I.," the inference being that Dr. Frost has followed the sad example of Thomson and Tait. There is no dearth of men willing and on the whole able enough to write mathematical text-books for beginners: those who could produce a volume to follow Frost's "Solid Geometry" are rare as white crows. When found—by press delegates—they should not only be made a note of, but coerced.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Flora of South Africa

No one, who ever spent much of the impressionable period of his youth at the Cape, that land of lowly plants with exquisite flowers,—but will be vividly interested in the masterly description of those plants' geographical regions of habitat, as given by Mr. Bolus, per your reviewer, "J. D. H.," on pp. 77-79 of your last week's issue.

But what can either one, or other, of those gentlemen mean by saying of "the Natal region" that it is "bounded on the east and south-east by the Atlantic"!

By the Indian Ocean if you like; and then you have a ready means of conveyance for those "Indian types of plants, both in genera and species" which the above-quoted authors say do abound along that eastern coast of South Africa,—but which the Atlantic could never have brought to it.

There is, however, a further local difference in the qualities of the two oceans, of such overwhelming importance to all vegetation, that I wonder no mention appears of it in a Cape botanical essay.

The Natal coast, for instance, on the east, is washed by a warm current from the equator, giving out so much steamy moisture that not only, as your article truly states, are there "the herbage- and bush- and tree-foliage greener, and the leafage larger" than elsewhere; but there, in that region of luxuriously fed, densely growing plants, does game abound; there do Kaffir tribes congregate and establish their kraals; and thereto do Dutch Boers emigrate out of the old, dried-up, southern colony, and found new republics; while therein have we also, every few years, to wage successive wars either with them, or with Zulus or Amakosi tribes of various power, until British lives have been sacrificed by thousands, and British money expended by millions.

But the west coast of South Africa, bless it, being washed by a cold current of the Atlantic coming from Antarctic seas, and giving out little or no vapour, even under a nearly vertical sun, —can hardly but be, even just as it is, an arid desert, where only a few starving Boschjesmen wander miserably up and down, existing perhaps on an occasional antelope, or roasted ants and stray locusts; and no one fights there for permanent possession of the ground.

Now all this egregious contrast between human society, as well as the flora, on the one side, *versus* the other, of Southern Africa (excepting some details dependent on the soil and the prevailing direction of the wind) are due to the Indian Ocean imparting to the air on the east coast an invisible, yet most potent quality which the Atlantic does not confer on the western coast. Could there then be found a more expressive emblazon, suitable to the present day, for a coat of arms for one of the flourishing new Governments on the eastern side of South Africa, than a wet, and dry, bulb hygrometer pictured with both bulbs marking 85° F., and with the surf of the Indian Ocean beating in the distance? C. PIAZZI SMYTH

May 29

The Composition of the Edible Bird's-Nest

As I have been much interested in the controversy concerning the composition of the edible bird's-nest, and particularly in the bearing of Mr. Green's investigations, which are given at length in your last issue (p. 81), would you permit me to give the result of some observations I made on this subject in the Solomon Islands. It will be remembered that it was the association of these nests with a so-called "fungoid growth" in the caves of North Borneo that led Mr. Pryer to consider that he had found the source of the material of which the nests are made, a supposed discovery which led to the re-opening of the controversy (NATURE, vol. xxx. p. 271). This low plant-substance was determined by Mr. George Murray to be the result of the growth of a microscopic alga, a species, probably new, of *Glœocapsa* (*Proc. Zool. Soc.*, 1884, p. 532).

In the Solomon Islands I was only able to obtain the edible nest in one locality (Oima Atoll) since the bird usually frequents inaccessible sea-caves and cliffs. The nests were of inferior quality, and were for the most part composed of fibrous materials derived from the vegetable drift (the husks of pandanus seeds especially). The gelatinous substance thickly incrusted the interior of the nests, and attached them to the rock. The surface of a cliff in the vicinity of the cave frequented by the swifts was coated by a reddish gum-like growth, which proved on examination to be an aggregation of the cells of a protophytic alga about 1/2500 of an inch in size. Unfortunately my specimens of this growth have miscarried, but I feel assured that it is very similar to that observed by Mr. Pryer in the Borneo caves, samples of which, through the kindness of Mr. George Murray, I had the opportunity of seeing at the British Museum. A similar growth is commonly to be found coating the coral-limestone cliffs in this group. It may be seen in all stages, the older portions being dark-coloured and rather tough, and the fresher portions being, as Mr. Pryer aptly remarked, like half-melted gum tragacanth. There are but few cells in the fresh alga, the mass being apparently composed of cellular debris, immersed in a rather diffluent material, the whole somewhat resembling the third section given in Mr. Green's paper.

That the salivary glands are especially concerned in the production of the gelatinous nest-substance there can now be but little doubt, and the investigations of Mr. Green have established the nature of its composition; yet it is possible, and I make the suggestion with great diffidence, that a *vegetable mucin*, or a substance closely allied to this animal product, may be found in these low plant-growths. H. B. GUPPY

95, Albert Street, N.W., May 29

"Arithmetic for Schools"

IN NATURE of May 20 (p. 51) there appears a criticism of my "Arithmetic for Schools," in which your reviewer states:—"In the purely arithmetical part of the book logical accuracy is attempted with considerable success. Want of grasp is much more evident in the part which deals with the applications. Then the division into subjects is strangely illogical, and slight inaccuracies of thought and language occur. Is it really the case, for example, that rate of interest (p. 181) is totally independent of *time*?" These are very serious charges to make against a book of the kind, and ought not to be made without very good reason. As your reviewer suggests the inferences (1) that the book is divided into parts, one of which contains the "pure arithmetic," and the other the "applied," and (2) that it is stated that *rate of interest is totally independent of time*, and as neither of these inferences has any foundation in fact, it seems only fair to myself that your reviewer should be asked to quote *verbatim* the other slight inaccuracies on which he bases his general statement.

Gonville and Caius College, May 24 JOHN B. LOCK

SUNSPOTS AND PRICES OF INDIAN FOOD-GRAINS

IN the volume of the *Bombay Gazetteer* which deals with the province of Kathiawar, there is at page 217 a long list of prices of the principal food-grains at Bhavnagar. The list contains, along with other information, the price of Indian millet for nearly every year from 1783 to 1882. This series of figures is long enough to afford the means of testing whether there is any tendency, in India, for times of scarcity, and consequent dearness of food, to recur after more or less regular intervals of years.

Ever since the discovery by Schwabe of the periodicity of the sunspots, and the further discovery by Sabine of the same periodicity in the variations of the earth's magnetism, there has been a growing belief in the minds of scientific men that the varying condition of the sun exerts a far greater influence on terrestrial affairs than is usually thought at all probable, and various investigators have traced, with more or less definiteness, a periodicity of eleven years—coinciding with that of the sunspots—in the variations of the rainfall, in those of the temperature and pressure of the atmosphere, and in the frequency of storms, &c. The late Prof. Stanley Jevons went so far as to express the opinion that even trade depressions are the remote effects of corresponding variations in the condition of the sun.

I am not aware that any attempt has hitherto been made to trace out any direct connection between the variations of prices in India and solar phenomena. The apparent hopelessness of the task has probably acted as a sufficient deterrent, for although it may be reasonable to suppose that solar variations influence the rainfall and other purely physical phenomena, yet it is well known that there are many causes of variation of price which cannot, with any show of reason, be attributed directly to the sun. Such, for instance, are wars, the gradual increase of the population, variations in the quantity of money in use, changes in the total volume of trade, &c. These circumstances complicate the problem very much, but it does not necessarily follow that it is hopeless to attempt to trace the possible influence of solar periodicity on the prices; for there are statistical methods by which most of the disturbing influences can be approximately, if not entirely, eliminated. Indeed, when these methods of elimination have been applied, it may be found that the solar periodicity is more decidedly traceable in the prices than in the rainfall: for, in the one case, the produce of every field exercises its due share of influence in determining the price; while, in the other case, the quantity of rain actually measured is but an infinitesimal portion of the whole quantity which falls, and may therefore very imperfectly represent the total rainfall over the whole of a district.

In considering a price in relation to the causes of variation to which it is subject, it may be thought of as divided into portions, each portion being assigned to its own particular cause. What is wanted here is to separate as distinctly as possible that portion which may be due to the variation of the influence of the sun from all the rest. But before any satisfactory attempt can be made to distinguish that portion of the price variation which may be due to variation of solar influence from the portion due to the average amount of solar influence and to other causes, it is necessary to adopt some standard of comparison which may reasonably be supposed free from solar effects of a periodically variable nature. Now as the physical state of the sun is known to go through a complete cycle of changes in a period of almost exactly eleven years, the average price for any consecutive eleven years

will be affected by the average amount of solar influence, and the difference between this average price and the actual price for the middle year of the eleven will be affected by the difference between the average condition of the sun and its actual condition in that middle year. This difference of price may also include the effects of other extraneous and non-periodic causes. Eleven years after the middle year just mentioned the sun will again be in its former condition, and a similar price difference for that year may be calculated. The same process may be carried on to the twenty-second, thirty-third, &c., years, and it will then produce a series of price differences equally affected by equal periodical solar influence. Non-periodic causes will, however, sometimes tend to unduly raise these price differences, sometimes to depress them, but on the average such disturbances will, in a long series of years, tend to balance each other, leaving the periodical portion of the solar influence outstanding. If, for instance, the years for which the calculations of the price differences have been made are those in which the sunspots are at a maximum, the average price difference will show how much prices tend to be raised or depressed by that condition of the sun which produces most spots. A similar series of calculations may be made for the years in which the spots are at a minimum, also for the intermediate years when the spots are increasing, and for those years when the spots are decreasing. A set of eleven average price differences, one for each year of the sunspot cycle of eleven years, will thus be obtained, and if, on arranging them in consecutive order, they show that prices are, on the average, decidedly high in those years when there are few sunspots, and decidedly low when the sunspots are numerous, or if they show any other decided and systematic variation in the sunspot period, the conclusion will be that the sunspot cycle does really affect the prices. If, on the other hand, the prices do not change in any systematic manner in the different years of the sunspot cycle, the conclusion will be against the hypothesis of a periodical variation of the prices corresponding to the periodical variation of the sunspots.

There is one point of view from which this method of taking differences is open to some objection. Suppose, merely for the sake of illustration, that the average price of millet throughout some particular sunspot period of eleven years is 50 pounds for a rupee, but that in the year of maximum sunspots the solar influence is such as to double the crop and lower the money price or raise the quantity price proportionately, that is, to 100 pounds for a rupee. The price difference for that year would be 50. If, however, by reason, say, of a more plentiful supply of money, the average price of millet for the whole of another sunspot period of eleven years is only 25 pounds for a rupee, and the crop in the year of maximum sunspots is, through solar influence, similarly doubled, the quantity price would only rise to 50 pounds, and the price difference would be only 25, although the solar influence, which is supposed to have produced the change, is the same as before. The difference between the two results would be due simply to the more plentiful supply of money, not at all to a difference of solar influence. This shows that it is needful to adopt some modification of the method, which will allow for gradual changes in the amount of money in use, and other similar causes of alteration of price. Such a modification will be made if, instead of taking price differences, the actual price of the middle year of the eleven is expressed as a percentage of the average price. Expressed in this way, the percentage for the year of maximum sunspots in each of the above examples would be 200, that is to say, in each case the number of pounds for a rupee would be 100 per cent. greater than the average number.

Table I. contains the Bhavnagar price list expressed in the percentage form in the manner just described. In the original table the prices are expressed in pounds for a rupee. If, therefore, the number for any year in Table I. is 125, it means that the number of pounds of grain for a rupee is 25 per cent. greater than the corresponding eleven-yearly average; and if the number is 75 it means that the number of pounds for a rupee is 25 per cent. less. In other words the excess above or the defect below 100 shows how much per cent. the number of pounds for a rupee is above or below the corresponding eleven-yearly average.

TABLE I.—*Percentages, Bhavnagar.*

Years.	1	2	3	4	5	6	7	8	9	10	11
1783 to 1793 . .	57	66	56	143	157	172	185	104	24	60	61
1794 to 1804 . .	66	69	140	163	145	131	108	82	78	75	63
1805 to 1815 . .	70	117	97	139	124	115	106	78	38	69	85
1816 to 1826 . .	118	159	162	103	68	81	78	111	71	51	74
1827 to 1837 . .	141	98	137	96	121	131	135	71	78	79	81
1838 to 1848 . .	91	58	100	92	81	141	164	93	86	107	66
1849 to 1859 . .	132	96	96	102	104	100	106	112	109	84	122
1860 to 1870 . .	103	100	103	86	52	62	87	105	104	92	84
1871 to 1881 . .	93	93	133	126	126	107	89	75	71	89	118
1882	100										
Means, including 1863 to 1866 .	97	95	114	117	109	116	118	92	73	78	84
Means, excluding 1863 to 1866 .	97	95	114	120	116	122	121	92	73	78	84
Smoothed means.	93	100	111	117	118	120	114	94	79	78	86

The numbers of Table I. are arranged in lines of eleven numbers each, so that the numbers occurring at equal intervals of eleven years, beginning with 1783, all fall in the first column, those occurring at equal intervals beginning with 1784, all fall in the second column, and so on. Now if there is any decided tendency for high or low prices to recur at more or less regular intervals of about eleven years, the great majority of the high prices should be found in a few contiguous columns in one part of the table, and the great majority of the low prices in a few contiguous columns in another part of the table. An examination of the numbers of Table I. shows that this is the case, for in columns 9, 10, and 11, no less than twenty-two out of the twenty-seven numbers are below 100, and only five of them are above 100; while in columns 3 to 7 the great majority of the numbers are above 100.

The average results are given at the foot of the table. They show that there is a decided tendency for years of high and low prices to recur, with some regularity, in a period of eleven years, five consecutive years being good years, when money prices are below the average; and the six following years being bad years, when money prices are above the average. The years which give the highest average money price, or the smallest number of pounds for a rupee, are those in column 9. The average number of pounds for a rupee in those years is 27 per cent. below the eleven-yearly average. The years which give the lowest average money prices are those in columns 4, 6, and 7. The average number of pounds for a rupee in those years is about 17 per cent. greater than the eleven-yearly average. There is thus an average difference of 44 per cent. between the years of low prices and those of high prices. This percentage difference would have been considerably greater if the prices had been reckoned in rupees for a fixed quantity of grain, instead of in pounds of grain for a rupee. To show that this is the case it is only necessary to convert the three prices 117, 100, and 73 regarded as pounds for a rupee, into their corresponding rupee prices, that is to say, into the number of rupees which would in each case be required to purchase 100 pounds. These are 0·85, 1·00, and 1·37 respectively,

giving a difference between the highest and lowest of 0·52, or no less than 52 per cent. of the average price, which is 8 per cent. more than the corresponding difference in the pound prices. Although this illustration by no means exhausts the question of the difference between quantity prices and money prices, it suffices for the purpose of guarding against the erroneous supposition that results worked out in quantity prices are directly applicable to money prices.

Having now found such remarkable evidence of regularly recurring periodical variations of the price of the staple food-grain at Bhavnagar, amounting on the average to more than 50 per cent. of the average money price, it seems desirable to inquire whether similar variations of price have taken place from year to year in other districts. For this purpose I have selected from the various volumes of the *Bombay Gazetteer* all those price lists which extend over periods of fifty years or more. These are for the districts of Ahmedabad, Kaira, Surat, Khandesh, Poona, Bijapur, Dharwar, Belgaum, and Kanara, and to these I have added Madras, for which station a long price list is given in the Report of the Indian Famine Commission.

The best way of testing whether any considerable portion of the variations of price in these districts can be regarded as regularly recurrent in a period of eleven years, corresponding to that of the sunspots, is to calculate the average eleven-yearly variation by the method already applied to the Bhavnagar prices. These calculations have been made. The results are entered in Table II. The corresponding average sunspot variation is also given.

TABLE II.

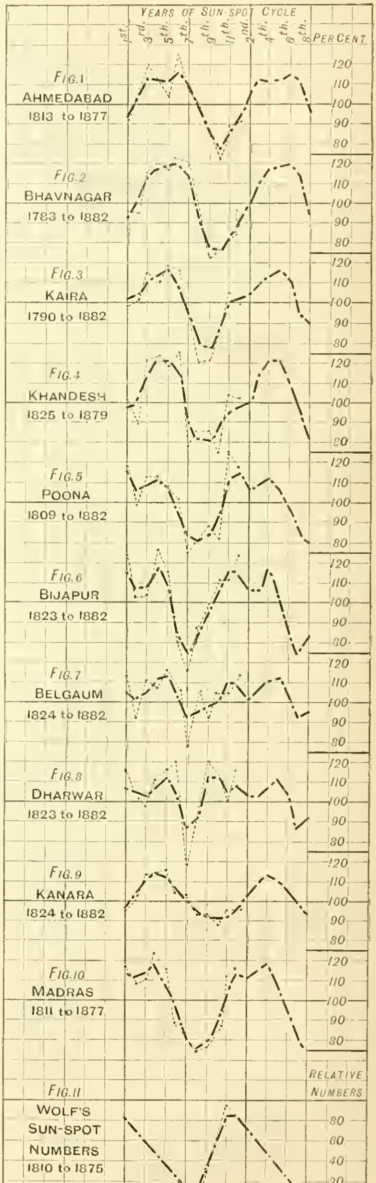
Districts.	Ahmedabad.	Bhavnagar.	Kaira.	Khandesh.	Poona.	Bijapur.	Belgaum.	Dharwar.	Kanara.	Madras.	Sunspots.
Years.	1813 to 1877	1783 to 1882	1790 to 1882	1825 to 1879	1809 to 1882	1823 to 1882	1824 to 1882	1823 to 1883	1824 to 1882	1811 to 1877	1810 to 1875
[1] 1 .	91	97	99	101	119	124	113	115	95	116	84·4
2 .	101	95	101	89	99	101	91	102	102	109	72·1
3 .	120	114	115	121	113	102	111	98	112	110	58·4
4 .	110	120	110	123	113	127	108	112	114	123	45·0
5 .	104	116	119	118	108	114	117	106	115	115	32·0
6 .	126	122	116	126	102	80	106	120	105	89	19·6
7 .	110	121	95	78	75	67	78	69	103	83	9·4
8 .	98	92	75	83	81	88	106	92	93	75	12·2
9 .	82	73	76	83	89	101	92	120	93	77	33·2
10 .	73	78	87	75	81	111	103	115	88	87	64·0
11 .	89	84	105	104	126	112	103	100	95	113	92·3

In calculating the average sunspot variation the sunspot numbers before 1811 have been excluded, partly because they are very much less reliable than the numbers for the later years, from lack of continuous observations, partly because the mean variation for the later years will be more directly comparable with the price variations, which, except in two cases, are deduced from the data of the years following 1810. In calculating the average eleven-yearly price variations the data for the years 1863 to 1866 have been excluded, because it is known that in those years prices were very much raised by the influence of the American war.

There is some irregularity in the eleven-yearly price variations (especially in those for Dharwar and Belgaum) which can hardly be attributed directly to the solar influence. The best way of removing this irregularity will be to take the means of each consecutive pair of the eleven

[1] The year 1871 is taken as the first year of the sunspot cycle of eleven

PRICES OF FOOD GRAINS IN INDIA
RECKONED IN POUNDS FOR A RUPEE.



average numbers which constitute the eleven-yearly price variation, and to repeat the process on the new means. This has been done, and the results are given in Table III.

TABLE III.

Districts.	Ahmedabad.	Bhavnagar.	Kaira.	Khandesh.	Poona.	Bijapur.	Belgaum.	Dharwar.	Kanara.	Madras.	Sunspots.
Years	1813 to 1877	1783 to 1882	1790 to 1882	1825 to 1879	1809 to 1882	1823 to 1882	1824 to 1882	1823 to 1883	1824 to 1882	1811 to 1877	1810 to 1875
1 .	93	93	101	99	116	115	105	108	97	113	83·2
2 .	103	100	104	100	107	107	101	104	103	111	71·7
3 .	113	111	110	113	109	108	105	102	110	113	58·5
4 .	111	117	113	121	112	117	111	107	114	118	45·1
5 .	111	118	116	121	108	109	112	111	112	110	32·1
6 .	116	120	111	112	97	85	102	104	107	94	20·1
7 .	111	114	95	91	83	75	92	87	101	82	12·6
8 .	97	94	80	82	81	86	95	93	95	77	16·7
9 .	84	79	78	81	85	100	98	112	92	79	35·6
10 .	79	78	89	84	94	109	100	112	91	91	63·3
11 .	85	86	99	96	113	115	105	107	93	107	83·2

These smoothed results are graphically represented by the dots connected with black lines in Figs. 1 to 10. To show the effect of the smoothing process the original unsmoothed numbers, viz. those of Table II., are graphically exhibited over the smoothed curves by the dots joined with faint dotted lines. It will be seen from these figures that the application of the smoothing process has got rid of almost all the irregularity. At the same time it has somewhat unduly reduced the range of the eleven-yearly variations. The amount of this reduction may be roughly estimated by applying the same smoothing process to the eleven average sunspot numbers given in the last column of Table II. This has been done in the last column of Table III. The results are curved in Fig. 11. The range of the original unsmoothed numbers is 82·9, that of the resulting smoothed numbers is 70·6; that is to say, the range of the smoothed numbers would have to be increased by 17 per cent. of itself to obtain the full range of the original numbers. From this it may be inferred that the range of each of the smoothed eleven-yearly price variations represented by Figs. 1 to 10 is too small, and should be increased by about 17 per cent. of itself to obtain the full range of the variation. On the other hand, the extreme range of the unsmoothed numbers will probably be somewhat too great in most cases, because the data do not extend over a sufficient number of years to eliminate completely the effects of casual fluctuations. The true mean range of the variation caused by solar influence will therefore probably lie somewhere between the range of the unsmoothed numbers and that of the smoothed numbers. The ranges of both the unsmoothed and the smoothed variations are shown below for each district. The range of each smoothed variation increased by 17 per cent. of itself is also given.

	Ahmedabad.	Bhavnagar.	Kaira.	Khandesh.	Poona.	Bijapur.	Belgaum.	Dharwar.	Kanara.	Madras.
Unsmoothed .	53	49	44	51	51	60	39	51	27	48
Smoothed . .	37	42	38	40	35	42	20	25	23	41
Smoothed, plus 17 per cent.	43	49	44	47	41	49	23	29	27	48

Now these results reveal the remarkable fact that, amid all the apparently irregular fluctuations of the yearly prices, there is in every one of the ten districts a periodical rise and fall of prices once every eleven years, corresponding to the regular variation which takes place in the number of the sunspots during the same period. They also show that in seven out of the ten districts the range of the eleven-yearly variation of prices lies between 40 and 50 per cent. of the average price, and that in the remaining three districts the range lies between 20 and 30 per cent. The ranges are greatest in those districts where scarcity and famine are most frequent, smallest in those which enjoy the greatest immunity in these respects. In Bijapur and the neighbouring districts of Belgaum and Dharwar the highest prices occur in the year of minimum sunspots; in Madras, Poona, and Khandesh a year or two later; in Kanara, Kaira, and Bhavnagar two or three years later; and in Ahmedabad three years later. The lowest prices occur in all the districts from three to five years after the year of maximum sunspots, that is to say, three years after at the southern stations; four or five years after at the northern. Bijapur and Poona are the first to show a very decided rise of prices, and this rise takes place in the year preceding the year of minimum sunspots. At all the other stations a very decided rise takes place a year or two later.

From what has been said it follows that the intervals of time between the year of minimum sunspots and the years of highest prices are less than the intervals between the year of maximum sunspots and the years of lowest prices. This shows that the eleven-yearly price variations do not exactly correspond to the eleven-yearly sunspot variation. The reason may be that on the occurrence of scarcity prices rise very rapidly, while on the return of a season of plenty they fall much more slowly, because the reserve stocks of grain consumed during a period of scarcity cannot be fully replaced until good crops for several successive years have been secured. If it were possible to obtain data showing the actual out-turn of the crops of each year, it would perhaps be found that the eleven-yearly variations calculated therefrom would correspond to the sunspot variation even more closely than the price variations correspond to it.

In estimating the significance of these eleven-yearly variations it must be remembered that quantity prices, not money prices, have been dealt with, and that the corresponding money prices would show a much greater percentage rise in dear times, and a less percentage fall in cheap times than are shown by the quantity prices. Indeed, to a person accustomed to thinking of money prices the quantity prices are apt to be very misleading if the difference is not constantly borne in mind, as may be seen from the consideration that if the quantity price, that is, the number of pounds for a rupee, becomes 50 per cent. less, that is dearer, than usual, the corresponding money price is 100 per cent. higher; while if the quantity price becomes 50 per cent. more, that is cheaper, the corresponding money price is only 33 per cent. lower. From a money point of view, therefore, a fall of 50 per cent. in the number of pounds for a rupee is much more serious than it seems to be, while a rise of 50 per cent. in that number is less advantageous than might at first sight be supposed. For financial purposes it would probably be best to convert the quantity prices at the beginning into their money equivalents, because it is impossible accurately to convert results (such as averages and the like) worked out in quantity prices into corresponding results, expressed in money prices.[1] Such conversions always give a too favourable appearance as regards cheap-

[1] For purely scientific purposes it would perhaps be best to work with the logarithms of the original prices, instead of with the prices themselves, regardless as to whether the prices are expressed in pounds for a rupee, or in rupees for a fixed quantity of grain. It would then be possible to pass directly from the results of one system to those of the other, without having to go through the labour of recalculation.

ness of food in times of plenty; and make the dearness of food in times of scarcity appear far less serious than it really is.

One of the most important practical results of this investigation probably is, that it affords a certain amount of power to predict the variations of prices in the coming sunspot cycle. Of course, until all those fluctuations which appear at present to be subject to no law have been explained and reduced to order, if ever that should be possible, exact prediction in any individual case is altogether out of the question, but as there is a regularly recurring eleven-yearly wave of prices running through the irregular fluctuations and following the sunspot wave in the manner defined by the curves, it is possible to form an estimate of the general level of prices in the different years of the coming sunspot cycle. There is thus some reason for believing that the present period of low prices following the last maximum of sunspots, which appears to have occurred about the end of 1882 or early in 1883, will not last very much longer, a brisk rise of prices being due in the Deccan and in Madras five years after the sunspot maximum, that is, in 1887 or 1888, and in more northern districts a year or two later.

This estimate will, of course, be subject to modification if it should be found that the sunspot curve is declining towards its minimum more or less rapidly than usual. The last period of sunspots appears to have been somewhat longer than the average, that is, about twelve years from the maximum of 1870 to that of 1882, instead of the normal length of almost exactly eleven years; and the coming minimum may possibly follow the last maximum more quickly than usual. Fortunately, the sunspot observations are not the only indicators of this cosmical periodicity, for, as I have shown in a paper communicated to the Royal Society in 1884, the magnetic observations recorded at the Colaba Observatory afford far smoother and more definite indications of this periodicity than the sunspot observations; and, what is even more important, the eleven-yearly magnetic variation precedes the sunspot variation by almost exactly six months, so that the magnetic indications are given half a year earlier than those of the sunspots. FREDERICK CHAMBERS

Bombay, April 1886

THE PHYSICAL APPEARANCE OF MARS IN 1886

A SERIES of observations of Mars were obtained here in March and April last with a 10-inch silver-on-glass reflector by With of Hereford. The powers employed were 252 and 475, but I found no advantage from the latter, which seemed too high for the purpose. As a rule a single lens magnifying 252 was amply sufficient, though there were several occasions when a power of about 350 would have been a decided acquisition.

The planet came to opposition on March 6, but during the first three weeks of March we had intense frosts, and it was not feasible to commence observations until towards the end of that month. The opposition magnitude of Mars was only 16″·6, so that as regards apparent diameter the planet was far from being favourably placed. At the opposition of 1877 the diameter was no less than 29″·5. But at the recent opposition the north hemisphere of the planet (which has not hitherto been so thoroughly examined as the south hemisphere, and does not exhibit so many striking features) was well presented for observation, the latitude of the centre of the disk being about 22° N. in March and April.

The markings seen were both numerous and diversified. There is evidently a mass of detail on the planet, which is, however, most difficult to trace out in reliable characters. Many faint lineaments reach the eye with sufficient distinctness to prove their existence, but they cannot be held steadily enough or with that perspicuity to allow of the delineation of their outlines, or to enable their relative positions to be correctly assigned. Only the more pronounced features can be drawn satisfactorily. The small diameter of Mars during the recent observations has in a great measure induced this uncertainty as to the physical aspect of the disk. Another cause is found in the rarity of really good telescopic images. Not only must the atmosphere be peculiarly favourable to sharp definition, but there must be an absence of wind. A complicated system of markings cannot be made out under the influence of annoying vibrations. Moreover, this planet, considered as a telescopic object, is far less satisfactory than either Jupiter or Saturn, and this circumstance, with the other drawbacks alluded to, have given rise to that uncertainty, and to many of the discordances, in regard to the visible markings observed on his surface.

My intention in the present paper is merely to describe general results, as a particular description would scarcely be intelligible without drawings. Between March 23 and April 30 the planet was examined on twenty-one evenings, and a considerable number of sketches were completed. During the period mentioned the weather afforded an unusual number of clear nights, and whenever the seeing was fairly good the visible features were carefully noted, the results being afterwards compared with each other and with former work in the same direction. My drawings correspond very closely amongst themselves, and there is a fair agreement in the main features with those depicted on the charts of Green, Schiaparelli, Knobel, and others. I have also compared them with the views given in Terby's work on Mars and with Boeddicker's drawings of 1881 and 1884 (with Lord Rosse's 3-foot reflector) published in the scientific *Transactions* of the Royal Dublin Society, and find in many instances a substantial confirmation. Some of the differences are larger than would have been considered probable, but experience has taught us that it is useless to expect uniformity in delineations of planetary details.

During the five weeks over which my observations extended I saw no conclusive evidences of physical changes in any of the markings. But the period was too limited, and the circumstances affecting the review altogether too unfavourable, to enable me to speak definitely on this point. The slight differences apparent amongst my drawings are merely such as were occasioned by changes in local atmospheric conditions. On a bad night faint markings, previously distinguished, would appear obliterated, and on thoroughly good nights I saw delicate appearances which were utterly beyond reach on less auspicious occasions. I am convinced that these changes in the character of the seeing, exercise great influence on the distinguishable features of a planet; more so, in fact, than observers usually concede. Inferences of real change are sometimes hastily adopted in consequence, but they can only be substantiated after the most searching examination and the most convincing proofs.

The exterior edges of many of the well-defined seas on Mars are very brilliant, and their boundaries very definite. These brilliant outlying borders remind one of the light areas often abutting on the dark spots of Jupiter, only in the case of Mars they are more extensive, more permanent, and altogether dissimilar in form. I may instance a particular case of this bright bordering in the immediate region east of the Kaiser Sea on Mars. On several occasions this was so striking as to vie with the bright patch about the north pole. This shimmering extends several degrees east of the dark outline of the sea, but is limited by a faint and irregularly-condensed marking extending northwards, with an inclination east, from the knot in longitude 290° just east of the north extremity of the Kaiser Sea as figured in Mr. Green's chart. This marking runs over a considerable tract, and its east extension underlies Davies' forked bay and Burton Bay, to both of which it is connected by faint ligaments of shade,

reminding one of the "canals" of Schiaparelli. This special marking, which is not included in Mr. Green's map, may be identical with the network of dark narrow streaks figured in this region by Schiaparelli in his chart for January and February, 1882. It is also more or less definitely shown in some other drawings, notably on one by Schmidt, which forms No. 17 in Dr. Terby's Areography.

As to the Kaiser Sea, it appears very faint and narrow, if not really broken, in the region some 10° or 15° south of its north extremity. This peculiarity is well drawn in Herr Boeddicker's drawings of December 27, November 19, and December 26, 1881 (Nos. 11, 13, and 14) in the scientific *Transactions* of the Royal Dublin Society for December, 1882. Consulting other drawings I cannot find that this feature is sufficiently indicated. It is obvious, however, that it would only be well detected when placed near the apparent centre of the disk as during the recent opposition.

Mr. Knobel's drawings in 1873 (*Monthly Notices*, vol. xxxiii., facing p. 476) agree generally with mine far closer than those he has published in the *Memoirs*, vol. xlviii. part ii., 1884. I always see Knobel Sea on Green's chart separated on its south side from the fainter curving band running east, as in the sketches Nos. 6, 7, 8, and 9, 1873. This break is not depicted in the subsequent drawings of 1884, so that the appearance has either been subject to actual variation of aspect or the difference of inclination has originated the want of uniformity. Probably the latter is the real cause, for the inclination of Mars in April and May, 1873, was nearly identical with that of March and April, 1886, and it is for these periods the drawings are so nearly alike in their more conspicuous forms. I see the northern boundary of Knobel Sea distinctly separate from the dark longitudinal strip immediately contiguous to the north polar cap. The drawing No. 12, May 19, 1873, by Mr. Knobel, portrays the leading features of this region much as I have more recently observed them. In 1884, Mr. Knobel delineated the whole mass of shading outlying the north pole as blended uninterruptedly, but these differences are unquestionably due to the changes of inclination, which must necessarily introduce such discordances into the apparent forms of the markings as observed at different epochs.

As to the canal-shaped features of Schiaparelli, first seen in 1877 and 1878, and subsequently confirmed, I have distinguished a large number of appearances highly suggestive of such a configuration, but the Italian drawings made during the three months from October, 1881, to February, 1882, give them a definite character, and (apart from their duplication) a straightness of direction and general uniformity of tone which my observations do not confirm. The more delicate and complex markings on the planet appear to my eye, under the best circumstances, as extremely faint, linear shadings with evident gradations in tone and irregularities occasioning breaks and condensations here and there. If they existed under the same aspect and with the same boldness as delineated by Schiaparelli, they would have been readily detected here whenever the seeing was fairly good, for these objects are referred to as readily observed in the 8-inch refractor of the Milan Observatory in February, 1882, when the planet's diameter was only 13″. The duplication of these lines was also traceable under the same unfavourable conditions. The wonder is, not that the eminent Italian astronomer has discovered such a marvellous extent of curious detail on this planet, because this detail unquestionably exists, though scarcely in the form and character under which it is represented, but that he should have observed its more complex and difficult configuration at the very period when Mars was so very unfavourably situated for observations of this critical nature.

The surface markings of this planet are so numerous and varied that they are far from being adequately represented on existing charts. In certain regions the disk is so variegated as to give a mottled appearance. Dark lines, and spots, and bright spaces are so thickly interspersed, and so difficult to observe with sufficient steadiness to estimate their positions and forms, that I found it impossible to make thoroughly satisfactory drawings. An observer has to be content with endeavours to depict the more prominent marks only, and even in connection with these there is always some element of uncertainty. The rotation-period of the planet is, however, so slow, the hourly rate being only 14°·6 in comparison with 36°·7 in the case of Jupiter, that plenty of time is afforded for drawing the leading markings before they show a displacement obvious to the eye. In addition to this a drawing of Mars may be made to rest on several successive evenings of observation if the observer comes 37·4 min. later to the telescope on each occasion. In regard to Jupiter the difficulty of suitably drawing the details is far greater, though they admit of more ready observation. The rapid rotation of this planet displaces objects in a few minutes, and makes it imperative that the work both of observing and charting should be very hastily performed; and it is not feasible in this case to base a sketch on observations of following nights, because the markings are influenced by different velocities, and suffer large relative displacements even at short intervals of time.

During the past few months the north polar cap of Mars has been very bright, sometimes offering a startling contrast to those regions of the surface more feebly reflective. Some of the other parts were also notably brilliant. These luminous regions of Mars require at least as much careful investigation as the darker parts, for it is probably in connection with them that physical changes (if at present operating on the planet's surface) may be definitely observed. In many previous drawings and descriptions of Mars sufficient weight has not been accorded to these white spots.

Many of our leading treatises on astronomy attribute a dense atmosphere to Mars, but nothing has been observed during my recent observations to corroborate this theory. It seems to me far more plausible to assume that the atmosphere of this planet is extremely attenuated. The chief spots are invariably visible, and the phenomena occasionally observed are rather to be imputed to the vagaries of our own atmosphere than to that of Mars. Jupiter and Saturn are doubtless enveloped in dense vapours shrouding their real surfaces from terrestrial eyes. Their markings are atmospheric, though in some cases very durable, and constantly undergoing changes of aspect and displacements of position by longitudinal currents. On Mars a totally different nature of things prevails. Here the appearances descried are absolute surface markings displaying none of the variations which are so conspicuously displayed in the markings on Jupiter. It is probable that many, if not all, the changes supposed to have occurred in the features of Mars are simply attributable to the constantly varying conditions under which the planet has necessarily to be observed. Were the circumstances of observation more equable there would be much greater unanimity amongst observers of this interesting object. It seems to me that the very pronounced character of the markings and their great permanency are quite opposed to the idea that the planet is surrounded by a dense cloud-laden atmosphere.

W. F. DENNING

M. CORNU ON THE HYDROGEN FUNCTION OF CERTAIN METALS[1]

WHEN we examine on different photographs those groups of lines which reappear periodically with a particular regularity, we find that these groups belong

[1] Translation from an article in the *Journal de Physique*.

precisely to the category of those which reverse themselves; for some are reversed and the others are on the point of being so. For the same metals, the reversals are more or less complete according to the conditions of the experiment, and for different metals according to their chemical and physical properties.

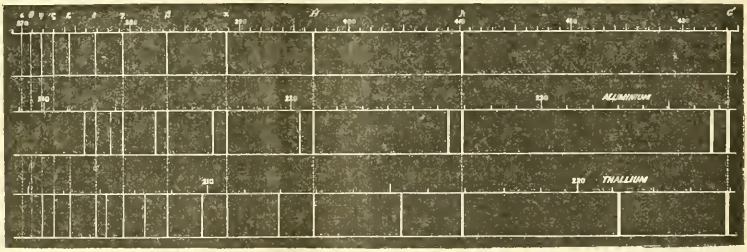
The law of distribution of these groups presents another common character relatively to the succession of distances and intensities: the lines get nearer together towards the more refrangible end and diminish in intensity. This character is much the more striking when the number of reversed lines is considerable, because the field on which they appear is more uniform. It seems that with the elevation of temperature the spectrum tends towards a limit, that of a continuous brilliant background despoiled of all lines except the regular series of the self reversing ones. It is to this constitution that I wish to draw the attention of observers.

The number of metallic spectra capable of giving a regular series of spontaneously reversed lines on a continuous background is considerable; but the most beautiful series that I have observed were supplied by two metals which one could scarcely have anticipated, from a chemical point of view, to find side by side; these are aluminium and thallium, whose equivalents are at the extremity of the list of those of the simple bodies. The diagram gives an idea of the distribution of these reversed lines; one sees that they form in each spectrum a series of doublets fulfilling the conditions of distance and intensity given above.

I shall not stop to indicate the fruitless trials of numerical calculations that I have taken in hand in order to represent each of these series by the substitution of the series of entire numbers in a simple function; I may add that I had given up these researches until the discovery of Dr. Huggins on the spectra of white stars brought back my attention to this subject.

These spectra present, in fact, a common series of dark lines, that is to say, reversed, fulfilling precisely the conditions of distance and intensity which characterise in metallic spectra the spontaneously reversed lines: they prolong the series of well-known lines of the spectrum of hydrogen, C, F, G, *h*. One could then foresee that the whole series belonged to them; that is what has since been confirmed by Vogel, though this result is still not quite certain. The interest of this identification was such that I sought to prove it myself, which I could not realise till lately. The experiment is not without difficulty; but in taking more minute precautions to get rid of all impurity in the hydrogen, I have seen the impurity lines obliterated, and finally I succeeded in obtaining photographs showing the series of star-lines in all their purity.

The spectrum of hydrogen is placed on the first line in the above diagram: the comparison has been rendered easier by the choice of scales showing intuitively the identity of the law of distribution of lines in the three spectra.



DESCRIPTION OF THE DIAGRAM – The graduations define the lines according to their wave-lengths. The first line represents the dark lines of the violet and ultra-violet spectra of the white stars. The second represents a double series of inverted lines in the ultra-violet spectrum of aluminium (electric arc). The scale of the drawing has been chosen in a manner to make G and δ coincide with the homologous lines of the first series (first line of each doublet). One could have operated in the same way with the second series (second lines). This mode of representation advantageously replaces the numerical tables, showing the verification of the two empiric formulas—

First series ... ... $\lambda_1 = 47\cdot30 + 0\cdot43783\, h$
Second series ... ... $\lambda_2 = 47\cdot18 + 0\cdot43678\, h$

which give the length of the wave of each line in function of the wave-length h of the corresponding line of hydrogen; the difference between the calculation and the observation is of the order of the experimental errors. The third line represents a double series of inverted lines in the ultra-violet spectrum of thallium (electric arc). The scale of the drawing was chosen like the one above; the empiric formulas which represent these two series are:—

$$\lambda_1 = 94\cdot61 + 0\cdot29776\, h$$
$$\lambda_2 = 111\cdot31 + 0\cdot75294\, h.$$

We might compare in the same way the more complex groups, like magnesium, zinc, sodium, &c.; the only difficulty is to establish the agreement of the groups; we do this immediately by a quite simple graphic construction. We arrive at the following statement, which resumes the whole of my researches. In the metallic spectra certain series of lines, spontaneously reversed, present sensibly the same law of distribution and intensity as that of the hydrogen lines.

It is not necessary to dwell on the importance of this relation: it makes evident the existence of a law which is general relatively to the emissive powers of incandescent vapours, and, again, it shows that this law of succession of spectral lines, common to so many series, seems to be expressed by the help of the same function, which one might call the hydrogenic function, which should play the principal part in these studies: the result then appears to constitute a first step towards the solution of the great problems which the spectroscope brings on for solution.

R.

VEGETATION OF SOUTH GEORGIA

ON Tuesday, January 17, 1775, Capt. Cook landed on this remote island, which is situated about 1000 miles east of Cape Horn, in about 54° S. lat. and 37° W. long., and took possession of it in the name of King George the Third, after whom he named it. Capt. Cook landed in three different places, and the ceremony of adding the island to the British dominions, he informs us, was performed under a waving of colours and a discharge of small arms. Whether any British subject has ever set foot on it since that day I know not; but the description of the island by its famous discoverer was not likely to tempt any one to go out of his way with that object in view. Although

lying only as far south of the equator as York is north of it, South Georgia is covered, in the higher parts at least, with permanent snows and glaciers, and is altogether of a most wild and desolate aspect. Large masses of ice were continually breaking off from the perpendicular cliffs and falling into the sea with a noise like cannon. "The inner parts of the country," says Cook, "were not less savage and horrible. The wild rocks raised their lofty summits till they were lost in the clouds, and the valleys lay covered with everlasting snow. Not a tree was to be seen, nor a shrub even big enough to make a toothpick. The only vegetation we met with was a coarse strong-bladed grass growing in tufts, wild burnet, and a plant like moss, which sprung from the rocks."

Animal life, however, was more abundant. Seals were plentiful, and the penguins the largest ever seen by Cook; some which were taken on board weighed from twenty-nine to thirty-eight pounds. Eight kinds of "oceanic birds" are enumerated, and one, a yellow bird, was found to be delicious food. All the land birds observed were "a few small larks." From Cook's narrative it appears that Forster, the botanist, was one of the landing party, hence it might have been expected that few flowering plants would have escaped observation, especially as the visit was made in January, the midsummer of the southern hemisphere. Forster himself states ("Observations made during a Voyage round the World," p. 16) that South Georgia is an isle of about eighty leagues in extent, consisting of high hills, none of which were free from snow in the middle of January, except a few rocks near the sea. And he adds that there was no soil except in a few crevices of the rocks.

No further information respecting this island has been published, so far as I am aware, until since the return of a recent German Expedition, which made the island one of its stations for meteorological and other observations. When collecting the materials to illustrate the flora of the very much broken coldest southern zone of vegetation for the "Botany of the *Challenger* Expedition," I had to be content with Cook and Forster's very meagre accounts of South Georgia; but from the published northern limits of drift ice in different longitudes in the southern hemisphere, it was not expected that South Georgia possessed much more than the scanty flora they attributed to it, though Macquarie Island, in the same latitude, and nearly in the longitude of New Zealand, was known to support a comparatively luxuriant vegetation. Dreary and barren as it is, however, South Georgia is not so bad as it has been painted. The officers of the German Expedition spent nearly a year on the island, and appear to have explored it thoroughly, botanically and otherwise. During this period the atmospheric pressure was subject to extraordinary fluctuations, the extremes exhibiting a difference of 64 millimetres, or a fraction over $2\frac{1}{2}$ inches, while the range of temperature during the same period was only 48·6 Fahr., or in round numbers, from 8° to 57° Fahr.; thus showing the freezing-point to be nearly midway in the range. The actual mean temperature of the year was 35°·06 Fahr.; of June, the coldest month, 25°·6 Fahr.; and of February, the warmest month, 41°·6 Fahr.

With regard to the flowering plants collected in the island by Dr. Will, one of the officers of the Expedition, we are indebted to Dr. Engler for an enumeration of them in his *Jahrbücher*, vol. vii. p. 281. They are thirteen in number, and their general distribution is so extremely interesting that I may be pardoned for giving it in detail:—

(1) *Ranunculus biternatus*, Sm. (Ranunculaceæ).—Fuegia, Falklands, Tristan d'Acunha (?) Marion, and Kerguelen Islands.

(2) *Colobanthus subulatus*, d'Urville (Caryophyllaceæ).—Fuegia, Campbell's Island, New Zealand, and Alps of Victoria, Australia.

(3) *Colobanthus crassifolius*, d'Urville (Caryophyllaceæ).—Fuegia and Falklands.

(4) *Montia fontana*, L. (Portulaceæ).—Fuegia, Marion, Kerguelen, Campbell's Island, and widely diffused.

(5) *Acæna adscendens*, Vahl. (Rosaceæ).—Fuegia, Marion, Crozets, Kerguelen, Macquarie Islands, and New Zealand.

(6) *Acæna lævigata*, Ait. (Rosaceæ).—Fuegia.

(7) *Callitriche verna*, L. var. (Halorageæ).—Fuegia, Marion, Kerguelen, Heard Islands, New Zealand, and widely diffused.

(8) *Juncus novæ-zealandiæ*, Hook. f. (Juncaceæ).—New Zealand.

(9) *Rostkovia magellanica*, Hook. f. (Juncaceæ).—Andes, Fuegia, Falklands, and Campbell's Islands.

(10) *Aira antarctica*, Hook. f. (Gramineæ).—Fuegia, Falklands, South Shetlands, and Kerguelen Island.

(11) *Phleum alpinum*, L. (Gramineæ).—Magellan's Straits, and widely dispersed in the cold regions of the northern hemisphere.

(12) *Festuca erecta*, D'Urville (Gramineæ).—Fuegia, Falklands, and Kerguelen.

(13) *Poa flabellata*, Hook. f., syn. *Dactylis cæspitosa*, Forst. (Gramineæ).—Fuegia and Falklands.

From the collector's remarks, appended by Engler to each species, it appears that some of the foregoing plants flourish luxuriantly in South Georgia, especially the species of *Acæna* (the burnet of Cook's narrative), and *Aira antarctica* and *Poa flabellata*. The *Ranunculus* was abundant by the side of a stream and elsewhere, and *Colobanthus subulatus* (doubtless the moss-like plant mentioned by Cook) formed large tufts on the south side of the hills. Nine out of the thirteen plants in South Georgia are also found in the eastern part of this southernmost zone of vegetation from Kerguelen to New Zealand, taking these islands together. One, *Juncus novæ-zealandiæ*, had not previously been found in what may be termed the American part of the zone; but, as Prof. Buchanan, to whom Dr. Engler submitted the South Georgian specimens, remarks, this is so nearly allied to the South American *Juncus stipulatus* that it may be cited as another instance of representative and closely-allied species in the American and Australian regions.

Thus are we gradually obtaining a knowledge of the vegetation of the detached fragments of the Antarctic flora; yet several islands are still quite unknown botanically or only very imperfectly. Concerning Diego Alvarez, or Gough Island, situated about 4° south of the Tristan d'Acunha group, we know nothing except that the vegetation is said to be similar to that of Tristan d'Acunha, and to include *Phylica nitida*, the only arboreous member of the latter flora. Then there is a group of islands, including Lindsay, Bouvet, and Thomson, in about the same latitude as South Georgia, but 35° eastward, of which nothing is known botanically.

W. BOTTING HEMSLEY

NOTES

THE Visitation of Greenwich Observatory takes place on Saturday next.

THE Ladies' *Soirée* at the Royal Society takes place on the evening of Wednesday, the 9th inst.

THE honour of C.M.G. has been conferred on Mr. Charles Meldrum, Director of the Royal Alfred Observatory, Mauritius.

THE explosion of the 43-ton gun has led to the appointment of a Committee of Inquiry, in which the name of Mr. Anderson is conspicuous by its absence, although surely no greater authority on the points at issue exists. A year ago, in his important lectures at the Society of Arts, he drew attention to the want of relation between the sections and pressures, and predicted disasters.

Sir Bernhard Samuelson, M.P., and Mr. Philip Magnus, of the City Guilds of London Institute, have been appointed by the Education Department English representatives at the International Congress on Technical Education, to be held at Bordeaux in September next.

Prof. Flower, the Director of the Natural History Department of the British Museum, has allowed the zoological collections made by Brigade-Surgeon J. E. T. Aitchison, C.I.E., the naturalist lately attached to the Afghan Delimitation Commission, to be placed on view temporarily at the South Kensington Department. To those interested in the zoology of those regions and in the geographical range of species, a view of these collections in their entirety will be found most interesting. We believe that at an early date this collection will be broken up to be sent to India, and distributed to various museums and countries, and that it is only localised here until such time as a report on its details is furnished to the Government of India.

Mr. Nicholson has been appointed Curator of Kew Gardens, in the room of Mr. Smith, resigned. Mr. Nicholson has been one of the chief assistants at the Gardens for some years.

A series of Conferences on the "Mineral Resources of the Colonies and India" will be held by the Geologists' Association in the Colonial and Indian Exhibition on Saturday afternoons, commencing at 3 p.m. After the reading of the paper there will be a discussion, terminating at 4.30. The Conference will then adjourn to the Courts, where further explanations of the exhibits will be given. The first meeting will be on Saturday next, when an address will be given on the Mineral Resources of India and Burmah, by Prof. V. Ball, F.R.S.; Sir Richard Temple will preside. The arrangements for succeeding Saturdays are as follow:—June 19, South Africa, by Prof. T. R. Jones, F.R.S.; Sir Ch. Mills in the chair. July 3, Canada, by Dr. A. R. Selwyn; the Marquis of Lorne in the chair. July 17, New Zealand, by Dr. J. Von Haast. July 24, Australia, by Mr. F. W. Rudler. There will probably also be a lecture by the President of the Geologists' Association (Mr. W. Topley), on the Coaling Stations in Relation to the Fuel Deposits of the Empire; but the date of this is not yet fixed. Conferences of the Anthropological Institute on the Races of the British Empire will also be held in the Conference Hall of the Colonial and Indian Exhibition. The first was on Tuesday on the Races of Africa. The others are:—Monday, June 7: Races of America (West Indies). Tuesday, June 22: Races of Australia. Tuesday, June 29: Races of New Zealand, Fiji. Tuesday, July 6: Races (Aboriginal) of India. Tuesday, July 13: Races of Ceylon, Straits Settlements, Borneo. The chair will be taken at 4 p.m. The memoirs read and discussed in the Conference Hall will be illustrated by selections from the exhibits. Afterwards, but not later than 5 o'clock, the Conference will adjourn to the Courts, and there inspect and hear explanations of the remaining exhibits connected with the subject of the day.

The Lick Trustees have decided to purchase from Messrs. Feil and Mantois a 36-inch crown disk, which was made by them at the same time with the crown disk of the objective now in the hands of the Clarks. The Clarks "have received the order to figure this disk as a third (photographic) lens for the large objective."

A curious phenomenon, the *Scotsman* reports, was witnessed at Stonehaven on Sunday afternoon, May 23. At intervals, just before and after high tide, without any apparent cause, the water along the coast rose and fell from 10 to 18 inches at a time, the subsidence leaving as much as 15 to 18 feet of the beach dry. The disturbance continued for three hours, commencing at about half-past 4 o'clock. There was no wind, and the sea was quite smooth, but the water advanced and retired with a speed equal to the run of a large river during a spate, and caused so much commotion in the harbour that the fishermen had to secure their boats with extra moorings to prevent damage being done. Indeed, it is seldom that there is so much commotion in the harbour, even during stormy weather. It is surmised that the phenomenon was due to some eruption or subsidence in the sea bottom.

The Executive Committee of Aberdare Hall, Cardiff, has issued a most satisfactory report of the progress of Aberdare Hall during its first term. Seven students were entered when this Hall for lady students was opened in October 1885. Two of these are studying for the Intermediate Science Examination (London University), one for the Intermediate in Arts, and four for the Matriculation Examination. Two scholarships tenable at Aberdare Hall were awarded. At the beginning of the next session several large scholarships and many exhibitions will be offered for competition at University College, Cardiff, and three exhibitions tenable at Aberdare Hall. The institution deserves every encouragement.

The New York Assembly has passed the Bill providing for the appropriation of 20,000 dollars annually to the Metropolitan Museum of Art and the American Museum of Natural History, in order that they may be kept open to the public, free of charge, on Sundays. It is expected that it will soon be favourably reported by the Senate Committee, and become law.

Up to Saturday morning the accounts from Catania were reassuring; the flow of lava was much slower and was rapidly cooling, and Nicolosi was considered almost safe. But at 4 p.m. a fresh outpour of lava manifested itself, and flowing over the earlier stream which had partially hardened, it again menaces Nicolosi and Belpasso. At 9.15 p.m., the lava stream which threatens Nicolosi showed a front 180 metres wide, from 6 to 10 metres high, and was moving at the rate of 10 metres per hour. According to latest reports the eruption is as active as ever.

On April 28 a lovely mirage was seen at about noon at Östersund, in Northern Sweden. In the south-west, above the Storsjö, a great lake, the lofty Oviks Mountains, covered with snow, were seen reflected on the sunlit clouds. Below them was a dark broad belt of forest sloping down to an ice-covered lake, in which some woody islands could be seen. At the beginning the western sky was clear, but gradually a dark bank of clouds rolled up, at last obscuring the mirage, but it reappeared several times when the sun broke through.

Mr. Penhallow, who has resided for some years in the service of the Japanese Government in Yezo, contributes to the last number of the *Canadian Record of Science* an article on the physical characteristics of the Ainos. Referring to the many contradictory reports as to the great hairiness of the Yezoines, his conclusion is that, although there are many exceptions, they generally possess a more than ordinarily hairy body, enough so at least to make them deserve the epithet of "hairy Kuriles." The bushy appearance of the hair and beard is doubtless due as much to the fact that the men never shave and seem rarely even to clip their beards, as to any natural excess of growth. The Aino of Saghalien offers a striking departure from the rule of hairiness which essentially characterises the Yezoine; and this would appear therefore not to be a race characteristic, but to be due to the peculiar and widely different conditions of life, dress, and exposure to which these people have been subjected. From

a considerable number of measurements, Mr. Penhallow summarises the physical characteristics of the Ainos as follows:—The forehead is usually high, though narrow; eyebrows heavy and overhanging; nose somewhat inclined to flatness, though but little more so than in Europeans; mouth wide, but well formed; chin well formed and medium size; eyes straight, brown, and dull; cheek-bones inclined to be prominent; facial angle high, the mean of the measurements giving an angle of 72°; the body is compact, well built, and muscular; much more than ordinarily hairy, skin of light colour, comparable to that of Europeans, and the average height is about 5 feet 2 inches.

THAT frogs have a formidable enemy in the common mouse is evidenced by the following incident. A correspondent, Mr. W. August Carter, of South Norwood, states that he observed, a short time since, several mice pursuing some frogs in a shed which was overrun with these reptiles. The alacrity of the latter, however, rendered the attacks of the mice futile for a considerable period. Again and again the frogs escaped from the clutches of their foes, but only to be recaptured, severely shaken, and bitten. The energy put forth by these reptiles was so great that they actually swayed their captors to and fro in their efforts to wrest themselves from their grasp. At length the wounds inflicted upon them rendered the frogs incapable of further resistance, and they were easily overpowered by the mice, which devoured a certain part of them.

IN a lecture recently delivered before the Scientific Society of Bamberg, Dr. Hartwig, the Director of the new Astronomical Observatory there sketched out the future work of that institution. It was well, he said, that an observatory should devote itself to some specialty, with which its name would be associated, as that of Paris was with the determination and mapping of the fixed stars, Greenwich with the movements of the moon, Vienna with comets, and so on. In a similar way Bamberg would occupy a certain limited field, and labour therein. In the first place it would undertake the systematic investigation of the parallaxes of the fixed stars, a work which had already been partially performed at the Cape Observatory for the southern, and at Newhaven in the United States for the northern hemisphere. Bamberg will be provided with a new 7-inch heliometer, the largest of its kind at present in the world, although the Cape Observatory will shortly be provided with one of the same size. The present Cape heliometer is a 4-inch, and that at Newhaven a 6-inch one. Dr. Hartwig said that this 7-inch heliometer is at present the finest instrument known to astronomy. He pointed out that at present the parallaxes of only eight or ten fixed stars were calculated, while about three thousand remain to be done, and this, he said, would take a single qualified observer more than thirty years to accomplish. He hoped that as Leipzig and Göttingen were about to be provided with heliometers, they would participate in the work, so that in a comparatively short time we may obtain a more accurate notion of the distance of many fixed stars and of their grouping in space. Another work which Bamberg would undertake is the investigation of the physical libration of the moon—a problem that has been studied at Königsberg since 1845, and in Strasburg since 1870. After describing at some length the instruments with which the new Observatory is provided, Dr. Hartwig concluded by assuring his hearers that with these an observatory would be established which would take a high place amongst existing astronomical institutions, and which would be excelled in Germany by the Observatories of Strasburg and Potsdam alone. The Bamberg Observatory, it should be stated, owes its existence to the munificence of a private individual, the late Dr. Remeis, a member of the Scientific Society of Bamberg.

THE *Darling Downs Gazette* of March 20 describes some recently discovered caves fifteen miles from Rockhampton, Queensland. A party, headed by Mr. W. M'Ilwraith, of the Rockhampton Natural History Society, recently visited the caves. From some wells on the route they saw the peaks of an uncommon range of hills. "They stand up in a fine sharp profile like the pinnacles and turrets of a stately Gothic pile. The vestibule of the wonderful structure is formed by an immense chasm in the rocks. Two walls of limestone or marble rock set in an acute angle rise on either side to a height of about 60 feet, and converge in front at a higher elevation. At 9 o'clock at night the party began exploring, and after clambering over a mass of detached, sharp-edged, pock-pitted rocks, got into a rocky chamber. Its walls were beautifully white in parts, and show the rock to be of limestone formation. They visited in succession caves of different dimensions, and named one the 'Chinese Joss-house.' It is a little recess off the passage: the walls are beautifully white, and stalactites and stalagmites unite to form beautiful pillars, the whole being wonderfully beautiful, reminding the visitors of Chinese ivory carved work. In the morning they continued their exploration, wandering through numerous passages, and crawling and slipping till they came to a large cavern. In one of the passages the bats extinguished their candles, and they returned to the upper regions. They then saw daylight streaming from the opposite side of the mountain, and estimated the distance from light to light at five chains or more. They returned to the starting-point, climbed a ladder, and traversed other passages, and crossed a gulf on a bridge formed of saplings. Eventually they reached a wide opening, and the light poured in from an opening in the caves. This latter is a large chamber, and in it are the roots of a tree, which have taken hold in the bottom of the cave, and hang like ropes. The most striking stalagmites in it resemble the head of an elephant and the bust of a man. Various caves were discovered, and also openings leading from one main suite of caves to another one. The cave particularly alluded to is called 'The Cathedral.' It is 50 feet long from the porch to the pulpit stairs, 30 feet across, and the ceiling is so lofty that the gleams of the candle did not reach it. There are stalactitic formations on the ceilings and floor, but the walls are plain, and have niches in some parts. Some of the party descended 60 feet here, and in another failed to reach the opening. The writer says, 'Wherever we went almost underground our footsteps had a hollow sound, and the conclusion we come to at present is that the region has been a hot-spring area, and the caves were formed by the action of hot water.'"

THE various species of Salmonidæ hatched out and reared by the Buckland Museum authorities have been turned into the Thames at Penton Hook, with a view of replenishing the stock of fish in that river. The Thames Angling Preservation Society are making arrangements to receive a consignment of land-locked salmon fry at their nursery again this year, in order to rear them for the Thames. The exertions now being made to re-stock the unpolluted portions of this river are sure to terminate in good results, indeed many of the trout taken lately are said to be the result of previous efforts made by pisciculturists in this direction.

ORNITHOLOGISTS, antiquarians, and librarians will in a few days have the opportunity of possessing a book which is said to be the only work published on the subject of duck decoys. It will be in quarto, with many illustrations, coloured and woodcut. Its author, Sir Ralph Payne-Gallwey, is already known to naturalists by his book on wild-fowl, issued some few years since by the publisher of the present volume, Mr. Van Voorst.

DR. VON HAAST writes that the large geological relief model of New Zealand, referred to in our recent article on the Colonial

and Indian Exhibition, has been prepared by Dr. James Hector, the Director of the Geological Survey of New Zealand, and forms part of the large exhibit of that gentleman. There are several large labels inside the glass case, in which the necessary explanations are given.

THE additions to the Zoological Society's Gardens during the past week include a Ring-tailed Lemur (*Lemur catta*) from Madagascar, presented by Mr. Angus Ogilvy ; two Black-tailed Parrakeets (*Polytelis melanura*) from South Australia, presented by Mr. James Thomson ; an Indian Cobra (*Naia tripudians*) from India, presented by Messrs. H. Thwaites and V. A. Julius ; a Common Viper (*Vipera berus*), British, presented by Mr. W. H. B. Pain ; a Loggerhead Turtle (*Thalassochelys caouana*) from the Atlantic Ocean, presented by Mr. R. G. Fraser, R.N. ; a Rook (*Corvus frugilegus*), British, presented by Mr. H. J. Peckover ; a Black-faced Spider Monkey (*Ateles ater*) from Eastern Peru, a Crab-eating Raccoon (*Procyon cancrivorus*) from West Indies ; an Indian Cobra (*Naia tripudians*) from India, deposited ; two Spotted Hyænas (*Hyæna crocuta*) from South Africa, two Side-striped Jackals (*Canis lateralis*) from West Africa, a Griffon Vulture (*Gyps fulvus*), a Smooth Snake (*Coronella lævis*), a Viperine Snake (*Tropidonotus viperinus*), European, purchased ; two Triangular Spotted Pigeons (*Columba guinea*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

A CATALOGUE OF "COMPARISON" STARS.—Dr. N. M. Kam of Schiedam has published in *Verhandelingen der Koninklijke Akademie van Wetenschappen*, Deel. xxiv. (Amsterdam), a star catalogue compiled from the places of stars determined by meridian observations, which have been extracted from vols. i. to lxvi. of the *Astronomische Nachrichten*, and reduced to the epoch 1855·0. The positions of the stars contained in this catalogue were determined in connection with observations of planets and comets, and it was in compliance with Argelander's express desire that the work of collecting them and reducing the positions to a common epoch was commenced by Hoek, then Director of the Utrecht Observatory. Dr. Kam, who was Hoek's assistant, continued the work after the death of the latter, and has at length been able to publish his results. The principal catalogue contains the completely determined places of 4350 stars, and is followed by two subsidiary catalogues, the first giving the places of 236 stars, and the second those of 335 stars ; all of the latter, however, are incomplete, *i.e.* the place is given in one element only. The catalogues are followed by a comparison of the places of the stars contained in them with their places as given in the Bonn *Durchmusterung*, or, for stars south of − 2° Decl., with other authorities. Notes on proper motions, corrigenda, &c., are appended, which are of considerable interest and value. We hope that the work of collecting and cataloguing the class of stars here dealt with will be continued either by Dr. Kam or by some other astronomer as well fitted for the task as he has proved himself to be.

THE PARIS OBSERVATORY.—Admiral Mouchez, Director of the Paris Observatory, has recently published his annual report to the Council of the Observatory. It is a very instructive and interesting document, and affords gratifying evidence of the enterprise and energy with which the work of this great institution is carried on.

The most striking portion of the report is that which deals with the work of the Bros. Henry in astronomical photography, but as this, as well as M. Lœwy's ingenious device for determining the amount of astronomical refraction, have already been noticed in NATURE, it will not be necessary to again refer to them. Leaving these two great undertakings therefore on one side, the rest of the report exhibits a large amount of solid work. The meridian service has comprised 16,173 observations, 795 of the sun and planets. The instruments of the Salle Méridienne have been devoted to the observation of Lalande's stars. As the great Catalogue approaches completion, the stars still to be observed become more widely scattered, and fewer observations are necessarily secured. The division-errors of the Gambey circle are being carefully investigated by M. Périgaud, and the Garden circle has been used for the determination of the absolute positions of a number of circumpolar stars. A new flexure apparatus has been constructed by M. Gautier, and 603 stars have been already observed with it. The same ingenious artist has also devised a new mode of supporting a mercury trough, for freeing it from the effect of tremors, which has been found to work very satisfactorily. The equatorials have been employed as usual in observations of comets, minor planets, and nebulæ ; the equatorial of the east tower having been employed by MM. Henry in the revision of some of their photographic charts containing very faint stars, especially the Pleiades and the regions round Vega and ε Lyræ. In the department of the calculations, the calculations for the great Catalogue had been completed as far as 8h. of R.A., and were being carried on from 8h. to 12h. The Catalogue itself was printed up to No. 3800, and the manuscript prepared up to No. 4700. Of the volume of observations for 1882, seventy-three sheets had been printed, and the rest was in the printer's hands. The volume for 1883 had been commenced, and of the *Mémoires*, tome xviii., had been distributed, and tome xix. was in course of publication.

Several important investigations have also been carried on by individual members of the staff. M. Lœwy has devised a new method for determining the absolute co-ordinates of circumpolar stars, and M. Renan has published two notes on his experiments in application of these methods. M. Callandreau has published several notes on the theory of the figure of the planets and of the earth, and numerical tables for assisting in the calculation of ephemerides for minor planets ; whilst M. Prosper Henry has been engaged in devising suitable methods for the measurement and reduction of the photographic star-charts, which differ so widely from ordinary astronomical observations. A new determination of the length of the seconds pendulum has also been made by Capt. Defforges, of the Geographical Service, the length corrected to sea-level being found to be 0·99394m. Amongst the works to be carried out in the present year is the study of the movements of the soil by the aid of a multiplying seismograph devised by M. Bouquet de la Grye. The report concludes with a reproduction of a photograph of the Pleiades and a comparison of the results thus obtained by photography in a single hour with those obtained by M. Wolf in his study of the same group through the toil of years.

NOTES ON VARIABLE STARS.—Mr. Espin, the special observer to the Liverpool Astronomical Society, has recently commenced the issue of circulars calling attention to various variable stars or stars suspected of variation. Circular No. 1 gives an ephemeris for 10 Sagittæ, the next maximum, mag. 5·6, falling due June 5·4d., and the next minimum, mag. 6·4, June 11·1, period 8·317d. Circular No. 2 calls attention to the star D.M. + 8°, No. 3780, R.A. (1885·0) 18h. 32m. 51s., Decl. 8° 43′·5 N., as a probable variable. Circular No. 3 gives new elements for U Hydræ, R.A. 10h. 31·9m., Decl. 12° 40′·7 S., from whence it would appear that the next maximum is due 1886 June 25·5d. Circular No. 4 gives provisional elements for W. Cygni, R.A. (1886·0) 21h. 31m. 44s., Decl. 44° 51′·0 N., as follows :— P = 120 to 130 days, V = 5·8 ± to 7·5 ±, M = 1886 May 19 ±, m = 1886 Feb. 14 ±.

THE "CANALS" OF MARS.—M. Terby, in a note presented some little time ago to the Royal Academy of Belgium, drew attention to the occurrence in the drawings of Mars made by Herschel and Schrœter of several markings resembling the well-known Kaiser Sea in size and distinctness, and pointed out that M. Schiaparelli, in his observations of 1881–82, represented the "canal" Indus as developed to dimensions almost as great as those of the Kaiser Sea, and that this development coincided with the "gemination" or doubling of almost all the other canals. M. Faye now announces at the last meeting of the Académie des Sciences that M. Perrotin and the other observers at the Nice Observatory have recently been able to re-detect M. Schiaparelli's canals. The reality of the existence of the delicate markings discovered by the keen-sighted astronomer of Brera seems thus fully demonstrated, and it appears highly probable that they vary in shape and distinctness with the changes of the Martial seasons.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 JUNE 6–12

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on June 6

Sun rises, 3h. 47m.; souths, 11h. 58m. 23·3s.; sets, 20h. 9m.; decl. on meridian, 22° 41′ N.: Sidereal Time at Sunset, 13h. 10m.

Moon (at First Quarter on June 9) rises, 8h. 10m.; souths, 15h. 49m.; sets, 23h. 17m.; decl. on meridian, 15° 7′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	3 25	11 29	19 33	21 45 N.
Venus	2 10	9 7	16 4	10 20 N.
Mars	11 47	18 18	0 49*	5 28 N.
Jupiter	12 31	18 48	1 5*	2 43 N.
Saturn	5 26	13 37	21 48	22 43 N.

* Indicates that the setting is that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich)

June	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
10	B.A.C. 4043	6½	0 5	0 51	74 325
11	38 Virginis	6	1 0	near approach	200 —

June	h.	
9	9	Mars in conjunction with and 0° 6′ north of the Moon.
9	22	Jupiter in conjunction with and 0° 1′ north of the Moon.
11	5	Mercury at least distance from the Sun.
12	2	Mercury in superior conjunction with the Sun.

Variable Stars

Star	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·2	81 16 N.	June 9,	2 16	*m*
S Canis Minoris	7 26·5	8 34 N.	,, 11,		*m*
W Virginis	13 20·2	2 47 S	,, 8,	21 40	*M*
δ Libræ	14 54·9	8 4 S.	,, 6,	0 50	*m*
U Coronæ	15 13·6	32 4 N.	,, 7,	1 16	*m*
U Ophiuchi	17 10·8	1 20 N.	,, 11,	0 42	*m*
X Sagittarii	17 40·4	27 47 S.	,, 12,	2 20	*M*
W Sagittarii	17 57·8	29 35 S.	,, 6,	2 25	*M*
T Herculis	18 4·8	31 0 N.	,, 11,		*m*
η Aquilæ	19 46·7	0 43 N.	,, 11,	21 30	*M*
R Sagittæ	20 8·9	16 23 N.	,, 10,		*m*
δ Cephei	22 24·9	57 50 N.	,, 6,	0 0	*m*
			,, 12,	21 35	*M*

M signifies maximum; *m* minimum.

Meteor Showers

Radiants near β Ophiuchi, R.A. 261°, Decl. 5° N., from Sagitta, R.A. 292°, Decl. 15° N., and from Vulpecula, R.A. 312°, Decl. 24° N., are represented at this time of the year, as well as the *Cygnids II.*, R.A. 319°, Decl. 32° N.

GEOGRAPHICAL NOTES

The French forces in Tonquin having now succeeded in occupying Lao-Kai, near the Chinese frontier, the capital of the Black Flag State, the whole course of the Red River in Tonquin is for the first time open to exploration. Accordingly two flat-bottomed gunboats with an exceedingly small draught have been built and equipped, and left Hanoi on April 3 to ascend the river, having on board officers whose duty it is to survey the river and the adjacent country, to fix the positions of the most important points, and to produce a map of the whole.

At the meeting of the Geographical Society of Paris on the 21st ult., M. de Lesseps referred to the works on the Panama Canal, and argued that locks or dams were unnecessary. M. Aubry gave a summary of a journey which he made in 1883 and 1884 to Choa and the Gallas country in pursuit of a mission with which he was charged by the Minister of Public Instruction. He collected a large number of mineralogical specimens, and studied the region from a geological and palæontological point of view. He also surveyed the courses of two rivers.

The Government of British North Borneo has secured the services of Capt. Beeston for the purpose of making a mineralogical and geographical survey of the country. He has started for the Segama River, which has already been visited by Frank Hatton, to investigate the localities in which gold is said to have been found.

At the instance of the Société de Géographie Commerciale of Nantes, a Commercial-Geographic Exhibition will be held in that city between June 15 and August 15 next. According to the programme the Exhibition will be divided into five classes: (1) scientific geography; (2) ethnography; (3) travelling and means of communication; (4) French and French-Colonial produce; (5) educational material.

ON RECENT PROGRESS IN THE COAL-TAR INDUSTRY[1]

Those who have read Goethe's episodes from his life, known as "Wahrheit und Dichtung," will remember his description of his visit in 1741 to the burning hill near Dutweiler, a village in the Palatinate. Here he met old Stauf, a coal philosopher, *philosophus per ignem*, whose peculiar appearance and more peculiar mode of life, Goethe remarks upon. He was engaged in an unsavoury process of collecting the oils, resin, and tar obtained in the destructive distillation of coal carried on in a rude form of coke oven. Nor were his labours crowned with pecuniary success, for he complained that he wished to turn the oil and resin to account, and save the soot, on which Goethe adds that, in attempting to do too much, the enterprise altogether failed. We can scarcely imagine, however, what Goethe's feelings would have been could he have foreseen the beautiful and useful products which the development of the science of a century and a half has been able to extract from Stauf's evil-smelling oils. With what wonder would he have regarded the synthetic power of modern chemistry, if he could have learnt that not only the brightest, the most varied colours of every tone and shade can be obtained from this coal-tar, but that some of the finest perfumes can, by the skill of the chemist, be extracted from it. Nay, that from these apparently useless oils, medicines which vie in potency with the rare vegeto-alkaloids can be obtained, and lastly, perhaps most remarkable of all, that the same raw material may be made to yield an innocuous principle, termed *saccharine*, possessed of far greater sweetness than sugar itself. The attainment of such results might well be regarded as savouring of the chimerical dreams of the alchemist, rather than expressions of sober truth, and the modern chemist may ask a riddle more paradoxical than that of Samson, "Out of the burning came forth coolness, and out of the strong came forth sweetness"; and by no one could the answer be given who had not ploughed with the heifer of science, "What smells stronger than tar, and what tastes sweeter than saccharine?" That these are matters of fact we may assure ourselves by the most convincing of all proofs—their money value, and we learn that the annual value of the products now extracted from an unsightly and apparently worthless material amounts to several millions sterling, whilst the industries based upon these results give employment to thousands of men.

Sources of the Coal-tar Products.—In order to obtain these products, whether colours, perfumes, antipyretic medicines, or sweet principle, a certain class of raw material is needed, for it is as impossible to get nutriment from a stone as to procure these products from wrong sources. All organic compounds can be traced back to certain hydrocarbons, which may be said to form the skeletons of the compounds, and these hydrocarbons are divisible into two great classes: (1) the paraffinoid, and (2) the benzenoid hydrocarbons. The chemical differences both in properties and constitution between these two series are well marked. One is the foundation of the fats, whilst the other class gives rise to the essences or aromatic bodies. Now all the colours, finer perfumes, and antipyretic medicines referred to, are members of the latter of these two classes. Hence if we wish to construct these complicated structures, we must employ building materials which are capable of being cemented into a coherent edifice, and therefore we must start with hydrocarbons belonging to the benzenoid series, as any attempt to build up the colours directly from paraffin compounds would prove impracticable. Of all the sources of hydrocarbons, by far the largest is the natural petroleum oils. But these consist almost entirely of paraffins, and hence this source is commercially inapplicable for the production of colours. We have, however, in coal itself, a raw material which

[1] A Discourse by Prof. Sir Henry E. Roscoe, M.P., LL.D., F.R.S., delivered at the Royal Institution, Friday, April 16, 1886.

by suitable treatment may be made to yield oils of a valuable character. Of these treatments, that followed out in the process of gas-making is the most important, for in addition to illuminating gas in abundant supply, tar is produced which contains principally that benzenoid class of substances already referred to, and which, to use the words of Hofmann, "is one of the most wonderful productions in the whole range of chemistry." The production of these latter as distinguished from the paraffinoid group appears to depend upon a high temperature being employed to effect the necessary decomposition.

The quantity of coal made into coke for use in the blast furnace is larger than that distilled for gas-making, no less than between eleven and twelve million tons of coal being annually consumed in the blast furnaces of this country in the form of coke, and capable of yielding two million tons of volatile products. Up to recent times, however, the whole of these volatile products has been burnt and lost in the coke ovens. But lately, various processes have been devised for preventing this loss, and for obtaining the oils, which might be made available as colour-producing materials. It is, moreover, a somewhat remarkable fact that only in one or two cases have the conditions been complied with which render it possible to obtain the necessary benzenoid substances. In the ordinary coking ovens, as well as in the blast furnaces, although the temperature ultimately reached is far in excess of that needed to form the colour-giving hydrocarbons, yet the heating process is carried on so gradually that the volatile products from the coal are obtained in the form of paraffinoid bodies mainly, and hence are useless for colour-making purposes. Amongst the few coking processes in which the heat is suddenly applied, and consequently a yield of colour-giving

TABLE I.—*One Ton of Lancashire Coal yields when distilled in Gas Retorts on an Average*

Gas (cubic feet).	Ammoniacal Liquor, 5° Tw.	Equal to Ammonium Sulphate.	Coal (Gas) Tar, sp. gr. 1·16.	Coke.
10,000	20 to 25 gallons.	30 lbs.	12 gallons = 139·2 lbs.	13 hundredweights.

Twelve Gallons of Gas-Tar yield (Average of Manchester and Salford Tar)

Benzene.	Toluene.	Phenol proper.	Solvent Naphtha for India-rubber, containing the three Xylenes.	Heavy Naphtha	Naphthalene.	Creosote.	Heavy Oil.	Anthracene.	Pitch.
lb.	lb.	lb.	lb.	lb.	lb	lb.	lb.	lb.	lb.
1·10	0·90	1·5	2·44	2·40	6·30	17·0	14	0.46	69.6
= Aniline	Toluidine		yielding		= α Naphthylamine				
1·10	0·77		0·12 Xylene =		5·25				
			0·07 Xylidine		= α or β Naphthol				
= Magenta 0·623		Aurin			4·75			Alizarin	
		1·2			= Vermilline Scarlet. RRR			20 %.	
or 1·10 lb. Aniline					7·11 or			2·25.	
yields 1·23 lb.					= Naphthol Yellow[1]				
Methyl Violet.					9·50				

Dyeing Power of Colours from 1 Ton of Lancashire Coal.

lb.	lb.	lb.	lb.	lb.	lb.
0·623 Magenta or	1·23 Methyl Violet	9·50 Naphthol Yellow	or 7·11 Vermilline	1·2 Aurin	2·25 Alizarin 20 %
dye	dye	dye	dye	dye	dye
500 yards 27 in. wide	1000 yards 27 in. wide	3800 yards 27 in. wide	2560 yards 27 in. wide	120 yards 27 in. wide	255 yds. Printer's cloth
Flannel a full shade.	Flannel a full Violet.	Flannel a full Yellow.	Flannel a full Scarlet.	Flannel a full Orange.	a full Turkey Red.

Dyeing Power of Colours from 1 lb. of Lancashire Coal.

Magenta or	Violet	Yellow or	Scarlet	Orange	Turkey Red
a piece of Flannel	a piece of Flannel	a piece of Flannel	a piece of Flannel	a piece of Flannel	a piece of Flannel
8 in. by 27 in.	24 in. by 27 in.	61 in. by 27 in.	41 in. by 27 in.	1·93 in. by 27 in.	4 in. by 27 in.

[1] The Naphthol Yellow is a representative colour from α Naphthol, while the Vermilline Scarlet is a representative colour from the combination of α Naphthylamine with β Naphthol.

hydrocarbons is obtained, may be mentioned the patented process of Simon-Carvès, the use of which is now spreading in England and abroad. The tar obtained in this process is almost identical in composition with the average gas-works tar, whilst the coke also appears to be equal for iron-smelting purposes to that derived from other coke ovens. A third source of these oils yet remains to be mentioned, viz. those obtained as a by-product in blast furnaces fed with coal.

Another condition has, in addition, to be considered in this industry, and that is the nature of the coal employed for distillation. It is a well-known fact that if Lancashire cannel be exclusively employed in gas-making a highly-luminous gas is obtained, but the tar is too rich in paraffins to be a source of profit to the tar-distiller, whilst, on the other hand, coal of a more anthracitic character, like that from Newcastle or Staffordshire, yields a tar too rich in one constituent, viz. naphthalene, and too poor in another, viz. benzene. It is also known to those engaged in carbonising coal principally for the sake of the tar that the coal from different measures, even in the same pit, yields tars of very different constitution. That under these varying conditions products of varying composition are obtained is a result that will surprise no one who considers the complicated chemical changes brought about in the process of the destructive distillation of coal.

History of Benzene and its Derivatives.—Having thus sketched the principles upon which the formation of these valuable tar colours depends, we should do wrong to pass over the history of the discovery of benzene (C_6H_6), which contributed so much to the unlocking of the coal-tar treasury.

Faraday in 1825 discovered two new hydrocarbons in the oils

obtained from portable gas. One of these was found to be butylene (C_4H_8); to the other Faraday gave the name of bicarburet of hydrogen, as he ascertained its empirical formula to be C_2H ($C = 6$). By exploding its vapour with oxygen, he observed that one volume contains 36 parts by weight of carbon to 3 parts by weight of hydrogen, and its specific gravity compared with hydrogen is therefore 39.[1]

Mitscherlich, in 1834, obtained the same hydrocarbon by distillation of benzoic acid, $C_7H_6O_2$, with slaked lime, and termed it benzin. He assumed that it is formed from benzoic acid simply by removal of carbon dioxide. Liebig denied this, adding the following editorial note to Mitscherlich's memoir:—"We have changed the name of the body obtained by Prof. Mitscherlich by the dry distillation of benzoic acid and lime, and termed by him benzin, into benzol, because the termination 'in' appears to denote an analogy between strychnine, quinine, &c., bodies to which it does not bear the slightest resemblance, whilst the ending in 'ol' corresponds better to its properties and mode of production. It would have been perhaps better if the name which the discover, Faraday, had given to this body had been retained, as its relation to benzoic acid and benzoyl compounds is not any closer than it is to that of the tar or coal from which it is obtained."

Almost at the same time Péligot found that the same hydrocarbon occurs, together with benzone, $C_{13}H_{10}O$ (diphenylketone, $CO(C_6H_5)_2$), in the products of the dry distillation of calcium benzoate.

The different results obtained by Mitscherlich and Péligot are represented by the following formulæ:—

$$C_7H_6O_2 + CaO = C_6H_6 + CaCO_3.$$
$$(C_7H_5O_2)_2Ca = C_{13}H_{10}O + CaCO_3.$$

TABLE II.

	1. *Benzene.*	2. *Toluene.*	3. *Phenols.*	4. *Xylene.*	5. *Naphthalene.*	6. *Anthracene.*
Yellows	Orange Yellow, or Acid Yellow	...	Picric Acid	..	Manchester Yellow Dinitronaphthol)	Alizarin (pure
	Metanil Yellow	...	...	..	Naphthol Yellow	Anthrapurpurin
	Auramine	...	...	..	...	
Browns	Brown, Y	Bismarck Brown, R	.		...	Flavopurpurin
Oranges	Diphenylamine Orange (Blackley Orange)	...	...	.	...	
	Chrysoidine, Y	Chrysoidine, R	Aurin		.	
	Orange I. (mixture of 1 and 5)	...	...		.	
	Orange II. (1 and 5)	.	..			
	Orange III. (Helianthine		...	.	..	
	Orange IV.	..	. .		...	
Reds	Safranin	Magenta, R	Eosin		Bordeaux	
	...	Magenta, B	Safrosin	..	Vermilline Scarlet, R	
	...	.	Cyanosine		Vermilline Scarlet, R R R	
	...	..	Rose Bengal	.	Vermilline Scarlet, B B B	
	..	..	Phloxin	...	Roccellin	
	..	..	Erythrosin	(Mixture of Xylene and Naphthalene)	(Mixture 1 and 5) New Red	
	..	..	...	Xylidine Scarlet	Biebrich Scarlet	
	...	..	...	(Mixture of Cumene and Naphthalene)	Crocein Scarlet	
	...		...	Cumidine Scarlet	... (Mixtures of 1 and 5)	
Blues	Diphenylamine Blue	Blackley Blue, R	.	...	Victoria Blue, 1	
	Methylene Blue	Blackley Blue, 1	.	...	Victoria Blue, 5	
	Indulin (Campbelline)	Alkali Blue, R	.	...	...	
	...	Alkali Blue, 6 B	.	...	...	
Violets	Methyl Violet, 6 B	...	.	..	...	
	Methyl Violet, R	..	..	...	...	
Greens	Malachite Green	..	...	..	...	
	Brilliant Green	..	...	...	...	
	Acid Green (Acid Green)	..	...	...	...	

Péligot obtained benzene only as a by-product, exactly as in the preparation of acetone (dimethylketone) from calcium acetate a certain quantity of marsh gas is always formed.

It is not clear how Liebig became acquainted with the fact that benzene is formed by the dry distillation of coal, as his pupil Hofmann, who obtained it in 1845 from coal-tar, observes: "It is frequently stated in memoirs and text-books that coal-tar oil contains benzene. I am, however, unacquainted with any research in which this question has been investigated." It is, however, worthy of remark that about the year 1834, at the time when Mitscherlich had converted benzene into nitrobenzene, the distillation of coal-tar was carried out on a large scale in the neighbourhood of Manchester; the naphtha which was obtained was employed for the purpose of dissolving the residual pitch, and thus obtaining black varnish. Attempts were made to supplant the naphtha obtained from wood-tar, which at that time was much used in the hat factories at Gorton, near Manchester, for the preparation of "lacquer," by coal-tar naphtha. The substitute, however, did not answer, as the impure naphtha left, on evaporation, so unpleasant a smell, that the workmen refused to employ it. It was also known, about the year 1838, that wood-naphtha contained oxygen, whilst that from coal-tar did not, and hence Mr. John Dale attempted to convert the latter into the former, or into some similar substance. By the action of sulphuric acid and potassium nitrate, he obtained a liquid possessing a smell resembling that of bitter almond oil, the properties of which he did not further investigate. This was, however, done in 1842 by Mr. John Leigh, who exhibited considerable quantities of benzene, nitrobenzene, and dinitrobenzene, to the Chemical Section of the British Association meeting that year in Manchester. His communication is, however, so printed in the Report, that it is not possible from the description to identify the bodies in question.

Large quantities of benzene were prepared in 1848, under Hofmann's direction, by Mansfield, who proved that the naphtha

[1] *Phil. Trans.*, 1825, p. 440.

in coal-tar contains homologues of benzenes, which may be separated from it by fractional distillation. On the 17th of February, 1856, Mansfield was occupied with the distillation of this hydrocarbon, which he foresaw would find further applications, for the Paris Exhibition, in a still. The liquid in the retort boiled over and took fire, burning Mansfield so severely that he died in a few days.

The next step in the production of colours from benzene and toluene is the manufacture of nitrobenzene, $C_6H_5NO_2$, and nitrotoluene, $C_7H_7NO_2$. The former compound, discovered in 1834 by Mitscherlich, was first introduced as a technical product by Collas under the name of artificial oil of bitter almonds, and Mansfield in 1847 patented a process for its manufacture. It is now used for perfuming soap, but mainly for the manufacture of aniline ($C_6H_5NH_2$) for aniline blue and aniline black and for magenta. It is made on a very large scale by allowing a mixture of well-cooled fuming nitric acid and strong sulphuric acid to run into benzene contained in cast-iron vessels provided with stirrers.

To prepare aniline from nitrobenzene, this compound is acted upon with a mixture of iron turnings and hydrochloric acid in a cast-iron vessel. Commercial aniline is a mixture of this compound with toluidine obtained from toluene contained in commercial benzene. Some idea of the magnitude of this industry may be gained from the fact that in one aniline works near Manchester no less than 500 tons of this material are manufactured annually. From the year 1857, after Perkin's celebrated discovery[1] of the aniline colours, up to the present day, the history of the chemistry of the tar products has been that of a continued series of victories, each one more remarkable than the last.

Coal-tar Colours.—To even enumerate the different chemical compounds which have been prepared during the last thirty years from coal-tar would be a serious task, whilst to explain their constitution and to exhibit the endless variety of their coloured derivatives which are now manufactured would occupy far more time than is placed at my disposal. On the industrial importance of these discoveries the speaker reminded his audience of the wonderful potency of chemical research, as shown by the fact that the greasy material which in 1869 was burnt in the furnaces or sold as a cheap waggon grease at the rate of a few shillings a ton, received two years afterwards, when pressed into cakes, a price of no less than one shilling per pound, and this revolution was caused by Gräbe and Liebermann's synthesis of alizarin, the colouring matter of madder,[2] which is now manufactured from anthracene at a rate of more than two millions sterling per annum; and it is stated that an offer was once made, in the earlier stages of its history, by a manufacturer of anthracene to the Paris authorities to take up the asphalt used in the streets for the purpose of distilling it, in order to recover the crude anthracene.

Again, we have in the azo-scarlets derived from naphthalene a second remarkable instance of the replacement of a natural colouring matter, that of the cochineal insect, by artificial tar-products, and the naphthol-yellows are gradually driving out the dyes obtained from wood extracts and berries. It is, however, true that some of the natural dye-stuffs appear to withstand the action of light better than their artificial substitutes, and our soldiers' red coats are still dyed with cochineal.

The introduction of these artificial scarlets has, it is interesting to note, greatly diminished the cultivation of cochineal in the Canaries, where, in its place, tobacco and sugar are now being largely grown.

Let us next turn to inquire as to the quantities of these various products obtainable by the distillation of one ton of coal in a gas-retort. The six most important materials found in gas-tar from which colours can be prepared are:—

1. Benzene.	4. Metaxylene (from solvent naphtha).
2. Toluene.	5. Naphthalene.
3. Phenol.	6. Anthracene.

The average quantity of each of these six raw materials obtainable by the destructive distillation of one ton of Lancashire coal is seen in Table I. Moreover, this table shows the average amount of certain colours which each of these raw materials yields, viz.:—

1. } 2. } Magenta 0·623 lb.	4. (*Xylidine* 0·07 lb.) 5. Vermilline scarlet 7·11 lbs.
3. Aurin 1·2 lb.	6. Alizarin 2·25 lbs. (20 per cent.)

Further, it shows the dyeing power of the above quantities of each of these colours, all obtained from one ton of coal, viz.:—

1 and 2. Magenta, 500 yards of flannel.
3. Aurin, 120 yards of flannel 27 in. wide.
4 and 5. Vermilline scarlet, 2560 yards of flannel.
6. Alizarin, 255 yards Turkey red cloth.

Lastly, to point out still more clearly these relationships, the dyeing power of one pound of coal is seen in the lowest horizontal column, and here we have a party-coloured flag, which exhibits the exact amount of colour obtainable from one pound of Lancashire coal.

Let us moreover remember, in this context, that no less than ten million tons of coal are used for gas-making every year in this country, and then let us form a notion of the vast colouring power which this quantity of coal represents.

The several colours here chosen as examples are only a few amongst a very numerous list of varied colour derivatives of each group. Thus we are at present acquainted with about sixteen distinct yellow colours; about twelve orange; more than thirty red colours; about fifteen blues, seven greens, and nine violets; also a number of browns and blacks, not to speak of mixtures of these several chemical compounds, giving rise to an almost infinite number of shades and tones of colour. These colours are capable of a rough arrangement according as they are originally derived from one or other of the hydrocarbons contained in the coal-tar. The fifty specimens of different colours exhibited may thus be classified, but in Table II., for the sake of brevity, only the commercial names and not the chemical formulæ of these compounds is given.

(*To be continued.*)

[1] See Lectures by Prof. Hofmann, F.R.S., "On Mauve and Magenta," April 11, 1882, and W. H. Perkin, F.R.S., "On the Newest Colouring Matters," May 14, 1869, *Proc. Roy Inst.*; also President's Address (Dr. Perkin, F.R.S.), *Journal of Society of Chemical Industry*, vol. iv., July 1884, on Coal-Tar Colours.

[2] "On the Artificial Production of Alizarine, the Colouring Matter of Madder," by Prof. H. E. Roscoe, *Proc. Roy. Inst.*, April 1, 1870; also Dr. Perkin, F.R.S., "On the History of Alizarine," *Journal Society of Arts*, May 30, 1879.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Prof. Liveing has been appointed Chairman of the Examiners for the Natural Sciences Tripos, and Mr. James Ward of those for the Moral Sciences Tripos. There were 106 candidates for the first part of the Natural Sciences Tripos recently held.

Attention has recently been given to the preservation of University buildings from fire, and serious defects have been, or are being, remedied. Such matters should be carefully thought out in regard to every museum and library, and it is to be hoped that attention will be constantly given to the efficiency of means of prevention and extinction of fires. The report on this subject in No. 636 of the *Cambridge University Reporter* is well worthy of the study of officials concerned in guarding precious scientific collections.

Prof. Darwin will lecture in the Long Vacation on the Theory of the Potential, Attractions, and the Figure of the Earth, the first lecture being on Tuesday, July 13.

A recent discussion of a report by the Special Board on Medicine emphasised the desirability of teaching elementary physics as part of general education to those intending to become medical students, and showed that the new "extra subjects" of the Previous Examination do not satisfactorily secure this, dynamics and a mathematical treatment being required, rather than experimental acquaintance with the physical forces. Mr. Oscar Browning said the interests of education were suffering terribly from the want of agreement as to what schoolboys ought to be taught. Mr. Shaw remarked on the importance of a training in inductive reasoning for medical students, for their whole practice would consist in drawing inductions.

The grants from the Worts Fund to Messrs. Bateson, Seward, Gadow, and Potter, to which we recently referred, have been voted by the Senate.

Prof. Alfred Marshall is giving a prize of 15*l.* annually for Political Economy, to be open to all members of the University under the M.A. degree. The examination is to consist of the papers on Political Economy in Part I., and on Advanced Poli-

ical Economy in Part II. of the Moral Sciences Tripos. The first award is to be in June 1887. He desires to concentrate the attention of some students more systematically than hitherto, noting that on some sides Natural Science studies constitute the best preparation.

During the last ten years, grants from the Worts Fund for Antiquarian and Literary subjects have amounted to 1100*l*.; for Biological and Geological subjects, to 1225*l*.; and for Medical subjects, to 100*l*.

Sir J. Lubbock's Rede Lecture will be delivered on Wednesday, June 9, at 2 p.m., in the Senate House, subject, "On the Forms of Seedlings and the Causes to which they are due."

SOCIETIES AND ACADEMIES

London

Royal Society, May 27.—"The Influence of Stress and Strain on the Physical Properties of Matter. Part I. Elasticity (continued). The Effect of Magnetisation on the Elasticity and the Internal Friction of Metals." By Herbert Tomlinson, B.A. Communicated by Prof. W. Grylls Adams, M.A., F.R.S.

The principal object of this investigation was to test the soundness of the view advanced by Prof. G. Wiedemann respecting the cause of the internal friction of a torsionally oscillating wire. According to this view, the internal friction is mainly due to permanent rotation to-and-fro of the molecules about their axes; it seemed probable, therefore, that experiments on the effects of magnetising a wire, either longitudinally with a helix, or circularly by passing a current through it, would aid in elucidating the matter.

The following are the principal results which have been obtained:—

(1) When the deformations produced by the oscillations are small, the internal friction of a torsionally vibrating wire of iron or steel is not affected by sustained longitudinal magnetisation of moderate amount. The internal friction is also not affected by the sustained magnetisation even when the latter is carried to the point of saturation, provided the magnetising current be, previously to experimenting, reversed a great number of times. When no previous reversals have been made, the internal friction is slightly increased by intense magnetisation.

(2) When the deformations produced by the oscillations are large, the internal friction is very sensibly increased by sustained longitudinal magnetisation of large amount.

(3) The torsional elasticity is entirely independent of any sustained longitudinally magnetising stress which may be acting upon an iron or steel wire, provided the deformations produced by the torsional oscillations be small. When the deformations are large, the number of oscillations executed in a given time is very slightly lessened by sustained longitudinal magnetisation of large amount.

(4) When the magnetising current is interrupted and, to a greater extent, when it is reversed repeatedly whilst the wire is oscillating, the internal friction is increased, provided the magnetising stress be of moderate amount. The increase of internal friction may become very considerable when the magnetising stress is great.

When the number of interruptions or reversals in a given time of the magnetising current exceeds a certain limit, the effect on the internal friction begins to decline.

(5) When the deformations produced by the oscillations are small, the torsional elasticity is not affected by either repeatedly interrupted or reversed longitudinal magnetisation even when the magnetising stress is large.

(6) There exists a limit of magnetic stress within which no permanent rotation whatever of the molecules is produced. This limit may be widened by previous repeated reversals of a large magnetising stress.

(7) The passage of a moderate electric current, whether sustained or interrupted, through a torsionally vibrating wire of iron, steel, or nickel does not affect, except by heating, either the internal friction or the torsional elasticity, provided the deformations produced by the oscillations be small.

(8) The effect of longitudinal magnetisation, even when carried to the point of saturation, on the longitudinal oscillation of an iron or steel wire, is *nil*.

(9) The passage of an electric current, whether sustained or interrupted, through a longitudinally oscillating wire of iron or steel does not, except by heating, affect the number of oscillations executed in a given time.

Chemical Society, May 6.—Dr. Hugo Muller, F.R.S., President, in the chair.—Messrs. John W. King, William Herbert Hyatt, and George T. Holloway were admitted Fellows of the Society.—The following papers were read:—Paranitrobenzoylacetic acid and some of its derivatives, by Dr. W. H. Perkin, jun., and Dr. E. Bellinot.—An acetic ferment which forms cellulose, by Adrian J. Brown.

Victoria Institute, May 28.—Annual Meeting.—The chair being taken by Prof. Stokes, P.R.S., Capt. Francis Petrie, as Honorary Secretary, read the report, which showed that the home, colonial, and American members were now upwards of 1150, and an increasing number of leading men of science had contributed to its transactions, and the Institute was much indebted to many other scientific men of eminence, at present outside its ranks, who had kindly given their aid and advice, so that the Institute might the more worthily foster a true appreciation of the results of scientific inquiry.—Prof. Hull, F.R.S., Director of the Geological Survey of Ireland, delivered the address, in which he gave an account of the work, discoveries, and general results of the recent Geological and Geographical Expedition to Arabia and Western Palestine, of which he had charge. Prof. Hull, having sketched the course taken by the scientific Expedition (which to a considerable extent took the route ascribed to the Israelites), the physical features of the country, evidences of raised beaches, &c., showed that at one time an arm of the Mediterranean had occupied the valley of the Nile as far as the First Cataract, the level of the land being 200 feet lower than at present (an opinion which had also been arrived at by another of the Institute's members, Sir W. Dawson), and that, at the time of the Exodus, the Red Sea ran up into the Bitter Lakes, and clearly must have formed a barrier to the travellers' progress at that time; he then alluded to the great changes of elevation in the land eastward of these lakes, mentioning that the waters of the Jordan valley once stood 1300 feet above their present height. The various geological and geographical features of the country were so described as to make the address a condensed report of all that is now known of that part of the East.—A vote of thanks was accorded to Dr. Hull, after which the members and their guests adjourned to the museum, where refreshments were served.

Edinburgh

Mathematical Society, May 14.—Dr. R. M. Ferguson, President, in the chair.—Mr. J. S. Mackay gave a construction, due to the Right Hon. H. C. E. Childers, for solving the problem of medial section; Mr. W. Peddie read the second part of a paper on the theory of contour lines and its application to physical science; and Mr. A. Y. Fraser submitted a paper, by Mr. Charles Chree, on the vibrations of a spherical or cylindrical body surrounded by or containing fluid.

Paris

Academy of Sciences, May 24.—M. Jurien de la Gravière, President, in the chair.—Order of appearance of the first vessels in the leaves of the Cruciferæ: mixed formation (part 5), by M. A. Trécul. In a previous paper the author showed that the primary lobes in the type of mixed formation presented by certain Cruciferæ appear on either side of the young leaves in two superimposed series—a lower *basipetal* and an upper *basifugal*. He now proves that the first vessels of the nervous system corresponding to these lobes usually appear in the same order. Those opposed to the lobes of the basifugal series follow from below upwards, while those opposed to the lobes of the basipetal series make their appearance successively from above downwards.—A study of the movements communicated to the air by the action of a bird's wing: M. Müller's experiments, by M. Marey. A description is given of M. Müller's mechanical experiments, which are conducted at night by the aid of phosphorescent vapours, and during the day by means of smoke in the way adopted by Tyndall.—Note accompanying the presentation of M. Verbeek's fresh studies on the Krakatão eruption, by M. Daubrée. Besides a detailed account of the eruption this comprehensive work contains a full description of the meteorological and magnetic phenomena attending it, together with some theoretical considerations on their causes. The author calculates that the quantity of matter ejected was at least 18 cubic kilometres in volume, all incoherent, consequently unaccompanied by any flow of lava.—Presentation of various maps of France, Algeria, Tunisia, and Africa, issued by the Geo-

graphical Service of the Army, by M. Perrier. Amongst these maps are one of France, scale 1 : 200,000, comprising the districts of Amiens, Melun, Lille, Mézières; one of Algeria, part 6, scale 1 : 50,000, districts of Azeffun, Jebel-Filfila, Jemmapes, Ben-Harun, Aine-Bessem, Rio Salado; and one of Tunisia, scale 1 : 200,000, districts of Nefta, Rejem-Matong, Dwirat, Wed-Fessi.—Note on a new form of purulent infection following an acute attack of pneumonia, by M. Jaccoud.—Researches on the organisation of the star-fish, by M. Edm. Perrier. Amongst the collections brought back by the Cape Horn Mission were several specimens of a new species of star-fish (*Asterias hyade i*, E. P.), with their young still attached, a circumstance which has helped to throw fresh light on some disputed points connected with the anatomical structure of these animals.—Observations of the new comets 1886 *a* (Brooks I.) and 1886 *b* (Brooks II.) made at the Observatory of Nice (Gautier equatorial), by M. Charlois.—On the geography of the Central Tunisian seaboard, by M. Rouire. A careful survey of the section of the coast between Hammamet and Susa has determined the existence of a large marine inlet at the head of Hammamet Bay, which receives all the drainage of Central Tunisia. It was also ascertained that at some more or less remote period the Halk-el-Mengel Sebkha was certainly navigable.—Determination of the absolute value of the wavelength of the ray D_2, by M. J. Macé de Lépinay. A fresh attempt to settle this disputed point gives the general result—

$$5 \cdot 8917 \times 10^{-5} \text{ (millilitre)}^{\frac{1}{3}};$$

and in the air, at 0°, normal pressure—

$$5 \cdot 8900 \times 10^{-5} \text{ (millilitre)}^{\frac{1}{3}}.$$

—On a visual illusion: apparent motion of a small object when slightly illumined amid the surrounding darkness, by M. Aug. Charpentier.—A new electric fuse for exploding mines charged with powder or dynamite, by MM. Scola and Ruggieri. For this fuse the authors claim that it prevents all accidents from slow combustion, and also removes some other dangers and difficulties attending mining operations.—Note on an apparatus intended to test the efficacy, or ascertain defects in the preparation, of electric fuses, by M. Ducretet.—Description of the cyclone that swept over Madrid on May 12, by M. A. F. Noguès.—On two different conditions of the black oxide of copper, by M. Joannis.—Action of the air, silica, and kaolin on the alkaline haloid salts: new methods of preparing hydrochloric acid, chlorine, and iodine, by M. Alex. Gorgeu.—On the oxidation of oils, by M. Ach. Livache.—On a little-known cause of corrosion in steam-boilers, by MM. D. Klein and A. Berg.—On a new means of employing the iodo-iodurctted reaction in the research of the alkaloids, and especially of the leucomaïnes in urine, by MM. Chibret and Izarn.—A fresh study of Entoniscus (*E. kossmanni*, *E. fraissi*, *E. moniezii*), by MM. A. Giard and J. Bonnier.—On the embryogeny of Comatula (*C. mediterranea*), by M. J. Barrois.—Observations regarding the nervous system and certain organic features of the scutibranch gasteropods, by M. E. L. Bouvier.—On a new Ichthyobdella, by M. R. Saint Loup. This species, which the author describes under the name of *Scorpænobdella elegans*, was recently observed in the Marine Zoological Laboratory at Marseilles.—On the superficial vascular apparatus of fishes, by M. P. de Sède.—On a fungus developed in the human saliva, by M. Galippe. This fungus, discovered in some saliva filtered by Pasteur's apparatus, and cultivated in Van Tieghem's cellules, belongs to the family of the Monilia. The author proposes to name it *Monilia sputicola* (sp.n.).—Remarks on the fifth volume of M. Habich's *Anales de Construcciones civiles y de Minas*, presented to the Academy, by M. Daubrée. To this volume M. Chalon contributes a paper on the prehistoric monuments of Peru, which show a remarkable resemblance to the menhirs, cromlechs, dolmens, and other "Druidical" remains in the west of Europe. They occur in large numbers in every part of the country.—At the request of M. de Lesseps, the President appointed a Commission comprising the members of the Sections for Geography, Navigation, and Astronomy, with MM. Daubrée, Favé, Lalanne, and de Jonquières to study the differences of level caused by the tides in the Pacific and Atlantic Oceans.

STOCKHOLM

Academy of Sciences, April 14.—On the results of some experiments on the condition of electricity in a vacuum, by Prof. E. Edlund.—On the power and fineness of the hollow muscles of the frog, by Herr C. G. Santesson.—On the oxidation of cymal, and on nitrocymal, by Prof. O. Widman and Dr. J. O. Bladin.—Mineralogical notes, by Dr. G. Flink.—Some remarks on the geological map of Sweden, by Herr E. Törnebohm.—Determination of the definite elements of the orbit of the Comet VIII. (1881), by Dr. K. G. Olsson.—Micrometrical determinations of some telescopic star clusters, by Prof. H. Schultz.—Contributions to the theory of wave-motions in a gaseous medium, by Prof. A. V. Backlund.—On the integration of the differential equation in the problem of N bodies, by Prof. G. Dillner.

May 12.—On a new method for determining the velocity of the electric molecules in a current of a certain power, by Prof. Edlund.—Contributions to the knowledge of the discharge of the Ruhmkorff coil, by Hr. T. Moll.—A method for increasing the convergence of periodical series, by Hr. C. Charlier.—Research on a non-linear differential equation of the second order, by Prof. H. Gyldén.—An account of the Zoological Station of Christineberg, in the province of Bohus, belonging to the Academy, by Prof. Sven Lovén.—Researches on the changes of the arsenious oxide in contact with putrid animal matters, by Prof. Hamberg.—A balance constructed by Hr. F. J. Lemcke for determining the consumption of the normal light in the measurement of the power of the light, exhibited by Prof. F. L. Ekman.—New or imperfectly known Isopoda, part 3, by Dr. C. Bavallius.—On naphtoë acids, by Dr. Ekstrand.

BOOKS AND PAMPHLETS RECEIVED

"Ling-Nam," by B. C. Henry (Partridge).—"Infant School Management," by S. S. Hale (Stanford).—"The Romance of Mathematics," by P. Hampson (E. Stock).—"Handy Guide to Norway," by T. B. Willson (Stanford).—"Chemical Arithmetic," by J. M. Coit (Heath, Boston).—"Summary Report of the Operations of the Geological and Natural History Survey of Canada," part 3 (Maclean, Ottawa).—"Earthquakes of Ischia," by H. J. Johnston-Lavis (Dulau).—"La Mythologie," by A. Lang; traduit de l'Anglais par Léon Parmentier (A. Dupret).—"Determination of Rock-forming Minerals," by Dr. E. Hussack, translated by Dr. E. J. Smith (Wiley, New York).—"Studies from the Biological Observatory, Johns Hopkins University," vol. iii. No. 6.—"Catalogue No. 10 of Physical Apparatus for Universities and Superior Schools" (F. Ernecke, Berlin).—"Account of the Graphic Method in Use for Determining the Co-ordinates of the Secondary Trigonometrical Stations of the Ordnance Survey," by J. O. Farrell (Eyre and Spottiswoode).—"Modern Armour for National Defence," 2nd edition, by W. H. Jacques.—"Ericsson's Destroyer and Submarine Gun," by W. H. Jacques.—"Heavy Ordnance and National Defence," by W. H. Jacques (Putnam, New York).—"Circulars of Information of the Bureau of Education," No. 4, 1885: Education in Japan (Washington).—"John Bunyan and the Gipsies," by J. Simson (Maclachlan, Edinburgh).

CONTENTS PAGE

THURSDAY, JUNE 10, 1886

THE 43-TON GUN EXPLOSION

THE bursting of the 43-ton gun on board the *Collingwood* has naturally attracted considerable attention from men of science as well as from the general public, and it may not be out of place at the present time to compare the ideas of scientific men with the data given in text-books published by authority and put into the hands of artillery officers for purposes of instruction.

The latest gunnery text-book is that compiled by Major Mackinlay, R.A., and published in 1883. In it is a table compiled by another artillery officer, in which is given the calculated and actual results obtained from all classes of ordnance, among them being the 12″ B.L. gun of 43 tons, Mark II., the gun which burst on board the *Collingwood.*

The powder charge is given as 286 lbs. prismatic, projectile 720 lbs.; the total work theoretically producible from the expansion of the charge in the bore 22,884 foot tons; the actual work produced in muzzle energy of projectile 17,180 foot tons. There is therefore, according to the table, a loss of 5704 foot tons to be expended—externally in expelling the powder gases, displacing the atmosphere, and recoil; internally in heating and stretching the gun, in friction of the gas check and gases, and rotating the shot.

In a lecture delivered in January 1885, just after the bursting of the *Active's* 6″ gun, Mr. Wm. Anderson pointed out that the lost work was very much greater than was usually imagined, and he went through a calculation on thermo-dynamical principles of the forces produced in a 10″ B.L. gun. Taking Mr. Anderson's formulæ and adapting them to the 12″ B.L. gun, we shall not have to proceed far before finding out how erroneous is the table given in the text-book. We will only examine the forces external to the gun (*i.e.* those producing recoil), as Mr. Anderson's accuracy on these points has been indorsed by Col. Moncrieff, C.B., F.R.S., and we shall neglect internal forces employed in friction stretching and heating the gun—forces difficult to estimate, and on which there is some difference of opinion.

As to the energy of the projectile there can be no question, but taking the terminal pressure at 3 tons (we believe it has risen as high as 5·5 to 6 tons) the energy of the expelled gases is 12,208 foot tons, that expended in lifting the atmosphere 2501 tons, together 14,714 foot tons; so that, neglecting internal forces altogether—and these are no small quantity—we have 14,714 foot tons of energy against 5704 in the text-book.

We can only conclude that the pressure corresponding to this difference, as well as that due to the forces expended inside the bore, have been entirely neglected by the designers of the guns.

The powder with which the gun was burst was that known as "cocoa"; this powder, while diminishing the maximum pressure carries it further down the bore (a most dangerous thing for guns weak in the chase), and increases the mean and final pressures, and the muzzle velocity; therefore the calculation we have given is probably within the mark for an equal weight of cocoa powder.

The reason for the errors we have pointed out may probably be found in the blind confidence placed in the indications of crusher gauges. It is well known that a certain length of time must be taken up in the compression of all metals, and it is extremely doubtful whether something near the two-hundredth part of a second in which the explosion takes place is sufficient for the compression of copper cylinders, especially when comparisons are made between those near the breech, which are longer under the influence of the powder gases, and those in the muzzle, which are not acted on for any appreciable time.

The only item in the above estimates which has been ascertained by experiment is the muzzle velocity, the others are the result of calculation, and although Col. Moncrieff tells us they are to be trusted when measured in recoil, it would be far more satisfactory were they ascertained by direct experiment.

Means for verification have been pointed out, and although we believe the Government is provided with the instruments nothing has yet been done with them.

The failure of the *Collingwood's* gun raised hopes in the minds of many that at last a proper inquiry into the question would be held; but it has been referred back to the same Committee, associated with the same civilians (except Mr. Leece, who is dead), who recommended that the gun "should remain unaltered," but that "the charge of 295 lbs. of cocoa powder should not be exceeded."

They therefore are to sit in judgment on themselves, and if they relied on the same data when recommending certain additional strength should be given to other guns, as they did when merely limiting the charge of the 12″, what reason is there for supposing that the results with other guns will differ from this one except in loss of life and damage done?

GEOLOGY OF TURKESTAN

Turkestan; a Geological and Orographical Description based upon Data collected during the Journeys of 1874 *to* 1880. By J. V. Moushketoff. Pp. 714. With Map and Engravings. Russian. (St. Petersburg, 1886.)

FOR several years past all who take an interest in Central Asia have followed with great interest the yearly reports published by Profs. Romanovsky and Moushketoff on their geological explorations in the mountains of the Tian-Shan, the high plateaux of the Pamir, and the lowlands of the Amu-daria. The extensive character of these explorations, prosecuted for several consecutive years, and the practical experience of the two Russian geologists promised that new light would soon be thrown on several important but doubtful points in the geology of this most interesting region; but the appearance of their works has been delayed for some years. We have, however, now before us the first volume of M. Moushketoff's work, and when the whole, which will comprise three large volumes, as also M. Romanovsky's work on the same subject, is published, we shall have an almost complete picture of the geology of this region which contains the key to so many important geological questions in Europe.

The first volume of M. Moushketoff's "Turkestan" consists of two parts. The first is an analysis of all explorations made in Turkestan up to 1884; and, the literature of the subject being scattered in periodicals, it will be of great value for the geographer. All the Russian and English explorations are mentioned, and the chief of them briefly analysed. The second part contains a description of the journeys of the author in the "Turan or Aral basin," including Samarkand, the western outskirts of the Tian-Shan, the valley of Ferganah, the western outskirts of the Pamir and Alay region, the valley of the Amu-daria from Termez to Khiva, and the Kyzyl-kum sands. A summary concludes the volume, which is accompanied by a novelty that will agreeably surprise geologists, namely, a geological map of Russian Turkestan, on a scale of 67 miles to an inch. Another map, on a scale of 20 miles to an inch, is in course of preparation, that now published being only intended to show the extension and limits of four great geological subdivisions: the Post-Tertiary deposits; the Tertiary, together with the Chalk, Jurassic, and Trias; the Primary, including the Archæan crystalline slates; and the unstratified crystalline rocks (granites, porphyries, diabases, and so on). Of course, it is regrettable that the Secondary deposits could not be separated from the Tertiary; but we must wait for the appearance of the promised map on a larger scale.

As to the conclusions arrived at in this volume, we may remark at once that the personal inclinations of the author being chiefly directed to petrography on the one hand, and dynamic geology on the other, these two departments have received most attention; while Prof. Romanovsky, being a well-known palæontologist, has devoted his chief attention and wide practical knowledge to the discrimination of the different subdivisions of the sedimentary deposits explored by him. M. Romanovsky having published almost every year very valuable reports on his summer's work, his researches are embodied in M. Moushketoff's work, so that each is complementary to the other.

Nearly the whole of the Aral basin (and we have seen that the author includes under this name the wide tracts east and south-east of Lake Aral) is covered by Chalk, Tertiary, and Post-Tertiary deposits; the remaining portion, that is, no more than one-twentieth of the area, being occupied by crystalline unstratified rocks, metamorphosed slate, and Palæozoic deposits which appear from below the above. A mere glance at a topographical map of the region would be sufficient to indicate their extent—all the hills rising amidst the wide steppes being built up of Palæozoic or Archæan rocks. The Devonian limestones of the mountains Urda-bashi and Karatash; the syenites, diabases, and crystalline slates of the Mogol-tau, Kochkar-ata, and Karnak mountains; and the Devonian and Carboniferous limestones of the Kazy-kurt hills are in this category. Some gold, silver-and-lead ores, copper, as also almandine and beryl, are found in these mountains. It is interesting also to notice that the crystalline rocks in the Palæozoic islands scattered amidst the steppes are much more metamorphosed than the corresponding rocks in the Tian-Shan mountains. They have obviously been long subject to the influence of water, which once covered what is now the steppe region.

Jurassic deposits are the next geological formation found in the East Aral basin. The lowest strata seem, however, to belong to the Rhætic subdivision—the few remains of plants which they contain being some of them Jurassic, while others should be recognised as Rhætic, and the third Triassic. They contain no traces of marine origin, and only one fresh-water shell, the *Anadonta boroldaica*, Romanovsky. It must therefore be concluded that throughout the Triassic and Jurassic periods nearly all Turkestan was a land having on its borders numerous lakes containing sweet or brackish water. These Jurassic deposits appear only on the borders of the East Aral basin: namely, on the Baidam and Saram rivers, and in Ferganah; in the west they are known on the Mangishlak peninsula (Caspian). Everywhere they contain most valuable deposits of coal. We may add that the geologist will thus find, in the Aral basin, the well-known geological feature so characteristic of the structure of East Siberia and Manchuria.

Chalk and Tertiary deposits are widely spread. They constitute the bottom of nearly all the basin, and reach a thickness of 2000 feet in Ferganah, and 5000 feet in the Hissar region. Two systems of dislocation are pretty well observed amidst these deposits which are folded in two chief directions: north-east (60°) and north-west (60°). This observation of M. Moushketoff is well worthy of notice. We thus find, on the outskirts of the hilly tracts of Asia, the two great systems of upheavals which are so characteristic of Asia: the system of ridges and plateaux running from south-west to north-east, which we have found appearing with such persistency in the East Siberian hilly tracts; and the north-western direction, which appears predominantly in South-West Asia.

The Chalk deposits show great variety of structure: sandstones, limestones, and marls predominating. As to their fossils, they appear at some places in immense masses, but the number of species is mostly limited. According to M. Romanovsky, the Upper and Middle Chalk are represented there: the former, very rich in oysters, is closely akin to the Senonian of Europe; it is much developed on the outskirts of the Tian-Shan, especially in Ferganah, but it changes its characters (Senonian Ammonites making their appearance) farther west, in the lower parts of the Amu-daria. The Middle Chalk, which, however, it is difficult to separate from the former, has a still wider extension. The Chalk contains a number of useful minerals; namely, phosphorite, gypsum, naphtha, ozokerite, and sulphur.

The Tertiary deposits are so closely connected with the Chalk deposits that it is often difficult to separate them from one another; they are still poorer in fossils (excepting those on the northern and western shores of Lake Aral, as yet unexplored), especially towards the east, as we approach the Tian-Shan. In this last region we have, as is known, the Eocene deposits, consisting for the most part of deep-sea deposits of Nummulite sandstones. They are covered with Lower and Middle Oligocene, very much like the German and Belgian Tertiary deposits, and these last in their turn disappear under Miocene limestones and Sarmatian clays.

The Tertiary deposits of the Tian-Shan contain, on the contrary, very little or no clays, and chiefly consist of conglomerates and sandstones. Some of them date from

the Eocene period, some others from the Oligocene; while the most recent Miocene and Pliocene deposits contain a fauna in process of decay: the deep-sea fauna of the banks of the Aral is substituted by a shallow-water fauna, and the Nummulite banks disappear. The chief fossils are teeth of sharks, some Lamellibranchiata, and a few oysters (*Sphenia rostrata*, Lamk.; *Modiola subcarinata*, Lamk.; *M. jeremejewi*, Roman.; *Alligator darwini*, Ludw.; *Ostrea raincurti*, Desh.; *O. longirostris*, Lamk., &c.). These features, as also the extension of pudding-stones, especially on the outskirts of the Tian-Shan, are indicative of their littoral origin. The same distinction appears as to the minerals they contain. Several great beds of gypsum, brown-coal, and bituminous slates are found in the Tertiary deposits around Lake Aral, as also naphtha in the Balkhan mountains; but both naphtha and brown-coal are absent in the Tian-Shan deposits, which contain, on the contrary, salt, together with gypsum.

The Post-Pliocene Aral-Caspian deposits can hardly be delimitated from the Tertiary deposits. Their maximum thickness does not exceed 100 feet. Both in the Black-Sands (Kara kum) and the Red-Sands (Kyzyl-kum) they consist of a sandy clay which often passes upwards into a clayey sandstone. As to their petrographical features, they are the same from the Volga to the foot of the Tian-Shan. The fossils they contain (*Cardium edule*, *Dreyssena polymorpha*, *Neritina liturata*, *Adacna vitrea*, and *Hydrobia stagnalis* in the Kara-kum; *Lithoglyphus caspius*, *Hydrobia stagnalis*, *Anadonta ponderosa*, and the Spongia described as *Metschnikowia tuberculata* by M. Grimm in the Kyzyl-kum) are all now living in the Caspian and Lake Aral, and precisely in the littoral shallow-water zone.

What are the limits of this immense Post-Pliocene basin surely forms one of the most interesting problems of geology, and they can already be determined approximately. In the west, the Ergeni hills (which run due south of the great Tsaritoyn bend of the Volga) form its western shore[1]—a great gulf extending along the broad valley of the two Manych rivers towards the Black Sea. Further south it must have been much nearer to the present shore of the Caspian, with a broad gulf to the west in what is now the valley of the Kura. How far this gulf extended towards the north remains still unsettled. The evidence derived from the *Dreyssena polymorpha*, found as far north as the Samara winding of the Volga, is still contested by MM. Möller and Grimm—this species of *Dreyssena* being a too cosmopolitan one; but the discovery of a few Caspian mussels even further north, towards Simbirsk, as well as the orography of this region, make one incline to the opinion that a narrow gulf of the Aral-Caspian Post-Pliocene sea extended almost as far as the mouth of the Kama, with a wide lake filling up the Oka depression of the Volga and communicating with the sea by an outlet. It is known that this basin extended towards Lake Aral and further east, with a peninsula which entered it from the north, and which is now known as the Ust-urt and Mugo-djar hills. How far it extended towards the east remains still unsettled. M. Moushketoff only mentions the supposition of the late M. Severtsoff as to the connection which existed between Lake Aral and Lake Balkhash. However probable this connection, we ought to take into consideration the latest researches of Russian zoologists, according to which the fauna of Lake Balkhash would have much more kinship with the lakes of Central Asia than with the fauna of Lake Aral. If this fact is confirmed, we should probably distinguish two different periods—an earlier and a later one—during which last the connection between Lake Balkhash and Lake Aral was broken, but continued between the former and the eastern lakes of Central Asia.

[1] Prof. Barbot-de-Marny, whose deep insight and keen observation are so highly esteemed, extended these limits further west. Several considerations derived from the orography and physical geography of the region give, in my opinion, great probability to M. Moushketoff's view on the question. He has also had the opportunity of making a more thorough exploration of the region.

As to the southern limits of the Aral-Caspian basin, they cannot yet be determined with certainty. Aral-Caspian deposits are wanting in the middle parts of the Kyzyl-kum plateau, so that the southern shores of this basin must have been somewhere in the latitude of the Bukan-tau mountains. Further east they ran in a more southern latitude In the Sary-kamysh depression and for 160 miles further south we again find Aral-Caspian mussels, as far as the Bala-Ishem wells, and in this region the Uzboy (formerly considered as the old bed of the Amu) disappears. South of Lake Aral they hardly reach the latitude of Merv. From all these data, M. Moushketoff concludes that the basin consisted of two different parts—the Caspian and the Aral part—connected by a narrow outlet passing by the base of the Balkhan mountains. The eastern portion was shallower than the western; it had more islands, and its organic life was poorer. It was also subdivided, in its turn, into two parts connected by the Aibughir outlet.

As to the drying up of this basin and its subsequent modifications, which M. Moushketoff attributes in great part to the agency of the wind, we shall devote to them a second article, inasmuch as the author's observations on the dunes and moving sands deserve special attention.

P. K.

(*To be continued.*)

THE NATURALIST'S DIARY

The Naturalist's Diary. Arranged and Edited by Charles Roberts, F.R.C.S., L.R.C.P., &c. (Swan Sonnenschein, Le Bas, and Lowry, Paternoster Square).

THIS book may be described as a most excellent *vade mecum* and guide to any person who not only wishes to keep a phenological diary, but who wishes to know what to enter therein. The preface and introduction show forth the principles which have guided the author in making this compilation, and the important services it may be made to render to biologists and to men of science, as well as to practical gardeners, agriculturists, sportsmen, and residents in the country generally. It is also recommended to the notice of tourists, and especially to those who find themselves perchance perforce anchored in some one of our numerous health resorts, cut off from their usual avocations. Mr. Roberts's observations have been made on the breezy downs of Marlborough in connection with the Marlborough College Natural History Society, 1864–84. They include registration of mean, maximum, and minimum temperature in sun and shade, "accumulated temperature" above 42° day degrees, barometrical observations, rainfall, and direction of wind.

These are, however, only the necessary key to what follows in the most interesting observations upon the first appearance of each familiar flower, the maiden song of each sweet warbler of the grove, the arrival of summer visitants, such as the swallow, swift, corn-crake, or cuckoo, and the emergence of insect, reptile, fish, or hibernating mammal from winter's sleep.

The student is provided with a series of 365 pages, fittingly and instructively introduced, one being devoted to every day in the year. Each page is numbered both prospectively and retrospectively, showing not only the number of days or pages from the beginning, but to the end. These pages are partly blank, and upon the left-hand side the reader is told what to look for in the vegetable or animal kingdom, what flower may be expected to raise its head, or, as the season advances, what fruit may be expected to ripen. We are almost all of us keenly alive to the interest of watching the unfolding season, and a book of this kind embodying information already obtained, and inviting the reader to record his own observations on the same points, must commend itself to a large class of persons. Take as an example p. 133, or the 133rd day of the year, May 13, and we find that we should on this day "look out" for the green hair-streaked butterfly, the light tussock and rivulet moths, and the egg of the lesser whitethroat; we may also look for the spindle-tree in flower and the common mallow, although somewhat before their usual times. The blossom of the whitethorn, which is always known as "May," has been seen at Marlborough on April 30, and again has not been seen till June 4, information which is thus succinctly set forth, "*Cratægus oxyacantha*, 120-155, Hawthorn, Whitethorn, May," the figures indicating the earliest and latest days of the year upon which this favourite flower has been known to bloom.

There appears, indeed, to be no limit to the kind of things which an earnest student of Nature might not pleasantly note as affording material for his *Naturalist's Diary*. And so wide is now the net thrown, and so extraordinary are the correlations of science, that no fact need be passed over as unworthy of notice. For example, we are told in the introduction that "closely connected with the subject of migration, and equally deserving of systematic observation, is the congregation or flocking of birds in the autumn and winter months, as it is probably correlated with hibernation of fishes and reptiles." So that watching the loves of doves, and packing of partridges, listening to the early soft cooings of pigeons, or the crow of the pheasant, chronicling the advent of the cuckoo, or of "sweet Philomel complaining," or listening to the first strains of that "rapture so divine" which the immortal Shelley ascribed to our most sustained songster—in each case we may by accuracy of observation add a drop to the ocean of facts slowly developing into universal knowledge. Such a task could not fail of being attractive. Possibly it may tend to dissipate the sweet and more dreamy influences which steal over us insensibly while experiencing the gradual unfolding of Nature—the feeling so tenderly expressed by Longfellow in his exquisite prelude to the "Voices of the Night"; but this awakening from the poetic dream appears to be the fate of communities as well as of individuals, and we must, we suppose, resign ourselves to it. It is the province of science to ransack, to dissect, to arrange, to chronicle, and not to "babble o' green fields" only, as Dame Quickly said of poor Sir John Falstaff lying a-dying.

Downton, May 12 JOHN WRIGHTSON

OUR BOOK SHELF

Scientific Results of the Second Yarkand Mission, based upon the Collections and Notes of the late Dr. F. Stoliczka. "Araneida." By the Rev. O. P. Cambridge, M.A. (Published by order of the Government of India, Calcutta, 1885.)

WE have already on several occasions noticed the memoirs published by the Government of India on the collections made during this expedition to Yarkand. The spiders were placed in the very capable hands of the Rev. O. P. Cambridge for description. The collection cannot be considered as fairly representing the fauna of the extensive region traversed during the expedition, an area which Mr. Hume thinks might be subdivided into five well-marked regions, but which the author, judging from the collection of Araneida, conceives might have been well considered as but two: that is, (1) from Murree to Cashmere, including the latter as well as the former; and (2) the whole of the rest of the area travelled over by the Expedition, and comprising the neighbourhood of Leh, the route from Tantze to Chagra and Pankong Valley, and from Yarkand to Bursi, as well as Yarkand and neighbourhood, Kashghar, the hills west of Yarkand, and the Pamir.

In the former of these more than half of the whole number of spiders were collected—69 out of 132. The leading character of these is European, with a few more distinctly tropical and sub-tropical species. The character of the latter region is also European, but with decided sub-Alpine features, and scarcely a trace of any even sub-tropical form; and of the 69 species met with in the former three only were found in the latter, and only one, *Drassus dispulsus*, occurred throughout.

Of the 132 species, 23 seem identical with European species already described, leaving the large proportion of 109 as apparently new to science. Even this number cannot be supposed to represent the new species in the fauna of this region. The season of the year was very much against the success of the collection, and the hands of the collector were very much engaged with other branches of natural history; and there can be no doubt that a large harvest awaits the explorer of the southern slopes of the mountain regions of Cashmere, where the tropical character of the forms will become more marked; and probably a still greater diversity in the species will be found in those from the more central regions of India. For comparison upon these points the author regrets that there exist no materials, for almost nothing has as yet been published about the spiders of tropical India.

Two quarto plates with 21 figures of the more important new species accompany this Report.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Thomson Effect

I AM indebted to Dr. Everett for calling my attention to the confusion which has crept into § 193 of my book on "Heat." I had not noticed it; but, happily, it can easily be removed. Take to the end of the section the statement quoted by Dr.

Everett; and delete the word "Thus" in the sentence which, at present, (wrongly) follows instead of preceding it. This change is obviously called for by the context:—for the reader has just been told how far *theory* had guided Thomson as to certain "absorptions," &c., of heat; and, of course, expects next to be told what additional information, as to these "absorptions," &c., Thomson obtained by *experiment*.

Still, confused as it is, the passage could not (except possibly from the point of view of history) embarrass a reader of § 196; for the nature of the Thomson effect is there *again* clearly stated, and even illustrated by a diagram. [A much more serious case of confusion is to be found at p. 366, line 15; where (by the omission of a few words) my copyist has made absolute nonsense of a quotation from Clerk-Maxwell.]

The statement quoted by Dr. Everett obviously requires to be restricted, as follows:—

An electric current, passing from cold to hot in copper, behaves as a real fluid would do:—i.e. *it tends to reduce the gradient of temperature. In iron, under the same circumstances, it tends to increase the gradient.*

It is clear that this statement has nothing to do with the *general* nature of the Thomson effect:—*i.e.* "absorption" or "disengagement" of heat:—for *this* would depend upon the temperature of the fluid spoken of. It raises the question of the excess of Thomson effect in one locality, over that in another, at a lower mean temperature but with an equal gradient.

Dr. Everett seems to forget that, though the water-equivalent of a metal may be treated as sensibly constant through moderate ranges of temperature, the "specific heat of electricity" cannot so be treated. Using his notation, (with the proviso that θ is *absolute* temperature) we have $\sigma = k\theta$, and the equation he quotes from Thomson is

$$\frac{d\theta}{dt} = -\frac{k\theta}{c}\frac{d\theta}{dx}.$$

Happily, this can be integrated, so that we have

$$\theta = F\left(x - \frac{k}{c}t\theta\right) \; . \; . \; . \; . \; . \; . \; . \quad (1)$$

Now suppose the gradient of temperature to be uniform and positive along x positive (the direction of the unit current); when $t = 0$ we have

$$\theta = ex.$$

Generally, therefore,

$$\theta = e\left(x - \frac{k}{c}t\theta\right),$$

$$= \frac{ex}{1 + \frac{k}{c}et}.$$

Thus the gradient becomes less steep:—*i.e.* there is a tendency to reduce temperature differences, when k is positive, as in copper. In iron, where k is negative, the tendency is to make the gradient steeper:—*i.e.* to exaggerate differences of temperature. Of course, as in all these thermo-electric matters, reversal of sign of the gradient reverses the thermal effect.

The general integral (1) denotes a process of continued *simple shearing*, not *translation*, of the "temperature curve." Were it not for heat-conduction, harmonic waves of temperature would tend to become *breakers*. But it is idle to speculate farther.

How much of this is Thomson's I don't certainly know; and I am for the present too busy to enquire. But it would be difficult to overestimate his services to Thermo-electricity.

This will, I hope, meet with Dr. Everett's approval. As to his letter, I would say (in Scottish legal phrase) "*Quoad ultrà*, denied."

P. G. TAIT

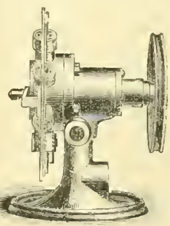
May 28

Power in Laboratories

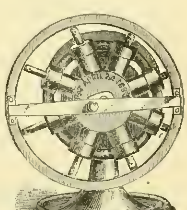
IN connection with the admirable devices for the distribution of driving-power in laboratories, illustrated in NATURE, vol. xxxiii. p. 248, the description of a novel and very effective form of water-engine, with which I have been experimenting for several months, will be of interest.

One of these motors is set up in the cellar of our science hall, where it is supplied with aqueduct-pressure of sixty pounds to the square inch, and the power is transmitted from it by means of rubber belting led over "idle pulleys" to the upper stories of the building, where a small engine-lathe and dynamo are driven. A word will suffice to explain the very simple construction of the motor—a system of radial cylinders, with their bases at the centre of the motor, through which runs the driving-shaft. The pistons in these cylinders are single-acting, and the water is admitted to them in succession by the rotary valve which forms part of the main shaft. The pistons, thus, in pressing outward, exert their force against a strong ring, to which is bolted a cross-bar which engages the crank of the main shaft. Thus the ring, in turning the shaft, has the vibratory motion of an eccentric, and returns the opposite pistons to the bases of the cylinders, at the same time exhausting the water through the interior of the rotary valve. Three pistons are thus constantly exerting a thrust upon the ring, whatever its position, and this thrust being always tangential to the arc of revolution of the crank, there is no "dead centre," and the uniform pressure at right angles to the crank at every part of its arc insures an even rotary motion and obviates the necessity of a balance-wheel. The ends of the piston-rods are slotted, and contain anti-friction rollers which bear against the ring, and this latter is grooved all round, so that, in addition to its simple and rapid motion as an eccentric, the ring is free to perform a slow motion of revolution independently of its work of driving the crank, and the wear of the interior face of the ring is thus equalised and becomes inappreciable.

The supply-pipe for this motor has a diameter of $1\frac{1}{2}$ inches, and it gives an equivalent of nearly 2 horse-power. The flow of water is regulated by means of a balanced valve, under control from every point where the power is used. As the use of the power is, for the most part, discontinuous, like that in lathe-work, I find it better to start and stop the motor as often as desired than to use the ordinary device of shifting a belt off and on a loose pulley. All possible economy of water is assured, as



Side View.



Front View.

none of it runs to waste without giving its equivalent of power at just the time when it is required. It will be seen that this form of motor is specially adapted to such uses, as there is no fly-wheel whose inertia has to be overcome; and as the motor has no "dead centre," it readily starts from any position, overcoming a maximum resistance.

Where continuous running is required, at an invariable speed, a centrifugal governor is attached to the belt-wheel, and acts upon the amplitude of vibration of the ring, diminishing the stroke of the pistons when the resistance is removed. The governor thus gauges the water-supply exactly proportional to the resistance to be overcome, and makes the motor a very effective driving-power for dynamos and all sorts of machines and apparatus in which a uniform speed is necessary, while the resistance is variable.

The difficulties barring the economic use of water as a motive-power, owing to its weight and incompressibility, seem to have been successfully overcome in this form of motor, with which unexampled speeds have been attained, and more than 80 per cent. of the theoretical power of the water derived. The little cut annexed shows the smallest size of these motors—it stands about 10 inches high, and uses a $\frac{1}{2}$-inch supply, consuming less than six quarts of water in 100 revolutions. I frequently run it at a speed of 1000 revolutions to the minute, and at the manufactory I have seen the same motor attain double this velocity. The motor runs equally well with compressed air (or with steam, if the piston-packings are changed), and with either of these media even higher speeds are attainable.

I find that the constant readiness of the motor for the immediate development of power, the little care it has required (only occasional oiling), and its economical consumption of water, are

very great advantages in its favour; and, for all laboratories supplied with aqueduct-pressure, I venture to think that it affords the best solution of the problem of inexpensive, convenient, and effective power. DAVID P. TODD

Lawrence Observatory, Amherst, Mass., May 15

Scientific Nomenclature

IN a letter published in NATURE for May 27 (p. 76) Prof. Minchin proposes to replace the expression "potential energy" by "static energy." It seems to have escaped his notice that a similar expression, proposed many years ago by Sir William Thomson, was used until it was replaced by the very words which Prof. Minchin wishes now to abolish. A short account of the question is given by Maxwell in "Matter and Motion," p. 81, and I should like to bring the following passage to the notice of those who take an interest in this question:—

"This is called the 'sum of the tensions' by Helmholtz in his celebrated memoir on the 'Conservation of Energy.' Thomson called it statical energy; it has also been called energy of position; but Rankine introduced the term potential energy—a very felicitous expression, since it not only signifies the energy which the system has not in actual possession but only has the power to acquire, but it also indicates its connection with what has been called (on other grounds) the potential function."

Harrow, June 8 G. GRIFFITH

Neæra

I WISH to request any of your readers who may dredge, or have opportunity this summer, to observe living or fresh specimens of the genus *Neæra*, Gray, and see whether branchiæ exist in that group. A Lamellibranch without branchiæ is anomalous, to say the least. I find in a new species of *Neæra* (sub-genus *Myonera*) from the Gulf of Mexico the following anatomical facts:—The mantle closed except for the small siphon and a narrow short slit for the thorn-shaped foot; no gills, no palps; the oral opening circular, plain; the roof of the peripedal cavity between the base of the body and the mantle margin is flattish, fleshy, with sparse pustules; a peripheral very stout pink muscle runs on each side around this, and is prolonged upward to the shell before the true adductor at each end of each valve, thus accounting for the double scars to be found there; the foot is close to the oral orifice, not grooved for the byssus, but pedunculated and surrounded by a groove; around the siphonal opening are numerous tentacular processes and a moderate number of ocelli. The specimens appear to be adult and perfectly preserved. An examination of specimens of *Neæra arctica* and *Neæra obesa*, Lovèn, indicated a similar state of affairs, though these specimens were not in as good condition as the one from the Gulf of Mexico. I do not find in the literature any categorical statement of the observation of gills in this genus. Clark is non-committal (in his "British Testacea"), Jeffreys speaks of seeing the "pink gills" through the shell, but that which he saw *pink* was without doubt the circular muscle I have mentioned.

The question is worthy of a definite solution. My specimens seem to leave no doubt that there are no gills, but it is always best to be suspicious of material long in alcohol.

WM. H. DALL

Smithsonian Institution, Washington, D.C., May 27

"Plants and their Defences"

WITH regard to the interesting article in your issue for May 6 (p. 5) on "Plants and their Defences," I should like to offer two remarks, and in return would be very glad to receive from you information upon a certain point. (1) The author enumerates different species of plants protected by the severe stings of ants, but does not seem to know the remarkable work of Beccari, "Piante ospitatrici ossia piante formicarie della Malesia e della Papuasia" (Malesia, vol. ii., Firenze, 1885). Beccari describes seventeen partly new species of "Myrmecophilous" Rubiaceæ, among which are eleven of Hyd*n*ophytum (not Hyd*ro*phytum, as is erroneously given in the article in NATURE). You will find a further contribution to this question in Henry O. Forbes's "Wanderungen durch den Malayischen Archipel," vol. i. pp. 84-88 of the German translation.

For my part, I should be greatly obliged if you would communicate to me the title of the original work from which the author of "Plants and their Defences" has taken his account of *Triplaris Schomburgkiana*, *Schomburgkia tibicinis*, and *Acacia sphærocephala*.

(2) Concerning the same article, Mr. Alfred W. Bennett (NATURE of May 20, p. 52) is inclined to think that the poisonous fluid of the nettle-glands is not formic acid, as generally conjectured, because the fluid frequently has an alkaline reaction. As a matter of fact, Prof. Dr. Haberlandt, at Graz (Austria), has recently, in vol. xciii. of *Sitzungsberichte des kaiserl. Akademie der Wissenschaften in Wien*, 1886, Februar-Heft, shown in his article, "Zur Anatomie und Physiologie der pflanzlichen Brennhaare," that (1) the poison of the stinging glands is not identical with formic acid; (2) nor is it the albumen dissolved in the fluid of the glands; but (3) that most probably this fluid is a transformed ferment or enzymotic poison.

Frankfurt a. Oder, June 2 E. HUTH

A Remarkable Hailstorm

ON April 17, at 4 o'clock p.m. (local time), a very remarkable hailstorm visited the neighbourhood of a small hamlet, called *El Totumo*,[1] not far from the town of Tinaco, section Cojedes, State of Zamora, Venezuela. The place is approximately in 9° 25′ N. lat., and 68° 5′ long. W. of Greenwich, certainly not more than 200 metres above sea-level. My informant is a resident of El Totumo, named Nicolas Moreno Nuñez, who is universally said to be a trustworthy and respectable man. There was first a very heavy thunderstorm with much rain; but after some time hailstones began to fall in such abundance that it might have been easy to collect them by hundreds of bushels, some weighing as much as two ounces. It is well known that between the tropics hailstorms are exceedingly rare in localities situated in the lowlands; but the present case is still more interesting, on account of the colour of the hailstones, some of which were *whitish, whilst others were blue or rose-coloured*. I have read of but one instance in which the two last-mentioned colours were observed, viz. in the hailstorm of Minsk of June 14, 1880, described by Lagunowitch, and quoted by Th. Schwedoff in his memoir "On the Origin of Hailstorms."[2] Schwedoff thinks that the blue and rosy colours are owing to the presence of salts of cobalt and nickel, and thus confirm his hypothesis of the cosmic origin of hail. I do not know whether the existence of those mineral constituents in the hailstones of Minsk was ever made certain by chemical analysis, and it is of course impossible for me to do so in the present case, when almost a month has passed since the phenomenon took place. But it is undoubtedly a very curious coincidence that the *same* colours should have been observed in both instances and in localities so widely separated from each other; whilst there is not the slightest possibility that my informant, an honest and plain countryman of no literary education whatever, should have had any knowledge of such an observation having been made before.

Caracas University, May 12 A. ERNST

[1] This is the vernacular name of the calabash-tree (*Crescentia Cujete*); there is, or was, probably a remarkable specimen of this tree in the neighbourhood of the hamlet.

[2] I only know a Spanish translation of Schwedoff's memoir, in *Crónica científica* (Barcelona), 1882, No. 120, pp. 553-60.

VISITATION OF THE ROYAL OBSERVATORY

THE visitation to the Royal Observatory by the Board of Visitors took place last Saturday, when there was a very numerous attendance. The report of the Astronomer-Royal to the Board gives, as usual, an account of the work done during the past year, and references to any points of interest or importance which have been raised. From the report we select the following particulars:—

Mr. Turner has recently investigated the discordance between observations for coincidence of the collimators made respectively through the apertures in the cube of the transit-circle and with the instrument raised. A wooden model of the cube was constructed through which the observation could be made when the transit-circle was raised, and it was thus shown that the discordance was due to the cutting off of portions of the object-glasses by the cube, and not to any effect of temperature. In view of this result it seems desirable that the optical

definition of the collimator object-glasses should be thoroughly tested.

A discussion of the collimation-observations made throughout the year 1885 with the reversion-prism shows that for the regular observers the personality depending on the apparent direction of measurement is extremely small. Any possible effect of the kind is eliminated by the method of observation adopted with the reversion-prism. The personality depending on the direction of motion, as deduced from the results of reversed and ordinary transits of clock-stars with the reversion-prism, appears to be more decided, though the amount is small except in the case of one observer.

The personal equation instrument was completed last autumn, but was dismounted during the winter to preserve it from injury in the bad weather. Series of observations have been taken with it on five days, and the results appear to be very satisfactory, the accordance being as close as could be expected. The absolute personal equations thus obtained seem to show that all the observers observe too late, the differences between the several observers agreeing well with the relative personal equations found from observations of clock-stars.

The sun, moon, planets, and fundamental stars have been regularly observed during the past year, together with other stars from a working catalogue containing about 2750 stars. Good progress has been made in the observation of these stars, in view of the formation at the end of 1886 of a Ten-Year Catalogue, epoch 1880·0. The annual catalogue of stars observed in 1885 contains about 1250 stars.

The following statement shows the number of observations made with the transit-circle in the twelve months ending 1886 May 20:—

Transits, the separate limbs being counted as separate observations	5685
Determinations of collimation error	306
Determinations of level error	332
Circle observations	5133
Determinations of nadir point (included in the number of circle-observations)	318
Reflection-observations of stars (similarly included)	530

The value found for the co-latitude from the observations of 1885 is 38° 31′ 22″·04, differing by 0″·14 from the assumed value; the correction to the tabular obliquity of the ecliptic is + 0″·81; and the discordance between the results from the summer and winter solstices is − 0″·98, indicating that the mean of the observed distances from the pole to the ecliptic is too great by + 0″·49.

The mean error of the moon's tabular place (computed from Hansen's Lunar Tables with Prof. Newcomb's corrections) is + 0·02s. in R.A. and + 0″·36 in longitude, as deduced from 105 meridian observations in 1885. The mean error in tabular N.P.D. is − 0″·58, which would appear to agree with the observations of the sun in indicating that the mean of the observed N.P.D.'s is too great.

The observations of the moon with the altazimuth have, as usual, been restricted to the period from last quarter to first quarter in each lunation, the total number of observations of various kinds made in the twelve months ending 1886 May 20 being as follows:—

Azimuths of the moon and stars	213
Azimuths of the azimuth-mark	146
Azimuths of the collimating-mark	176
Zenith-distances of the moon	110
Zenith-distances of the collimating-mark ...	172

The reversion-prism has been used during the past year with the altazimuth for investigation of the personality depending on the apparent direction of motion of stars or the moon. The plan adopted has been to observe a transit over the first three wires with a certain apparent direction of motion and over the last three with the apparent direction of motion changed. A second transit is then observed with the conditions reversed, so that in each double transit there are two sets of observations over the same six wires with the apparent direction of motion different, from which a determination of the personality is obtained by simple subtraction without any calculation of intervals of wires. The results show that the personality is in every case small, and that further observations are required to separate it from accidental errors of observation.

A clock synchronised by hourly currents, on Lund's system, has been presented to the Observatory by the Standard Time and Telephone Company, and is fixed in the Astronomer-Royal's office.

A new plane mirror (silver on glass) has been obtained from Mr. Calver for the Lassell equatorial, and a wooden screen has been fixed at the eye-end to protect the open end of the tube from the heat of the observer's body, it having been found that the definition was much affected by convection-currents at the eye-end, giving rise to an apparent astigmatism which was at first supposed to be caused by tilt of the large mirror. The optical performance appears now to be satisfactory. At Mr. Common's suggestion a frictional connection between the clock and the driving screw has been applied so as to allow of the latter being turned in either direction (for slow motion in R.A.) without putting the clock out of gear.

For determination of motions of stars in the line of sight, 378 measures have been made of the displacement of the F line in the spectra of 51 stars, and 21 measures of the *b* lines in 8 stars, besides measures of the displacements of the *b* and F lines in the spectra of Mars and Venus, and of the east and west limbs of Jupiter, and comparisons with lines in the spectrum of the moon, or of the sky, made in the course of each night's observations of star-motions, or on the following morning, as a check on the general accuracy of the results. The observations of Sirius during the past twelve months indicate, as in the last three years, a displacement of the F line towards the blue (corresponding to a motion of approach), the amount being slightly larger than in the preceding year. Spectroscopic observations were interrupted on a number of nights through deficient supply of water for the driving-clock of the south-east equatorial. After some correspondence with the Kent Waterworks Company it was arranged that the pressure in the Observatory main should not be reduced to so low a point in the evening, and no further trouble with the water supply has been experienced since. The experiments with the reversion spectroscope of the Oxford University Observatory (lent by Prof. Pritchard) indicate that this form of instrument is well adapted for observation of displacements, provided certain improvements can be effected in the optical and mechanical parts.

For the year 1885 Greenwich photographs are available for measurement on 208 days, and photographs from India and the Mauritius, filling up the gaps in the series, on 152 days, making a total of 360 days out of 365 on which photographs have been measured. The record has thus been made practically complete for 1885 by means of the Indian and Mauritius photographs.

A table of the means of daily areas of spots and faculæ and of the mean heliographic latitude of spots has been formed for each synodic rotation of the sun, and for each year from the commencement of the Greenwich series in 1873 to the end of 1885.

Further experiments for determination of the temperature corrections for the horizontal and vertical force magnets by alternately warming and cooling the basement on successive days were made in the spring of this year, a continuous record of the temperature being obtained by means of the Richard thermograph. The following are the results thus obtained in 1885 and 1886 as compared with the previous determinations, the appa-

rent changes for 1° of temperature being expressed in terms of the horizontal force and vertical force respectively:—

For 1° Fahr. increase of temperature	1869	1885	1886
Apparent decrease of horizontal force	·00018	·00026	·00021
	1882	1885	1886
Apparent increase of vertical force ...	·00020	·00022	·00020

The following are the principal results for magnetic elements for 1885:—

Approximate mean declination 18° 2′ west.

Mean horizontal force... ... { 3·9376 (in English units). 3·8156 (in metric units).

Mean dip ... ... { 67° 27′ 28″ (by 9-inch needles). 67 27 32 (by 6 inch needles). 67 28 27 (by 3-inch needles).

In the year 1885 there were only three days of great magnetic disturbance, but there were also about twenty days of lesser disturbance for which it may be desirable to publish tracings of the photographic curves. It is proposed to add tracings of the registers on four quiet days to serve as types of the ordinary diurnal movement at four seasons of the year, as was done for 1884.

The automatic drop of the Greenwich time-ball failed on two days during the past twelve months, on one occasion through accumulation of snow on the mast, and on another through failure in the clock-work apparatus for daily reversal of the currents through the electro-magnets. This apparatus has since been removed, and the direction of the currents is now reversed by hand once a week. On one day the ball was not raised on account of the violence of the wind.

As regards the Deal time-ball, there have been seven cases of failure owing to interruption of the telegraphic connections, and on three days the violence of the wind prevented the raising of the ball. There have been three cases of failure of the 1 p.m. signal to the Post Office.

No further action has been taken as regards the establishment of hourly time-signals at the Lizard or Start, as the arrangements for preliminary trial of a collapsible cone at Devonport are not yet completed. One of the Transit of Venus clocks (Dent 2010) has been adapted by Messrs. E. Dent and Co. to give hourly time-signals, and to be synchronised by the help of an auxiliary seconds' pendulum on the plan I proposed in the last report.

The longitude of Gibraltar was determined last year under Capt. Wharton's direction, by exchange of telegraphic signals on August 8, 9, and 12 between Greenwich and Gibraltar, the Eastern Telegraph Co. having courteously given the free use of their telegraph cable for the purpose. The signals were transmitted by relay-action from the ends of the cable to the observing-stations at Greenwich and Gibraltar. Local time was determined at Gibraltar by the officers of H.M.S. *Sylvia* with the sextant, and at Greenwich by Commander Moore and Lieut. Douglas by means of sextant observations, and also by transits with the transit-circle. In connection with this determination a large number of observations of signals were made at Greenwich for the determination of the personal equations of the different observers in observing telegraph signals. At Greenwich the longitude signals were observed by five observers independently. Commander Moore and Lieut. Douglas made a series of observations at Greenwich last summer for comparison of the relative value of determinations of local time made with a sextant and with a small transit instrument respectively.

The record of the past year shows that the work in all branches tends to increase. This increase could not well be resisted without impairing the efficiency of the Observatory, but year by year it causes more pressure on our limited staff, which, in addition to scientific work, is charged with the ever-increasing duties of a Government Office. In this connection I may mention that a good deal of my own time, as well as that of the Chief Assistant, has lately been occupied with various matters connected with the Navy, reference having been made to me on the subject of gun-directors, mirrors for electric search-lights, and binoculars, in all of which there are involved questions requiring careful consideration.

Commencing with the year 1885, Greenwich civil time, reckoning from midnight to midnight and counting from 0 to 24 hours, has been adopted in the spectroscopic and photographic results as well as in the magnetical and meteorological. It is proposed to defer the introduction of this time-reckoning into the astronomical results till the year 1891, for which year the Board of Visitors have recommended its adoption in the *Nautical Almanac*. In an Observatory such as this, where observations of various classes are carried on, there is, however, considerable inconvenience in the retention of the present astronomical day, which now involves the use of two systems of reckoning mean solar time in the same establishment.

The construction of an object-glass of 28 inches aperture and of 28 feet focal length, with suitable tube, to be mounted on the south-east equatorial, has been authorised by the Government, and the necessary funds have been provided in the estimates. The work has been intrusted to Mr. Grubb, with whom I have arranged the details of the tube, which is to be of special construction, adapted to the conditions of the mounting, and available for spectroscopy and photography as well as for eye-observations. Mr. Grubb proposes to provide means for readily separating the lenses of the object-glass to such a distance as will give the proper correction for photographic rays. Messrs. Chance are engaged in the manufacture of the glass for the lenses, and have already made a flint disk which promises to be very satisfactory.

In view of the recent development of astronomical photography, I propose to have constructed, for use with the present 12¾-inch refractor of the south-east equatorial, a combination of a convex flint and concave crown lens, which, when placed about 2 feet within the focus, would correct the chromatic aberration of the object-glass for the photographic rays without alteration of the focal length. If this plan succeeds, the instrument would then be well adapted for photography, thanks to the firmness of its mounting and the excellence of its driving-clock.

THE PAST WINTER

AT the meeting of the Royal Meteorological Society held on May 19 a paper was read on "The Severe Weather of the Past Winter, 1885-86," by Mr. C. Harding, F.R.Met.Soc. The paper dealt with the six months from October to March in a general way, and with the three months from January to March more in detail, as the latter embraced the period during which the weather was most severe, and in which both frost and snow were exceptionally prevalent. The material used in the discussion was for the most part contributed by the kindness of the Meteorological Council.

The greatest deficiency of temperature throughout the winter occurred in the weeks ending January 25, March 1, 8, and 15, the defect on the average amounting to as much as 9° and 10° over the greater part of England. During the fortnight ending March 15 the mean temperature was below the freezing-point in the Midland Counties and in the north-west of England, and, considering the British Islands as a whole, the temperature was lower during this fortnight than in any similar period of the winter. The means for each of the six winter months show that the temperature was below the average over the whole Kingdom in October, January, February, and March. In the east, south, north-west, and south-west of England, and the Channel Islands, as well as over the

greater part of the Midland Counties, and the north of Ireland, temperature was also below the average in December, whilst in the north-west of England and over a great part of the north of Scotland each of the six months was below the average. There was no part of the British Islands, except the Channel Islands, in which the temperature for each week, from the commencement of January until the third week in March, did not fall to the freezing-point or below, whilst in the south-west of England there was not a single exception after the first week in October, and in the east of Scotland, the north-east and north-west of England, the thermometer fell to 32° or below in each week from the commencement of November. The lowest shade-temperatures observed in the British Islands were: in January, − 2° at Braemar on the 19th, and 1°·1 at Alston on the 20th; in February, 2°·5 at Braemar and 7°·1 at Alston on the 5th; in March, − 2° at Alston and 1°·2 at Buxton on the 7th, and 2° at Braemar on the 12th. There were extremely few instances of the temperature falling below 5°; but temperatures below 10° were observed in January and March over the greater part of Great Britain.

From the commencement of January to the middle of March there was almost continuous frost, and during this period it froze for upwards of 60 nights at many places in the British Islands. At Great Berkhamsted the minimum temperature registered 32° or below in January 22 days, February 23 days, March 18 days, making a total of 63 days between January 3 and March 18; whilst on the grass it froze for 73 consecutive nights, from January 5 to March 18. At Cheadle in Staffordshire, Churchstoke in Montgomery, Llandovery in Carmarthen, and Great Berkhamsted in Hertfordshire, it froze for 33 consecutive nights, from February 14 to March 18, whilst at very many stations the frost continued 30 days or more. In Great Britain the longest period of frost occurred between the middle of February and the middle of March, but in Ireland it occurred generally in January. At Greenwich it froze on 28 consecutive days from February 19 to March 18; the observations from 1845 do not show another instance of frost continuing for so long a period without interruption. The only instances of 20 or more consecutive days are:—

24 days in 1858, from February 17 to March 12.
22 days in 1879, from November 20 to December 11.
21 days in 1855, from January 14 to February 3.
21 days in 1878, from December 6 to December 26.

For the three months from January to March there are but few years since 1845 that have a period of continuous frost of one-half the length of that in 1886. The years with fifteen days or more are respectively:—

1886 (28), 1858 (24), 1855 (21), 1861 (19), 1881 (16).

Taking the actual days with frost at Greenwich, irrespective of continuity, there was frost on 53 days in the present year (1886) from January to March. In 1855 the number of frosts in the corresponding period was 58, but the only other instance of more than 50 days was in 1858, when the number was 53.

Probably the most interesting feature in connection with the past winter was the excessively cold weather experienced over the whole country at the commencement of March. The Greenwich observations from 1814 only show two instances of a similarly low temperature—these were in 1814 and 1845. The unusual frequency with which snow fell was also a matter of interest, and the heavy drifts occasioned serious blocks on many of the northern railways.

The records of the London Skating Club show that there was skating on the Club water in Regent's Park on 38 days during the winter, and 1885–86 was the only winter in which there was skating in each of the four months from December to March since the formation of the Club in 1830, and the only March records of skating in the 56 years are 16 days in 1886, 12 days in 1844, 10 days in 1858, and 1 day in 1853. On a pond at Pinner there was almost continuous skating for 3 months, and and at Rickmansworth for about 70 days, but at both places the ice was most carefully nursed. On January 7 there was safe skating on snow-ice after one night's frost.

The temperature of the water in the Thames at Deptford was, on the mean, slightly in excess of the air. From January 8 to March 20 the entire range was from 40° to 34°; and from March 1 to 19 the maximum temperature was 36°·5 and the minimum 35°, showing a total range of 1°·5.

The recent temperatures observed at several stations over England show that at 1 foot below the surface the greatest cold for the winter was reached during the first 17 days of March. The mean was generally about 2° in excess of the mean air temperature. In January the earth temperature at 1 foot was from 2° to 3° below the average over the whole country, whilst in February it was from 4°·5 to 6°·5 below the average; the first 17 days of March, however, show a much larger defect on the average, the deficiency ranging from 6°·3 at Lowestoft to 8°·5 at Norwood. The temperature of the soil at 2 feet was generally about 2° in excess of that at 1 foot.

The logs of ships traversing the North Atlantic show that the abnormal conditions which prevailed over the British Islands and indeed over nearly the whole of Europe extended also a considerable distance to the westward. They show a decided tendency to a low barometer, during the early months of 1886, in the locality where a high barometer generally prevails, and to the north of this low barometer strong and persistent easterly winds were experienced. These facts tend to show a general reversal of conditions over the Atlantic which would doubtless be very intimately related to our own exceptional weather.

THE ASTRONOMICAL DAY

THE recently published report of the Science and Art Department contains some most important information showing what the recent Government action has been in relation to the resolutions passed at the Washington Conference.

The first letter that we need refer to is one from the Astronomer-Royal, in April last year, suggesting that reference should be made to various scientific Societies, in order to obtain an authoritative expression of opinion from the scientific men in this country interested in the question. This was followed by a meeting of the committee appointed to advise the Science and Art Department on the matter. The following resolution was adopted by the committee, which consisted of Prof. Adams, the Astronomer-Royal, General Strachey, Captain Sir Frederick Evans (since deceased), Captain Wharton (the Hydrographer), and Colonel Donnelly:—"The committee recommend that the report of the British Delegates to the Washington International Prime Meridian Conference, with the resolutions adopted by that body, be communicated to certain Departments of State, learned Societies, telegraph companies, &c., and that they be informed that the resolutions appear to be such as commend themselves for adoption; but before informing the American Government to that effect they would be glad to receive their opinions on the subject."

The Science and Art Department then addressed a letter to various public offices, scientific bodies, and telegraph companies. Their replies may be thus condensed.

The Eastern Telegraph Company, and the Eastern Extension, Australian, and China Company, state that they have always adopted the twenty-four-hour system in timing their messages, thus avoiding the necessity of signalling the letters a.m. and p.m.

The Society of Telegraph-Engineers and Electricians cordially approve of the first six resolutions of the Washington Conference, but they reserve their opinion as to the seventh (the one referring to the application of the decimal system to space and time).

The Royal Astronomical Society forwards the following resolution:—

"The Council of the Royal Astronomical Society desire to express their concurrence in the resolutions of the Washington Conference, and consider it desirable that the reckoning of astronomical time from mean midnight be adopted in the Nautical Almanac for 1890, the earliest practicable date, and that it be thenceforward adopted by astronomers."

The India Office writes that "the Government of India will be perfectly prepared to accept whatever conclusions may be arrived at by Great Britain after the discussions which will doubtless precede any final decision modifying the practice of astronomers, navigators, or others in this country in the reckoning of longitude or time."

The Board of Trade thinks that the resolutions are such as commend themselves for adoption.

The Royal Society forwards a report drawn up by a specially appointed committee, which the Council of the Society adopted:—

"The committee recommend the Council to approve of resolutions 1 to 6.

"With regard to resolution 6, if the change of time-reckoning be generally adopted, and can without inconvenience to mariners be made in the nautical almanacs of all nations for 1890, the committee recommend that year for the change to be made.

"With regard to the seventh resolution the committee would remark that, for astronomical reasons, the division of angular space is bound up with the division of time, and that a decimal division of the day would be opposed to the practice of, we may say, all nations, from very early times to the present day.

"Such a change the committee conceive ought not to be made without the gravest consideration. The committee observe, however, that the resolution does not appear to go beyond the expression of a hope that the subject may be further studied, to which of course there can be no objection."

The Eastern and South African Telegraph Company give the same reply as that given by the two Eastern Companies to which we have already referred.

The Submarine Telegraph Company does not adopt the twenty-four-hour system.

The Office of Works has no observations to make.

The Colonial Office has no objections to offer.

The views of the Admiralty were thus stated in a letter dated July 1885:—

"My Lords desire me to inform you that this question has engaged their attention since the receipt of the communication from the Science and Art Department of the 29th May, but that, seeing how many and varied are the interests involved in a proposal to make any alteration in methods of reckoning time which have for so many hundreds of years prevailed, they have thought it desirable, before offering any opinion, to obtain full information on the results which would follow, and the effect which it might have both on seamen and astronomers, more especially as the main responsibility of action would finally rest on their Lordships, as controlling the production of the Nautical Almanac.

"When their Lordships have received the report of the Board of Visitors to the Royal Observatory, Greenwich, who, as eminent astronomers, have been consulted, they will be in a position to give an opinion; but as they have been advised that that body cannot conveniently meet, without undue haste, until the autumn, their Lordships regret that, for the present, they cannot furnish any definite reply to the question asked by the Lords of the Committee of Council on Education."

The General Post Office letter states that there will be no objection to the adoption of a legally authorised system of counting time from zero to twenty-four hours, and that for some purposes it seems to possess advantages over the present system. The Postmaster-General is inclined to think, however, that, in the case of his Department in particular, the introduction of the system should depend upon popular feeling.

The Elder Brethren of the Trinity House see no objection to the immediate adoption of the sixth resolution, that as soon as may be practicable the astronomical and nautical days will be arranged everywhere to begin at mean midnight.

The above letters are given in the order in which they were received by the Science and Art Department.

In January of the present year, the Admiralty sent a second letter, which we give *in extenso*:—

"SIR, *Admiralty, 5th January*, 1886

"I am commanded by my Lords Commissioners of the Admiralty to inform you that they have had under their careful consideration your letter of the 29th May last, inclosing a copy of the resolutions passed by the International Conference for fixing a Prime Meridian and Universal Day, held at Washington in October 1884, and asking their Lordships' opinion thereon.

"2. The first five of these resolutions, causing as they do a minimum of change in the customs of this country, cannot but meet with their Lordships' unqualified approval, but do not appear to call for any action on their part.

"3. My Lords do not consider that the seventh resolution demands any remark from them.

"4. With regard, however, to the sixth resolution, which proposes a fundamental change in the mode of reckoning astronomical time, my Lords are deeply interested, not only so far as it may affect Her Majesty's Navy, but in consequence of the responsibility for the publication of the Nautical Almanac being vested by Act of Parliament solely in them.

"5. My Lords are of opinion that the sixth resolution may be regarded in two different lights:—

"1st. It may be considered as the natural corollary of the adoption of a universal time, such time being a civil day at the Prime Meridian; because, should universal time be adopted (for scientific and certain other purposes) the disagreement between civil and astronomical time, if retained, would to a great degree render nugatory the endeavour to introduce uniformity. In this aspect the change would seem to depend upon the adoption of universal time.

"2nd. This proposed change may also, however, be looked upon as intrinsically desirable in itself, besides as in a measure facilitating the adoption of universal time; and in this light action may be taken before any international consensus is arrived at with regard to universal time as recommended by the Washington Conference.

"6. Before, however, coming to any final conclusion on the advisability of sanctioning such changes in the ephemeris as would be necessary to give effect to this resolution, my Lords have felt bound to consult, both as to the principle and in respect of details, those other classes who habitually use the Nautical Almanac, viz., the mercantile marine and astronomers, as represented respectively by the Board of Trade and by the Board of Visitors to Greenwich Observatory, the latter being the most representative body of astronomers to whom my Lords could appeal.

"7. My Lords find that, while there is a general agreement in the desirability of putting an end to the present dual system of reckoning time, the urgency of the change is differently regarded by seamen and astronomers.

"8. Astronomers are now apparently finding many difficulties in the present duplication of time, and are desirous of a speedy change.

"9. At sea it causes but little practical inconvenience, as the two systems do not come into collision, being used for totally different purposes; and my Lords agree with the opinion expressed to them by the Board of Trade, that the change will not be unattended with risk from the possibility of mistakes during the period of transition, and that it must be made with all possible precautions.

"My Lords also fully recognise that the fact of the change rendering the existing epitomes and text-books of navigation to a great extent useless must receive due consideration from several points of view.

"10. It does not, however, appear to my Lords that there is sufficient reason to cause them to place obstacles in the way of making the change desired by British astronomers and many seamen, and recommended by the unanimous votes of the Delegates of the Washington Conference, as they consider that the rearrangement of the Nautical Almanac may be so carried out as to minimise the above-mentioned risks.

"11. My Lords will, therefore, be prepared to sanction such alterations in the Nautical Almanac as will be necessary to establish the change to the new reckoning at a date sufficiently far in advance to give ample warning to seamen.

"12. As, however, the fundamental objects in view of the Washington Conference were, to simplify and unify the modes of reckoning time, to remove present discrepancies, and to endeavour to establish an international system, it would appear that no decided move of any kind should be made until the views of other nations, and more especially those maritime powers which publish astronomical ephemerides, are ascertained. It would be manifestly contrary to the interests of simplification that England should alter the practice of centuries only to find herself alone in the new method of reckoning astronomical time; nor would it be courteous to announce her intention of so doing without consulting other Governments on the steps proposed by their representatives, but not plenipotentiaries, at the Washington Conference.

"13. My Lords will, therefore, be pleased to learn that the opinions and intentions of the other maritime nations have been ascertained at as early a date as practicable, in view of the wishes of British astronomers.

"I am, &c.

"(Signed) EVAN MACGREGOR.

"The Secretary, Science and Art Department,
"South Kensington, S.W."

After the receipt of the second letter from the Admiralty another meeting of the committee was held, and the following report was drawn up for the information of my Lords:—

"Your committee find that the Science and Art Department having consulted the various bodies named in the accompanying list, the first five of the resolutions of the Washington Prime Meridian Conference have received unanimous approval, but demand no action on the part of this country.

"As regards the sixth resolution, which proposes that as soon as may be practicable the astronomical and nautical days shall be arranged everywhere to begin at mean midnight, it appears that the opinion in England is generally in favour of this change in the mode of reckoning astronomical time, and that the Admiralty have expressed their willingness to take the necessary steps to give effect to this resolution of the Conference by introducing civil reckoning into the British Nautical Almanac, the rearrangement of which they are satisfied may be so carried out as to minimise risks from mistakes by navigators during the period of transition, if other maritime nations are prepared to adopt the proposed method of reckoning astronomical time.

"Under these circumstances your committee suggest that the Foreign Office be invited to communicate this result of the inquiries of the Science and Art Department to the Government of the United States, and to inquire whether, as conveners of the Washington Conference, they are now prepared to take steps to invite the adhesion of other maritime States."

Next follows a letter from the Science and Art Department to the Foreign Office, asking them to make the inquiry referred to in the previous report, and another from our ambassador at Washington, stating that the United States Government had taken the matter in hand.

FACILITIES FOR BOTANICAL RESEARCH

IN an article under the above heading, published in NATURE, vol. xxxi. p. 460, I endeavoured to draw the attention of our younger botanists to the importance of extending their studies over a wider field than is at present usual, and mentioned some easily accessible stations at which students might observe tropical vegetation. Since that article was written, I have had the opportunity of acting on my own suggestion, and of visiting Ceylon; I am therefore now in a position to enlarge upon my previous suggestions, and to fill in from personal experience many details which, though often trivial in themselves, may yet bring the possibility of Eastern travel home to the mind of some in such a way as may lead to future action. But while giving some account of the facilities for botanical work in the East, care must be taken not to over-colour the picture; it happens too often that writers of an enthusiastic bent raise expectations in the minds of their readers, which actual experience can only disappoint: in the following paragraphs I shall endeavour to make a purely matter-of-fact statement, and leave the colouring to be filled in at the will or opportunity of the reader. Taking first Peradeniya, we may consider what are its attractions as a station for botanical work, and then pass on to discuss the relative merits of other stations.

In the first place, hardly any port in the east is more accessible than Colombo: it has been aptly called the "Clapham Junction" of the East: the steamers of all nations meet there, and the competition between them produces a moderate scale of fares. Once there, a direct train service lands the traveller in about three hours almost at the gate of the Royal Gardens; the mechanical discomforts of many a journey to remote districts in the United Kingdom are greater than this. The cost of the journey will vary according to the line of steamers selected; by the Peninsular and Oriental line a return ticket can be had from London to Colombo for 90*l.*, 100*l.*, or 110*l.*, according as the return journey is completed in three, six, or twelve months. The charges on the Messageries Maritimes are about the same. The Star, Clan, and British India lines make more moderate charges, but the pace is correspondingly slower. It is little use making a journey of more than 5000 miles for a brief visit; and it may be presumed that, except where the circumstances are extraordinary, students would find it convenient to stay in Ceylon for three or four months, or more. Little is to be gained by scamping an expedition such as this, in which it may often happen that a man may gain his first and last experience of tropical nature; further, the surroundings are so new that it is some little time before one with even a good knowledge of our temperate flora can accommodate himself sufficiently to them to carry on successful work. We may then regard the cost of the journey as 100*l.*, and the time required to make it a success about six months. The choice of season is an important point: in a country of alternating wet and dry

periods it is well to experience both, and for the botanical collector it is important that collections should be finally made up in dry weather; it would be found that leaving England in November, and landing at the beginning of December, the weather would still be wet and vegetation luxuriant, but preservation of dry species would be difficult: a gradual change would be experienced, till in February and March the dry and hot season would have come in, vegetation would be more or less checked, and the preservation of dry specimens would be easy. Returning towards the end of March the English winter would be past, and, if he be a teacher, the traveller would be in time for the summer session in our Universities or medical schools.

Once on the spot the first question is one of accommodation. At Peradeniya there are neither hotels nor lodgings; a house must be taken and temporarily furnished, and it is surprising how cheaply this can be done. I took a small bungalow, the rent of which was Rs. 40 per month; friends lent me some articles of furniture, and an expenditure of Rs. 150 supplied all else that was required for temporary housekeeping. The cost of keeping house, including the wages of two native servants, rent, &c., with a margin for incidental expenses, may be set down at about Rs. 250 per month; allowing further some Rs. 200 for travelling expenses, it will be found that Rs. 1,500 will represent the total necessary expenses of residence in the island for four months. But in its present depreciated state, the rupee is worth only about *1s. 6d.*: it will therefore be sufficient to lodge about 115*l.* at a bank in Ceylon to cover all necessary expenses for four months' residence. I would not advise, however, that that exact sum only should be transferred; it would be more prudent to allow a margin for possible contingencies. The total expenses of a six months' trip to Peradeniya may thus be set down as 250*l.* But there are various ways in which strict economy might reduce the cost, while if two friends were to club together, their individual expenses for housekeeping would be considerably below the sum above stated for one. Thus it will be seen that neither in difficulty of transit nor in point of expense are there sufficient obstacles to prevent a visit to Peradeniya, or some similar station, finding a place in the programme of the career of an average botanist. One of the chief obstacles will be felt by many to be the loss of possible opportunities while absent, or the break in continuity of teaching, or other work in which a man may be already engaged. I venture to think that these are much overrated objections; and against them may be set the very great advantages which a tropical visit carries with it. A further question is, at what period in a man's career will a visit of this sort best repay him? Some will say immediately after taking his degree: but I am inclined to think that even a first-class man is at that time hardly prepared to make the best of the opportunity should it offer. The experience gained by a few years of teaching and of original work at home will indicate what is to be expected and what is to be looked for, and will fit a man in many ways for striking out new lines for himself, even if it have not already defined for him a clear line of research. On the other hand, it is important that travelling should be undertaken before a man settles in life, so that his mind may be as free as possible from distractions and anxieties.

We may now pass on to consider what are the specific advantages presented by Peradeniya as a station for botanical work. It is, as I have said, easily accessible; being more than 1500 feet above the sea, the excessive heat of the low country is avoided, and it may be regarded as a decidedly healthy place. Secondly, it is situated in a central position, both as regards the whole island, and as regards the chief lines of communication by rail and road. Thus it is easy to gain access to the low country by train to Colombo, whence roads, traversed often by horse coaches, will lead along the coast, or inland in various directions; or, taking the up-country line, Newara Eliya may be visited, which lies about 6000 feet above the sea, and would serve as a good centre for working the higher levels; or again, a journey northwards by train and coach to Anuradhapura would give an insight into the low-level vegetation of the drier northern districts. From the above notes it will be readily seen how varied is the character of the country within easy reach from Peradeniya, presenting within a comparatively small area districts varying from the sea-level to 8000 feet, and including both damp and relatively dry areas at low levels. This in itself would lead one to expect a rich and varied flora; and in fact the list of native plants now numbers some 3000 species, a very considerable proportion of which are peculiar to the island. These and other natural advantages are, however, eclipsed by the importance of the Royal Gardens themselves as providing a field for those hitherto unfamiliar with tropical nature. Here there are collected in a small area a great variety of species, both native and imported; truly no botanist who has resided at Peradeniya can any longer complain of want of scope; if he does not find ample material for future work, he can only lay the blame on his own want of imagination. In the excellent herbarium and library, as well as in the fine series of coloured figures of native plants which are lodged in the Garden, he would find the greatest assistance in recognising and naming plants collected; while lastly, in the presence of the Director, who is the best living authority on the flora of Ceylon, are found those social and scientific elements which go far to enhance the pleasure of a visit to Ceylon.

In my former article mention was made of Java, where the Gardens of Buitenzorg, presided over by Dr. Treub, present great attractions for botanists. In my case, shortness of the time at my disposal prevented a visit to this famous Garden, and probably the same difficulty will present itself to others. There is, however, one conspicuous advantage which it possesses over Peradeniya as a station for botanical research, viz. a well appointed laboratory. If, as seems not improbable, a journey to the tropics and a period of steady work among tropical plants become a usual prelude to a career of active teaching in botany, ought not the English to provide themselves with some suitable station for such work? Is every man, whether well-to-do or impecunious, to depend upon his own resources alone for laboratory accommodation, reagents, glass, and all other accessories necessary for his work? or are we to be content to send our botanists to suck what advantage they can from the hospitable Dutch, just as we send our forestry students to study with the French? Surely it would be a most legitimate way of extending the usefulness of the Garden at Peradeniya, and, in a small way it is true, of guarding the credit of England as a tropical Power, to establish a laboratory for the use of travellers. It need not be a large or conspicuous building. Dr. Trimen tells me that suitable accommodation for the present could be found in the buildings already standing in the Gardens, and probably 100*l.* would cover the initial cost of supplying the bare necessaries of life in the laboratory. The knowledge that such accommodation would be found at the other end would certainly encourage those who are doubtful to undertake a journey to Ceylon.

It may be noted that no mention has been made of the Western tropics as a field for research; there can be no doubt as to the richness of the field, but I am not aware that there are any stations in the West which can compare with Peradeniya or Buitenzorg in convenience, accessibility, and general adaptation to the requirements of those who contemplate only a comparatively short visit.

Lastly, the cost of the journey will be found to be the most frequent deterrent from undertaking it; 250*l.* is a large sum to spend upon six months' work which can bring no direct financial return, however great may be the ultimate advantage gained from it; travelling Fellowships are

few; but still there are other sources from which grants may well be made to assist really promising students in attaining so desirable an end; and it is to be hoped that it may be regarded as a legitimate and not unfrequent outlet for public or private grants, to enable young men, who will ultimately engage in teaching, towards the attainment of experience which must always be of value to them in the exercise of their profession.

F. O. BOWER

NOTES

THE following is the list of Fellows elected into the Royal Society on Friday last, June 4:—Shelford Bidwell, M.A., William Colenso, F.L.S., Harold B. Dixon, F.C.S., Major-Gen. Edward Robert Festing, R.E., Andrew Russell Forsyth, M.A., Prof. A. H. Green, M.A., Prof. Victor Horsley, F.R.C.S., Raphael Meldola, F.R.A.S., Philip H. Pye-Smith, M.D., Henry Chamberlaine Russell, B.A., Adam Sedgwick, M.A., Prof. W. Cawthorne Unwin, B.Sc., Robert Warington, F.C.S., Capt. William James Lloyd Wharton, R.N., Henry Wilde.

ARRANGEMENTS are being made by the officers of several French Societies for holding an International Congress at Biarritz for discussing papers upon climatology, mineral and thermal springs, and allied subjects. A letter has been received from the Foreign Office transmitting copies of documents, and stating that the French Government is anxious that members of scientific Societies in this country should assist. The co-operation of the Royal Meteorological Society has also been specially asked by the President of the Congress, Dr. Durand Fardel. The sittings at Biarritz will occupy the first week in October, and be followed by a three weeks' tour to the principal watering-places of Southern France. Fellows of the Royal Meteorological Society will be allowed to travel over all French railways at half price. For further particulars apply to the Assistant Secretary of the Society.

THE Council of the Society of Arts have awarded the Society's silver medals to the following readers of papers during the Session, 1885-86:—To Prof. Francis Elgar, LL.D., for his paper on the load-lines of ships; to Henry Davey, for his paper on machinery in mines; to Prof. W. C. Unwin, for his paper on the employment of autographic records in testing materials; to C. V. Boys, for his paper on calculating machines; to Prof. Leonard Waldo, D.Sc., for his paper on watch-making by machinery; to John Mackenzie, for his paper on Bechuanaland and Austral Africa; to Edward Combes, C.M.G., for his paper on the industries and commerce of New South Wales; to G. Gordon Hake, for his paper on Cyprus since the British occupation; to Prof. W. N. Hartley, F.R.S., for his paper on photography and the spectroscope in their application to chemical analysis; to Prof. R. Meldola, for his paper on the scientific development of the coal-tar colour industry; to B. H. Baden Powell, C.I.E, for his paper on Indian manufactures from a practical point of view; to Capt. Richard Carnac Temple, for his paper on the every-day life of Indian women. Thanks were voted to the following members of the Council for the papers read by them:—To Capt. Douglas Galton, D.C.L., C.B., F.R.S., for his paper on results of experiments on mechanical motors for tramways made by the Commission at the Antwerp Exhibition; to W. H. Preece, F.R.S., for his paper on domestic electric lighting.

THE Society of Arts *conversazione* will be held, by permission of the Royal Commission, at the Colonial and Indian Exhibition, South Kensington, on Friday, July 16 next.

MR. TALFOURD ELY has resigned the Secretaryship of University College, London.

THE Russian Geographical Society has awarded this year its great gold medal to M. Yurgens for his remarkable work as chief of the Arctic Meteorological Station at the mouth of the Lena. The Medal of Count Lütke has been awarded to Col. Pyevtsoff for his most valuable account, full of new and interesting information, of his journey in N.W. Mongolia and Northern China, published, with a map, in the fifth volume of the West Siberian Branch of the Society. Great gold medals have been awarded to M. Dmitrevsky for his annotated translation of Otano Kitoro's work on Corea, and M. Tereshkevitch for his statistical description of the Government of Poltava. Small gold medals were awarded, to Prof. Lenz for his work in the capacity of President of the Physical Geography Section of the Society; to M. Fuss, for his calculations of the great levelling through Siberia; to the Director of the Tiflis Observatory, M. Milberg, for his magnetical observations carried on in connection with those of the Polar stations; and to M. Mainoff, for his work on the customary law of the Mordovians. Several silver medals were distributed to MM. Gedeonoff, Fedoroff, Krasnoff, and Ignatieff for astronomical, geological, and botanical works; to several persons who have sent observations on thunderstorms and rains, as also for various ethnographical and statistical researches.

THE Town Council of Banff, along with the Council of the Banffshire Field Club and Office-Bearers of the Banff Literary Society, have formed themselves into a General Committee (with power to add to their number) to promote the subscription of a fund for the erection of a memorial in Banff to the memory of the late Mr. Thomas Edward, A.L.S., "The Scottish Naturalist." The Committee feel sure it will be the desire of many throughout the whole nation to contribute to this fund, and to combine to make the memorial worthy of the universal admiration and respect entertained for Mr. Edward. In order to afford full opportunity for this, it is proposed to add to the Committee ladies and gentlemen throughout the various parts of the country who so appreciate Mr. Edward's life and work as to be willing to interest themselves in providing some substantial and suitable perpetuation of his memory. Communications should be addressed to the Interim Secretary, Mr. John Allan, Town Clerk of Banff.

THE Prince of Wales, considering that the rates of admission to the Colonial and Indian Exhibition at South Kensington should be brought within the means of all classes residing in the Metropolitan area, is making arrangements with the Railway Companies and other bodies in a position to co-operate in the organisation of a scheme whereby every working man, woman, and child will have an opportunity of visiting the Exhibition at greatly reduced prices on every week-day except Wednesday from the middle of August until the close of the Exhibition. Arrangements for enabling the working classes of the provinces to visit the Exhibition have been for some time in operation, under his Royal Highness's direction.

THE first of the conferences convened by the Geologists' Association on "The Mineral Resources of the Colonies and India," was held at the Colonial and Indian Exhibition on Saturday afternoon (June 5), when a lecture was delivered by Prof. V. Ball, F.R.S., on "The Mineral Resources of India and Burmah." The discussion brought out the urgent need for reform of the mining laws of India, and the following resolution, proposed by the chairman (Sir R. Temple), seconded by the Duke of Manchester, and supported by the lecturer and others, was unanimously adopted:—"This Conference having had under its review the mineral resources of India and the obstacles to development and exploitation of the same through the want of suitable or sufficient mining laws, respectfully urges upon the Secretary of State for India the desirability of regulating or revising the regulations for the working of mines in British India, including Burmah, and for the protection of mining interests

therein, and also of negotiating arrangements to the same effect with the Native States." The next conference will be held on Saturday, June 19, when Prof. T. Rupert Jones, F.R.S., will lecture on "The Mineral Resources of South Africa." Sir Charles Mills will preside.

THE latest news from Catania reports that the eruption of Mount Etna has almost entirely ceased. The lava-streams, stopping within 300 metres of Nicolosi, have become hard enough to walk on, but repeated shocks of earthquake of considerable violence continue throughout the region. A huge gulf has yawned at Acireale, but no disasters are reported. A correspondent of the *Standard* writing from Catania on May 27 sends some valuable notes. "The stream of lava issuing from Monte Penitello (an eminence about 4980 feet high, and about 7 kilometres distant from Nicolosi) has travelled a longitudinal course of about 6 kilometres, and has formed four basins, the last of which is behind Monte Nocella. The width of the stream is about 2½ kilometres. The stream flows directly southward from the north-east. The western branch, which flows towards Dogola Arcino, has done but little damage, traversing in its course mostly gravelly soil. Prof. d'Amico, who is studying the phases of the eruption from the Observatory of Pennesi, in Acireale, has contributed to the archives of Catania a complete record of all the volcanic disturbances which he has noted up to date. The maximum number of 92 telluric convulsions was registered on the 19th inst. On the following day there were only 20, but subsequently the number rose from 25 to 30, 27, 35, and finally to 52, on Tuesday the 25th."

SEVERAL shocks of earthquake were felt at Smyrna, in the Island of Chios, and in the Erythrian Peninsula on the night of June 5.

PROF. MILNE recently made an attempt to sound the depth of the crater of Asamayama, the active volcano about seventy miles to the north-west of Tokio, and in the historic period one of the most destructive of all Japanese volcanoes. The attempt however failed owing to unfavourable weather. No details of the experiment or of the precise manner in which it was to be carried out have been received as yet.

WE have already referred to the threatened abolition, of alteration, of the Imperial College of Engineering in Tokio, in consequence of administrative changes which have resulted in the abolition of the Department of Public Works, under which the College was placed. As the names of several men well known in the English scientific world have been associated with this institution, we are glad to be able to say that the change has not been so great as was at first apprehended. Mr. Mori, the new Minister of Education, long the representative of Japan in this country, has established a new University, to which the Engineering College has become affiliated, as well as the former Medical College. Except in two important details, the organisation remains the same. The College now comes under the exclusive control of Japanese, in other words, the post of Principal, hitherto held by Dr. Divers and his predecessor, is no longer to be occupied by a foreigner, and the whole University is to be governed by its professors, and not by ordinary officials. The idea, says the *Japan Mail*, is "to emancipate learning and its professors from the too often ignorant and always hampering control of the ordinary official." Everything now is subject to the professorial body. Japan is not to lose the services of any of the foreign professors of the College. Dr. Divers continues as Professor of Chemistry, but in the Science instead of the Engineering College. Prof. Milne continues in the Chair of Mining Engineering, and there is a probability that he will also be invited to occupy a Chair of Seismology which it is proposed to found. It thus appear that the change is not one which interferes with the work of the College; if anything its status as an educational institution is increased, because it now forms an integral portion of the highest teaching establishment in the country. It is not clear from the account from which we quote whether the College will remain in the fine buildings which were specially erected for it, or whether it will be removed to the University, which is some distance away.

IN connection with this change, the professors and students of the College, meeting for the last time as members of the Engineering College, presented the Principal, Dr. Divers, with a handsome bronze vase, inlaid with gold. Prof. Milne, as the senior, in handing the present to Dr. Divers on behalf of the subscribers, made a short speech referring to the services of the Principal to Japan, his pupils, and colleagues; and with this ceremony an institution in which English science has, or should have, a peculiar interest, came to an end, but only to rise again in another shape, let us hope for increased usefulness to Japan.

THE Chair of Seismology which has been founded in the Science College of the new University of Japan has been filled by the appointment of Mr. K. Sekiya as professor.

AT Cambridge to-day (Thursday) a vote of the Senate is to be taken in favour of appointing a syndicate to obtain plans and estimates for the erection of a Geological Museum, to confer with the Sedgwick Memorial Committee, and to report before the middle of the Michaelmas term. Thus it appears likely that at last the Sedgwick Museum will become an actual fact, after twelve years of incubation.

THE current number of the *Proceedings* of the Bath Natural History and Antiquarian Field Club contains several papers of scientific interest. Mr. Broome continues his list of fungi found in the neighbourhood of Bath. Mr. Blomefield, writing on the firs of Bournemouth, argues that these trees are true natives of that part of the country, in opposition to the theory that the Scotch fir, though indigenous in Scotland, is not indigenous in England. Dr. Norman describes the fresh-water algæ of the Bath thermal waters; and Mr. McMurtrie the salt-springs found at great depths in the Coal-measures at Radstock.

FUNGI also occupy a considerable space in the *Proceedings* of the Belfast Naturalists' Field Club, as in those of the Bath Society, for Mr. Lett gives a list of the fungi of the North of Ireland which fills more than 50 pages. Dr. Malcolmson describes some recent *Ostracoda* of Belfast Lough, and appends an elaborate table of their distribution. Mr. Joseph Wright adds two lists—one of the Foraminifera of Down and Antrim, the other of the Cretaceous Foraminifera of Keady Hill in the county of Derry. The record of the meetings is very full, and contains much information on various scientific subjects connected with the North of Ireland.

ARRANGEMENTS have been made by the Colonial and Indian Aquarium authorities to bring over a consignment of some of the principal species of fish in the waters of Java for purposes of exhibition. The British India Steamship Company have undertaken to transmit them in specially made glass carriers, which have been forwarded to Java for that purpose. Fourteen specimens of the *Chelone midas*, or green turtle, have lately arrived at the Aquarium from West India; also a collection of land crabs from the same quarter.

THROUGH the instrumentality of the National Fish-Culture Association the River Lea was last week enriched with a quantity of salmon fry, which were turned into those *locales* most suitable to their requirements Although the Lea is in a very polluted condition, there are portions free from contamination where fish thrive well. The Severn Fishery Board have turned into their river a large number of salmon fry. The ova from

which they were hatched out were collected in the first instance by the Board and forwarded to the National Fish-Culture Association to incubate, which was done most successfully. The Severn Fishery Board are to be commended upon the action they have taken to replenish their river with fish, as they set an example to other bodies having the interests of their waters at heart. The hatchery at South Kensington and Delaford belonging to the Association might become an extensive medium in carrying such an object into effect at a minimum cost.

THE additions to the Zoological Society's Gardens during the past week include a Nisnas Monkey (*Cercopithecus pyrrhonotus*) from Nubia, presented by the Rev. W. MacGregor; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. J. Coston; a Common Badger (*Meles taxus*), British, presented by Mr. C. A. Ross; six Black-footed Penguins (*Spheniscus demersus*) from South Africa, presented by Capt. John Hewat; four Siamese Blue Pies (*Urocissa magnirostris*) from Siam, two Small Hill-Mynahs (*Gracula religiosa*) from Southern India, a Rufous necked Weaver Bird (*Hyphantornis textor*) from South Africa, presented by Mr. J. M. Cook, F.Z.S.; a Golden Eagle (*Aquila chrysaetos*) from Russia, presented by Mr. Walter Holdsworth; six Long eared Owls (*Asio otus*), British, presented by Mr. G. B. Burnand; a Malbrouck Monkey (*Cercopithecus cynosurus*) from West Africa, three Ruffs (*Machetes pugnax*), British, deposited; a Glaucous Macaw (*Ara glauca*) from Paraguay, four Crested Pigeons (*Ocyphaps lophotes*) from Australia, four Amherst's Pheasants (*Thaumalea amherstiæ*) from China, two Great American Egrets (*Ardea egretta*) from South America, two Lapwings (*Vanellus vulgaris*), British, four Indian Tree Ducks (*Dendrocygna arcuata*) from India, purchased; a Japanese Deer (*Cervus sika*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

COMET BROOKS II.—The following ephemeris for this comet is by Prof. C. Frisby (Science Observer Special Circular, No. 67):—

For Greenwich Midnight

1886	R.A. h. m. s.	Decl. ° ′	Log r	Log Δ	Brightness
June 11	6 23 20	69 15·0 N.	0·0380	0·1783	0·26
15	6 58 55	66 54·1	0·0557	0·2015	0·21
19	7 27 41	64 28·3	0·0734	0·2275	0·17
23	7 39 51	62 58·9	0·0909	0·2486	0·14
27	7 59 57	60 49·5 N.	0·1081	0·2704	0·12

The brightness on May 2 is taken as unity.

COMET BROOKS III.—Dr. S. Oppenheim has calculated the following elements and ephemeris for this comet from observations made on May 25, 28, and 30, at Arcetri, Rome, and Vienna:—

$$T = 1886 \text{ June } 2{\cdot}90285 \text{ Berlin M.T.}$$

$$\left.\begin{array}{l} \omega = 173^\circ\ 57'\ 49''{\cdot}6 \\ \Omega = \ \ 47\ \ 14\ \ 43{\cdot}5 \\ i = \ \ 16\ \ \ 8\ \ 52{\cdot}3 \end{array}\right\} \text{Mean Eq. } 1886{\cdot}0.$$

$$\log q = 0{\cdot}170230$$

Ephemeris for Berlin Midnight

1886	R.A. h. m. s.	Decl. ° ′	Log r	Log Δ	Brightness
June 8	12 18 3	1 1·8 S.	0·1709	9·9183	0·87
12	12 26 1	3 32·8	0·1721	9·9274	0·83
16	12 34 25	6 1·6	0·1739	9·9375	0·79
20	12 43 15	8 27·4	0·1763	9·9486	0·75
24	12 52 27	10 49·2	0·1793	9·9605	0·70
28	13 1 59	13 6·1 S.	0·1828	9·9732	0·65

The brightness on May 25 is taken as unity.

The comet is faint, and not bright as stated in the telegram announcing the discovery.

SPECTROSCOPIC DETERMINATION OF THE MOTION OF THE SOLAR SYSTEM IN SPACE.—Dr. R. von Kövesligethy mentions (*Astronomische Nachrichten*, No. 2731) that some three years ago he tried to deduce the speed with which the sun is travelling in space and the point to which its progress is directed, from the observations of the displacements of lines in stellar spectra published in the *Monthly Notices*. The latter inquiry he gave up, as the data supplied did not seem sufficiently trustworthy for a satisfactory result to be obtained from them. He therefore assumed the apex as found from the discussion of the proper motions of stars, viz. R.A. = 216°·0, Decl. = 35°·1 N. Taking the simple arithmetical mean of the observations of the individual stars observed—about 70 in number—he found the speed of translation of the solar system to be about 8·6 geographical miles per second. This rate of motion would agree far better with Struve's value, derived from the consideration of the proper motions of stars, than Herr Homann's (NATURE, vol. xxxiii. p. 450) result does. Dr. Kövesligethy does not, however, place much reliance on the result he has thus obtained.

PUBLICATION OF THE ZONE-OBSERVATIONS OF THE "ASTRONOMISCHE GESELLSCHAFT."—M. Doubiago, who has succeeded the late Marian Kowalski as Director of the Kasan Observatory, has recently issued a volume containing the observations made at Kasan during the years 1869 77, of the stars situated in the zone between 75° and 80° of north declination. The principal object of this work, undertaken by M. Kowalski by arrangement with the *Astronomische Gesellschaft*, was the determination of the positions of the stars contained in this zone down to the ninth magnitude. M. Kowalski, however, determined to include in his work all the stars of the *Bonner Durchmusterung* situated in the above-mentioned zone, about 5000 in number, as well as a considerable number of fainter stars. The observations, commenced in 1869, were finished in 1879, and the present volume contains 14,329 observations, that is, about half the total number necessary to complete the projected scheme of having four observations of each star. The results are given in the usual form in which zone-observations are published, viz. the apparent positions for each day of observation are given, together with the reduction to the mean place for the beginning of the year. As far as we remember, Kasan has the honour of being the second of the observatories engaged on the zone work of the *Astronomische Gesellschaft* which has published their observations, Prof. Krüger having already published his Helsingfors zones (55° to 65° of north declination) in two volumes, the first volume having appeared in 1883, and the second in 1885.

THE MADRAS OBSERVATORY.—Mr. Pogson's report for the year 1884 has recently been issued. He points out that during the year a work on "Telegraphic Longitude Determinations in India" was printed and published. The number of observations made with the meridian circle during the year was 844, which brings up the total number of observations made with this instrument since 1862, now awaiting publication, to 51,722. The separate results and annual catalogues will fill eight volumes, to be followed by a final catalogue of about 5000 stars, reduced to the epoch 1875. All the reductions are completed up to date, and Mr. Pogson hopes that these volumes will appear in fairly rapid succession. We hope so too. The speedy publication of a catalogue of 5000 stars would do much towards restoring the Madras Observatory to the position, as a scientific institution, which it formerly held.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 JUNE 13-19

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on June 13

Sun rises, 3h. 45m.; souths, 11h. 59m. 45·2s.; sets, 20h. 15m.; decl. on meridian, 23° 14′ N.: Sidereal Time at Sunset, 13h. 43m.

Moon (Full on June 16) rises, 16h. 50m.; souths, 21h. 51m.; sets, 2h. 44m.*; decl. on meridian, 13° 26′ S.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	3 43	12 7	20 31	24 26 N.
Venus	2 0	9 10	16 20	12 52 N.
Mars	11 36	18 1	0 26*	4 7 N.
Jupiter	12 5	18 22	0 39*	2 32 N.
Saturn	5 2	13 13	21 24	22 41 N.

* Indicates that the setting is that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich)

June	Star	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image
			h. m.	h. m.	° °
18	B.A.C. 6536	6½	2 59	4 7	85 334
19	B.A.C. 7145	6½	21 40	22 18	113 186

Variable Stars

Star	R.A.	Decl.		
	h. m.	° ′		h. m.
U Cephei	0 52·2	81 16 N.	June 14,	1 55 *m*
			,, 19,	1 35 *m*
R Persei	3 22·8	35 17 N.	,, 18,	*M*
S Cancri	8 37·4	19 17 N.	,, 19,	21 27 *m*
W Virginis	13 20·2	2 47 S.	,, 17,	21 35 *m*
δ Libræ	14 54·9	8 4 S.	,, 13,	0 24 *m*
			,, 19,	23 58 *m*
U Coronæ	15 13·6	32 4 N.	,, 13,	22 59 *m*
U Ophiuchi	17 10·8	1 20 N.	,, 16,	1 28 *m*
			,, 16,	21 35 *m*
X Sagittarii	17 40·4	27 47 S.	,, 19,	2 20 *M*
W Sagittarii	17 57·8	29 35 S.	,, 18,	0 0 *m*
U Sagittarii	18 25·2	19 12 S.	,, 19,	2 25 *m*
β Lyræ	18 45·9	33 14 N.	,, 14,	0 0 *M*
S Vulpeculæ	19 43·7	27 0 N.	,, 15,	*M*
η Aquilæ	19 46·7	0 43 N.	,, 19,	2 30 *M*
R Lacertæ	22 38·2	41 47 N.	,, 16,	*M*

M signifies maximum ; *m* minimum.

BIOLOGICAL NOTES

DEVELOPMENT OF OPHIOPHOLIS AND ECHINARACHNIUS.—In the last series of studies from the Newport Marine Zoological Laboratory we find a memoir by Mr. Walter Fewkes, on the development of an Ophiuroid (*Ophiopholis aculeata*, Gray) and of an Elypeastroid (*Echinarachnius parma*, Gray). But few observations have been published on the metamorphosis of Ophiopholis, and these often misleading. The eggs would appear to be extruded separately into the water, and the young pass through a metamorphosis in which a pluteus-larva is formed ; the development of this pluteus is different from that of any described Ophiuran, though allied to that in Ophiothrix. The ova were voluntarily shed by the female on August 17 ; they were fertilised outside the body, and appeared to be very hardy. The yolk has a central and a peripheral region, which are distinguishable in the eight-cell and previous stages of segmentation. The cleavage is like that of other Echinoderms. A gastrula is formed by the invagination of the blastoderm, and consequently the stomach of the pluteus is an infolded wall of the blastoderm, and not formed by delamination from the cells in the cavity. The mesoderm-cells originate in two lateral clusters. The oldest pluteus observed was a little more than three days old ; they, however, appeared to be easily raised, and it is to be hoped that they will be yet traced to an adult form. In Echinarachnius the sexes are distinct, and in some cases there were colour distinctions. In the experiments on the ovum of *E. parma*, artificial fertilisation was resorted to from the middle of July to the end of August ; it was easily effected. In its mode of segmentation it resembles that of other Echinoderms. It has no polar globules, but possibly these may be formed while the egg is in the ovary. As in some other Echinoderms, a gastrula is formed by invagination. The pluteus figured by A. Agassiz in the revision of the Echini as probably that of Echinarachnius proves to belong to this species at about a week old. The development of the young Echinarachnius on the water-tube of the pluteus resembles that of other sea-urchins. The rosette-form of the water-tubes described in other Echinoderms also occurs. The first-formed calcareous deposits of the test are trifid in form, and vary in number in different specimens. The extremity of each trifid division bifurcates later in its growth, and the calcareous body thus formed appears to be inclosed in a transparent wall, which has a spherical outline. Spines are very early formed, and are proportionately very large as compared with those of the adult. The various stages are illustrated in numerous figures on eight, in several cases folding, plates.—*Bull. Mus. Comp. Anat. Harvard College*, vol. xii. No. 4, March 1886.

THE LEECHES OF JAPAN.—In the April number of the *Quarterly Journal of Microscopical Science*, Dr. C. O. Whitman publishes the first hundred pages of a memoir on the leeches of Japan. The material for the study was collected while the author was connected with the University of Tokio (1879–81). The coloured drawings accompanying the memoir were executed by Mr. Nomura, a young Japanese artist, and they well deserve the remarks of the author: "Mr. Nomura's attention to the minutest details, his infinite patience, trained eye, and his remarkably skilful brush, have given results that are marvels for neatness and accuracy." The first part of this study treats of the land leech (*Hæmadipsa japonica*), the medicinal leech (*Hirudo medicinalis*), and of three species of toothless leeches, which form a new genus, Leptostoma. It also contains a comparison of a few species from Europe, Asia, and America, and a considerable portion is devoted to a comparative study of the different genera, in the endeavour to find a satisfactory basis of classification. This has in a great measure been found in a law of abbreviation of the somites, which, in addition, gives a key to the phylogeny of the genera. The land leech is shown to be a highly instructive and specialised form. The genus Hirudo has been re diagnosed (p. 364), and while internal structure has been dealt with to only a very limited extent, still some interesting facts in connection with the nephridial organs of the land leech are detailed, and the existence of from twelve to fourteen sense-organs on the first ring of each complete somite is demonstrated, and they are homologued with the eyes, having possibly also other sense-functions. The author reserves the genus Hæmadipsa for the land leeches of Ceylon, India, and Japan, with three jaws and five inter-genitalia rings. He ascribes the genus to Tennant, but may it not have been formed by Baird? *H. japonica* is confined to the mountain slopes and ravines, never descending into the plains. It is not only a mountain leech, but it keeps habitually to the ground, living in moss, or under damp leaves and rubbish. They are most voracious, and on the approach of man or beast are at once on the alert. They advance by rapid strides. They bite so gently as scarcely to attract attention, but the wound is deep, and the scar is more or less permanent. They gorge on for about 30 to 40 minutes, and then drop ; while sucking they become bedewed with a transparent liquid, which keeps them moist. If placed in water, they do not swim but sink, and then creep out ; and while having a decided preference for a terrestrial life, can support life for days in water. If into a jar of hungry leeches a puff of breath is blown, they become immensely excited, and it will be difficult to keep them in ; while trying to keep back one, a dozen others will rush out. In a most interesting series of paragraphs Dr. Whitman traces the intimate relation that exists between these land and the medicinal leeches, the latter essentially fresh-water forms. The geographical area of land-leeches is mainly within the tropics, though in Japan they are exposed to a wide range of temperature. *H. nipponia* is described as a new Japanese medicinal leech, well known to the Japanese, and with habits and mode of life just like our European leech. Leptostoma, a new genus, is established (p. 376) on three species of almost edentulous leeches, which, though having a common ancestry with Hirudo, were not derived from it. All three species, *L. acranulatum*, *L. edentulum*, and *L. pigrum*, are from Tokio, and are fully diagnosed and beautifully figured. The segmental organs are shown to be sense-organs, and that from them the eyes have developed, so that they may be regarded as incipient eye spots.

NEW ELEMENT OF THE BLOOD AND ITS RELATION TO COAGULATION.—In an important paper by Mr. Geo. T. Kemp on this subject, he comes to the conclusion that in addition to the red corpuscles and leucocytes the blood normally contains a third histological element—the "plaques." These have been variously considered as young red corpuscles ; as nuclei floating in the blood ; as being derived from the red or the white corpuscles ; as being fibrin ; and as being globular depositions produced by cooling of the blood ; but the author proves that, although strong resemblances exist between the plaques and other histological elements of the blood, there is not yet sufficient evidence to establish a *genetic* connection. The plaques should therefore, at least for the present, be regarded as independent elements. When the blood is drawn, the plaques break down almost immediately, and this is not true of any other element of the blood. This breaking down of the plaques seems intimately connected, in its time relations at least, with the clotting of the blood. If a good-sized drop of blood from a finger be let fall on a cover-glass, and as quickly as possible washed by a good jet of ·75 per cent. NaCl solution, and then examined under the

microscope, the plaques, which have a property of sticking to the glass slip, will be found to fill the field; some will be isolated, some will be in groups; they will now appear glistening and granular, and their contours are jagged, becoming more and more so as time elapses; finally only a granular mass will be found. If, however, a drop of osmic acid be placed on the finger before the drop of blood be drawn, all the elements will be found presenting their normal appearances, and the plaques will be seen as pale homogeneous structures varying greatly in size, but for the most part about one-third or one-fourth of the diameter of the red corpuscles; they are biconcave, but not as much so as the red corpuscles. Once thus hardened they never change their form, but the plaques first referred to will be found to alter their form very speedily, and *pari passu* with these changes, processes are seen which run out from the granular masses, and when coagulation sets in these processes are nearly always found to be continuous with threads of fibrin. The connection between the breaking down of the blood is not histological but chemical. The plaques appear to give up a soluble substance which is active in coagulation. This active agent is most probably a *fibrin* ferment. Fibrin is deposited histologically independent of any of the cellular elements of the blood, and when the clot is very scant. The fibrin is seen deposited as long, needle-shaped, crystal-like bodies. — *Studies from Biolog. Lab., Johns Hopkins Univ.*, vol. iii. No. 6, May 1886.

ON RECENT PROGRESS IN THE COAL-TAR INDUSTRY[1]

II.

AZO-COLOURS.—Amongst the most important of the artificial colouring matters may be classed the so-called azo-colours. These colours are chiefly bright scarlets, oranges, reds, and yellows, with a few blues and violets. They owe their existence to the discovery by Griess, in 1860, of the fact that the so-called azo-group – N = N – can replace hydrogen in phenols and amido-compounds. But it is to Dr. O. N. Witt that is due the honour of having given the first start in a practical direction to the chrysoidine class of azo-colours by the discovery of chrysoidine, and perhaps still more so by the suggestions contained in a paper read before the Chemical Society. Dr. Caro, of Mannheim, was also acquainted with several compounds which belong to this class at the time Witt published his results, but it does not appear that he made practical use of them until Witt introduced the chrysoidines and tropeolines. To Roussin, of the firm of Poirrier of Paris, is due the credit of having first brought into the market some of the beautiful azo-derivatives of naphthol. Griess, therefore, as the original discoverer of the typical compounds and reactions by which the azo-colours are obtained, may be considered as the grandfather, whilst Roussin and Witt are really the fathers, of the azo-colour industry. Nor must it be forgotten that it is to Perkin we owe the recognition of the value of the sulpho group in relation to azo-colours, a discovery patented in 1863. Moreover it is interesting to note that changes in colour from yellow to red and claret are effected by the increase in the molecular weights of the radicals introduced as well as by the relative positions occupied by these groups.

Indophenol.—Witt is also the discover of a new blue dye-stuff termed indophenol, which has been used as a substitute for indigo. Certain difficulties, however, have arisen in the adoption of this colour on the large scale. The most important use indophenol is at present put to is for producing dark blues on reds dyed with azo-colours, both on wool and cotton. The piece goods are dyed a uniform red first, and then printed with indophenol white; for like indigo itself indophenol yields a colourless body on reduction, and this being a very powerful reducing agent destroys the azo-colour, being itself transformed into indophenol blue. The process works with surprising nicety and is very cheap. The blue is formed and the red discharged with such precision that patterns can be produced in which the blue discharge covers a great deal more space than the original red. This new printing process was devised by Mr. H. Koechlin, of Lorrach. The reds used for the purpose are in the case of wool the usual azo-scarlets, for cotton Congo red.

Artificial Indigo.—About five years ago the speaker had the honour of bringing before this audience[1] the remarkable discovery made by Baeyer of the artificial production from coal-tar products of indigo blue. Since that time but little progress has been made in this manufacture, as the cost of the process, unlike the case of alizarin, has as yet proved too serious to enable the artificial to compete successfully in the market with the natural indigo.

Through the kindness of a number of eminent colour manufacturers in this country and on the Continent, the speaker was enabled to illustrate his subject by a most complete series of specimens both of the colours themselves and of their application to the dyeing and printing of fabrics of all kinds. His thanks are especially due to his friend, Mr. Ivan Levinstein, of Manchester, for the interesting series of samples of cloth dyed with known quantities of fifty different coal-tar colours, each having a different chemical composition; also to the same gentleman, and to Messrs. Burt, Boulton, and Haywood, of London, for the interesting and unique series of specimens indicating the absolute quantities of products obtainable from *one ton of coal*, as well as for much assistance on the part of Mr. Levinstein in the preparation of the experimental illustrations for this discourse. To Dr. Martius of Berlin for a valuable series of colours, especially the well-known Congo red, made by his firm, including samples of wool dyed therewith, he is also much indebted. For the interesting details concerning indophenol and its applications the speaker owes his thanks to Dr. Witt and M. Koechlin.

Coal-tar Antipyretic Medicines.—Next in importance to the colour industry comes the still more novel discovery of the synthetical production of antipyretic medicines.

Up to this time quinine has held undisputed sway as a febrifuge and antiperiodic, but the artificial production of this substance has as yet eluded the grasp of the chemist. Three coal-tar products have, however, been recently prepared which have been found to possess strong febrifuge qualities, which if still in some respects inferior to the natural alkaloids, yet possess most valuable qualities, and are now manufactured in Germany at Höchst and at Ludwigshafen in large quantity. And here it is well to call to mind that the first tar colouring-matter discovered by Perkin (mauve) was obtained in 1856 during the prosecution of a research which had for its object the artificial production of quinine.

In considering the historical development of this portion of his subject, the speaker added that it is interesting to remember that the initiative in the production of artificial febrifuges was given by Prof. Dewar's discovery in 1881 that quinoline, the basis of these antipyretic medicines, is an aromatic compound, as from it he obtained aniline. Moreover that Dewar and McKendrick were the first to observe that certain pyridine salts act as febrifuges. So that these gentlemen may be said to be the fathers of the antipyretic medicines, as Witt and Roussin are of the azo-colour industry.

Kairine, the first of these, was discovered by Prof. O. Fischer, of Munich, in the year 1881, whilst engaged on his investigations of the oxyquinolines. The febrifuge properties of this substance were first noticed by Prof. Filehne, of Erlangen. Kairine is manufactured from quinoline, a basic product derived from aniline by heating it with glycerin and nitrobenzene by the following process. When treated with sulphuric acid, SO_4H_2, it forms quinoline sulphonic acid, and this when fused with caustic soda yields *oxyquinoline*, which is then reduced by tin and hydrochloric acid into tetrahydroxyquinoline, and this again on treatment with C_2H_5Br yields ethyl-tetraoxyquinoline or kairine. The lowering of the temperature of the body by this compound is most remarkable, though, unfortunately, the action is of much shorter duration than that effected by quinine itself; but on the other hand, with the exception of its burning taste, it exerts no evil effects such as are often observed after administration of large doses of quinine. The commercial article is the hydrochloride, the price is 85*s.* per lb., and the quantity manufactured has lately diminished owing to the discovery of the second artificial febrifuge, antipyrine.

The following graphical formula shows the constitution of kairine:—

$$C_6H_3(OH)\begin{array}{l}\diagup CH_2CH_2 \\ \diagdown N(CH_3)CH_2 \\ \quad\;\diagup\;\diagdown \\ \quad H \quad Cl\end{array}$$

[1] A Discourse by Prof. Sir Henry E. Roscoe, M.P., LL.D., F.R.S., delivered at the Royal Institution, Friday, April 16, 1886. Continued from p. 114.

[1] "On Indigo and its Artificial Production," *Proc. Roy. Inst.*, May 27, 1881.

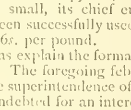
Antipyrine, the second of these febrifuges, was discovered in 1883 by Dr. L. Knorr in Erlangen, and its physiological properties were investigated by Prof. Filehne, of Erlangen. The materials used in the manufacture of antipyrine are aniline and aceto acetic ether. The aniline is first converted into phenylhydrazine, a body discovered by Emil Fischer in 1876. This body combines directly with aceto-acetic ether, with separation of water and alcohol, to form a body called pyrazol ($C_{10}H_{10}N_2O$). The methyl derivative of pyrazol derived by treating it with iodide of methyl, is *antipyrine*, its composition being $C_{11}H_{12}N_2O$. As a febrifuge, antipyrine is superior in many respects to kairine and even to quinine itself. It equals kairine in the certainty of its action, whilst in its duration it resembles quinine. It is almost tasteless and odourless, is easily soluble in cold water, and takes the form of a white crystalline powder. Its use as a medicine is accompanied by no drawbacks. It occurs in commerce in the free state. The production of antipyrine, in spite of these valuable qualities, is as yet small, its chief employment being in Germany, where it has been successfully used in cases of typhoid epidemic. The price is 6*s*. per pound.

The following equations explain the formation and constitution of this interesting body. The foregoing febrifuges are manufactured at Hochst under the superintendence of Dr. Pauli, to whose kindness the speaker is indebted for an interesting series of specimens illustrative of the manufacture of antipyrine.

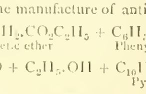
$$\underset{\text{Aceto-acetic ether}}{CH_3.CO.CH_2.CO_2C_2H_5} + \underset{\text{Phenylhydrazine}}{C_6H_5.NH.NH_2}$$
$$= H_2O + C_2H_5.OH + \underset{\text{Pyrazol}}{C_{10}H_{10}N_2O}$$

$$C_{10}H_{10}N_2O + ICH_3 = \underset{\text{Antipyrine-hydriodide}}{HI.C_{10}H_9(CH_3)N_2O.}$$

Dr. Knorr formulates pyrazol thus:



And antipyrine is



The antipyretic effect of this compound is strikingly shown in the following temperature readings in a case of typhoid kindly communicated to the speaker by his friend Dr. Dreschfeld of Manchester. Each of the second set of readings was made two hours after a dose of 30 grains of antipyrine had been administered.

I.	II.	Diff.
°	°	°
105·0	103·0	2·0
103·5	100·2	3·2
103·8	100·8	3·0
105·2	101·4	3·8
104·4	100·6	3·8

Thalline.—The third of the artificial febrifuges is *thalline*, which is offered as the tartrate and sulphate. It is manufactured by the Badische Company. Thalline is said to be used as an antidote for yellow fever. Its scientific name is tetrahydroparaquinanisol, and it was first prepared by Skraup by the action of methyl iodide and potash on paroxyquinoline.

We must, however, bear in mind that none of these synthetical febrifuges are antiperiodics, and therefore cannot be employed instead of the natural alkaloid quinine in cases of ague or intermittent fevers.

Coal-tar Aromatic Perfumes.—A third group of no less interest comprises the artificial aromatic essences, and of these may here be mentioned, in the first place, *cumarin*, $C_9H_6O_2$, the crystalline solid found in the sweet woodruff, in Tonka bean, and in certain sweet-scented grasses. This is now artificially prepared by acting upon sodium salicyl aldehyde with acetic anhydride by the reaction which is associated with the name of Dr. Perkin, and is used in the manufacture of the perfume known as "extract of new-mown hay."

A second interesting case of a production of a naturally occurring flavour is the artificial production of *vanillin*, the crystalline principle of vanilla. Vanilla is the stalk of the *Vanilla planifolia*, which incloses in its tissues prisms of crystalline vanillin, to which substance it owes its fragrance. Tiemann and Harrmann showed that vanillin is the aldehyde of methyl protocatechuic acid—

$$C_6H_3(OH)(OCH_3)CHO, [CHO : OCH_3 : OH = 1 : 3 : 4].$$

The chief seats of the vanilla productions are on the slopes of the Cordilleras north-west of Vera Cruz in Mexico, also the island of Réunion, and in the Mauritius. Since the discovery of the artificial production of vanillin, the growth of the vanilla has been very much restricted.

A variety of vanilla, termed vanillon, obtained in the East Indies, has long been used in perfumery for preparing "essence of heliotrope." This contains vanillin together with an oil, which is probably oil of bitter almonds. The essence of white heliotrope is now entirely prepared by synthetical operations. It is manufactured by adding a small quantity of artificial oil of bitter almonds to a solution of artificial vanillin; when these substances are allowed to remain for some time in contact, the mixture assumes an odour closely resembling that of natural heliotrope. Through the kindness of Mr. Rimmel the speaker was able to render the fragrance of this coal-tar perfume perceptible to his audience. Nor must we forget to mention the so-called essence of mirbane (nitrobenzene), of which about 150 tons per annum are used for perfuming soap; and artificial oil of bitter almonds, employed as a flavour in place of the natural oil.

Coal-tar Saccharine.—Of all the marvellous products of the coal-tar industry, the most remarkable is perhaps the production of a sweet principle surpassing sugar in its sweetness *two hundred and twenty* times. This substance is not a sugar, it contains carbon, hydrogen, sulphur, oxygen, and nitrogen. Its formula is

$$C_6H_4{<}\begin{matrix}CO\\SO_2\end{matrix}{>}NH,$$

and its chemical name is benzoyl sulphonic imide, or for common use, saccharine. It does not act as a nutriment, but is non-poisonous, and passes out of the body unchanged. The following is a concise statement of its properties, and mode of production from the toluene of coal-tar. It should, however, be first mentioned that the compound benzoyl sulphonic imide (saccharine) was first discovered by Constantin Fahlberg and Remsen, in America. But no patent was taken out for a commercial process till recently, and it is now patented in this country.

STEP I.—Toluene is treated with fuming sulphuric acid in the cold, or it is heated with ordinary sulphuric acid of 168½° Twaddell on the water bath, or not above 100° C. The latter method is the better. The acid is best caused to act upon the toluene in closed vessels rotating on horizontal axles.

$$\underset{\text{Toluene}}{C_6H_5CH_3} + SO_4H_2 = \underset{\substack{\text{Toluene sulphonic acids}\\\text{(ortho and para).}}}{C_6H_4\left\{\begin{matrix}CH_3\\SO_2.OH\end{matrix}\right.} + H_2O.$$

STEP II.—After all toluene (which as toluene is insoluble in the acid) has disappeared, the contents of the agitating vessel are run into wooden tanks in part filled with cold water, and the whole liquid is stirred up with chalk to neutralise the excess of sulphuric acid used and to obtain the two isomeric toluene sulphonic acids as calcium salts.

$$2\underset{\substack{\text{Toluene, ortho- and}\\\text{para-sulphonic acids}}}{\left(C_6H_4\left\{\begin{matrix}CH_3\\SO_2.OH\end{matrix}\right.\right)} + SO_4H_2 + 2(CaCO_3) =$$

$$\underset{\substack{\text{Calcium toluene ortho-}\\\text{and para-sulphonates}}}{\left(C_6H_4\left\{\begin{matrix}CH_3\\SO_3\end{matrix}\right.\right)_2Ca} + CaSO_4 + 2CO_2 + 2H_2O.$$

The neutralised mass is filtered through a filter-press to separate therefrom the precipitate of gypsum, which is washed with hot water, and the washings added to the filtrate.

STEP III.—The calcium salts are now treated with carbonate of sodium, to obtain the sodium salts, with precipitation of carbonate of calcium. The precipitate is removed by means of a

filter-press from the solution containing the sodium ortho- and parasulphonates.

$$\left(C_6H_4\begin{cases}CH_3\\SO_3\end{cases}\right)_2Ca + Na_2CO_3 = CaCO_3 + 2C_6H_4\begin{cases}CH_3\\SO_3ONa.\end{cases}$$

The sodium toluene sulphonates

STEP IV.—The solution of the sodium salts from III. is evaporated either in an open or in a vacuum-pan so far that a portion taken out will solidify on cooling. The contents of the pan are then run into moulds of wood or iron, and allowed to cool and solidify. The lumps are at length taken from the moulds, broken up small, and dried in a drying-room, and subsequently in a drying apparatus heated with steam, until quite desiccated.

STEP V.—The sodium sulphonate salts are now converted into their corresponding sulphonic chlorides. This is effected as follows:—The dried sulphonates are thoroughly mixed with phosphorus trichloride, itself as dry as possible. The mixture is then placed in lead-lined iron vessels, and a current of chlorine is passed over the mixture till the reaction is ended. The temperature generated by the reaction must be properly regulated by cooling the apparatus with water. The phosphorus oxychloride resulting from the decomposition is driven off, collected, and utilised for developing chlorine from bleaching powder for the chlorinating process, phosphate of lime being precipitated, which can be used in manures. For this purpose the oxychloride is treated with water, and the mixture, now containing hydrochloric and phosphoric acids, is brought into contact with the chloride of lime.

The reaction by which the ortho- and para-toluene sulphonic chlorides are produced is indicated by the following equation:—

$$C_6H_4\begin{cases}CH_3\\SO_3Na\end{cases} + (PCl_3 + 2Cl) =$$

$$C_6H_4\begin{cases}CH_3\\SO_2Cl\end{cases} + POCl_3 + NaCl.$$

Toluene sulphonic chlorides

The two sulphonic chlorides remaining in the apparatus are allowed to cool slowly, when the solid one (the para compound) is deposited in large crystals, so that the liquid one can be easily removed by the aid of a centrifugal machine. The crystalline residue is freed from all the liquid sulphonic chloride by washing with cold water. Only the liquid orthotoluene sulphonic chloride is capable of yielding saccharine, and the liquid product above separated is cooled with ice to crystallise out the last traces of the crystalline compound. The solid parasulphonic chloride obtained as by product, is decomposed into toluene, hydrochloric, and sulphurous acids by mixing it with carbon, moistening the mixture, and subjecting it under pressure to the action of superheated steam. The total change proceeds in two stages:—

1. $C_6H_4\begin{cases}CH_3\\SO_2Cl\end{cases} + H_2O = C_6H_4\begin{cases}CH_3\\SO_2OH\end{cases} + HCl.$

2. $2\left(C_6H_4\begin{cases}CH_3\\SO_2OH\end{cases}\right) + C = 2(C_6H_5 . CH_3) + CO_2 + SO_2.$

The toluene is then used again in Step I., and the hydrochloric and sulphurous acids in Step VII.

STEP VI.—The liquid orthotoluene sulphonic chloride is now converted into the orthotoluene sulphonic amide by treating the former with solid ammonium carbonate in the required proportions, and subjecting the resulting thick pulpy mixture to the action of steam. Carbonic acid is set free, and a mixture of orthotoluene sulphonic amide and ammonium chloride remains.

$$C_6H_4\begin{cases}CH_3\\SO_2Cl\end{cases} + (NH_4)_2CO_3 =$$

Toluene sulphonic chloride

$$C_6H_4\begin{cases}CH_3\\SO_2 . NH_2\end{cases} + NH_4Cl + H_2O + CO_2.$$

Toluene sulphonic amide

As the mixture is very liable to solidify on cooling, cold water is at once added to prevent this, and to dissolve out the ammonium chloride, the amide remaining in the solid state. The liquid is separated by centrifugating.

STEP VII.—The orthotoluene sulphonic amide is now oxidised, preferably by means of potassium permanganate. The result of this will be, precipitated manganese dioxide, free alkali and alkaline carbonate, and an alkaline orth sulphamido-benzoate. The alkaline liquid requires careful neutralisation during the oxidising process, and especially before evaporating, with a mineral acid, or else the sulphamido-benzoate formed would be again split up into orthosulphonic benzoate and free ammonia, thus:—

$$C_6H_4\begin{cases}CO . ONa\\SO_2 . NH_2\end{cases} + NaOH = C_6H_4\begin{cases}CO . ONa\\SO_2 . ONa\end{cases} + NH_3.$$

The oxidation process itself is thus represented:—

$$C_6H_4\begin{cases}CH_3\\SO_2 . NH_2\end{cases} + 3O + NaOH =$$

$$C_6H_4\begin{cases}CO . ONa\\SO_2 . NH_2\end{cases} + 2H_2O.$$

Sodium orthotoluene sulphamido-benzoate

By precipitation with dilute mineral acids such as hydrochloric or sulphurous acids, the pure benzoyl sulphonic imide is at once precipitated:

$$C_6H_4\begin{cases}CO . ONa\\SO_2 . NH_2\end{cases} + HCl =$$

$$NaCl + H_2O + C_6H_4\begin{cases}CO\\SO_2\end{cases}NH.$$

"Saccharine," or benzoyl sulphonic imide

Saccharine possesses a far sweeter taste than cane sugar, and has a faint and delicate flavour of bitter almonds. It is said to be 220 times sweeter than cane sugar, and to possess considerable antiseptic properties. On this account, and because of its great sweetness, it is possible that it may be useful in producing fruit preserves or jams, consisting of almost the pure fruit alone; the small percentage of saccharine necessary for sweetening these preserves being probably sufficient to prevent mouldiness. Saccharine has been proved by Stutzer, of Bonn, to be quite uninjurious when administered in considerable doses to dogs, the equivalent as regards sweetness in sugar administered, being comparable to over a pound of sugar each day. Stutzer found, moreover, that saccharine does not nourish as sugar does, but that it passes off in the urine unchanged. It is proposed thus to use it for many medical purposes, where cane sugar is excluded from the diet of certain patients, as in cases of "diabetes mellitus," and in this respect it may prove a great boon to suffering humanity, although we must remember that, as certain of the aromatic compounds if administered for a length of time are known to exert a physiological effect, especially on the liver, it will be desirable to use caution in the regular use of saccharine until its harmless action on the human body has been ascertained beyond doubt.

Saccharine is with difficulty soluble in cold water, from hot aqueous solutions it is easily crystallised. Alcohol and ether easily dissolve it. Hence from a mixture of sugar and saccharine, ether would easily separate the saccharine by solution, leaving the sugar. It melts at about 200° C. with partial decomposition.

The taste is a very pure sweet one, and in comparison with cane sugar it may be said that the sensation of sweetness is much more rapidly communicated to the palate on contact with saccharine than on contact with sugar. The speaker expressed his thanks to the discoverer of saccharine, Dr. Fahlberg, of Leipzig, for a complete and interesting series of preparations illustrating the domestic and medicinal uses of this remarkable compound, and also to his friend Mr. Watson Smith for the kind aid afforded him in the experimental illustration of his discourse.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 20.—"On the Working of the Harmonic Analyser at the Meteorological Office." By Robert H. Scott, F.R.S., and Richard H. Curtis, F.R.Met.Soc.

On the 9th of May, 1878, Sir W. Thomson exhibited to the Society a model of an integrating machine, which consisted of a series of five of the disk, globe, and cylinder integrators, which had been devised two years earlier by his brother Prof. James Thomson, and a description of which will be found in the *Proceedings* of the Royal Society, vol. xxiv. p. 262. Sir W. Thomson's paper describing this model will be found in vol. xxvii. of the *Proceedings*, p. 371; and reference should be made to both these papers for an explanation of the principle of the machine. In the communication last named it is stated that the machine was about to be "handed over to the Meteorological Office, to be brought immediately into practical work."

The model was received at the Office in the course of the

month, and was at once set in action; the results of the preliminary trials when obtained being referred to a Committee consisting of the late Prof. H. J. S. Smith and Prof. Stokes, who, on July 5 following, submitted to the Meteorological Council a favourable report on the performance of the model.

The Council at once resolved to have a machine constructed, which should be specially adapted to the requirements of the work for which it was intended, viz. the analysis of photographic thermograms and barograms.

In preparing a working design for actual execution, it was found necessary to make several modifications in the details of the mechanical arrangements of Sir W. Thomson's original model, and these were mainly worked out by Prof. Stokes and Mr. de la Rue. The construction of the instrument was intrusted to Mr. Munro. It was considered sufficient to limit the action of the machine so as to extend only to the determination of the mean, and the coefficients as far as those of the third order, in the expression

$$E = a + a_1 \cos\theta + b_1 \sin\theta + a_2 \cos 2\theta + b_2 \sin 2\theta + a_3 \cos 3\theta + b_3 \sin 3\theta + \&c.,$$

and to obtain these it was necessary to have seven sets of spheres, disks, and cylinders.

A description of the machine, as actually constructed, together with engravings giving a general view of the machine, and illustrating some of its details, will be found in *Engineering* for December 17, 1880.

The machine was delivered at the Office in December 1879, and a lengthened series of trials was at once commenced, to determine its constants, and thoroughly test the accuracy of its working, for which purpose systems of straight lines and curves, of which the values were known, were first used. A few small unforeseen difficulties were early met with, necessitating slight modifications in some portions of the instrument.

The chief of these faults was a slight turning of the cylinders upon their axes, when the balls were moved to and fro along the disks, parallel to the axes of the cylinders. The movement was always in the same direction, namely, towards the disks, whether the ball was moved to the right or left. After the trial of many expedients the defect was finally, in great measure, overcome by attaching weights to the spindles of the cylinders. It however still exists in the machine to a slight extent, and its effect is to *decrease* the readings on the cylinders by a very small amount.

It was decided to employ the analyser, in the first instance, in the determination of temperature constants, and careful comparisons have been made of the results obtained by its means with those got by actual measurement of the photographs and numerical calculations, as will presently be mentioned, and the accordance is so very close as to prove that the machine may safely be trusted to effect reductions which could only otherwise be accomplished by the far more laborious process of measurement and calculation.

It will facilitate an apprehension of the method of using the machine to give a somewhat detailed account of the operations involved in the treatment of the curves, with an example of the manner in which the readings of the machine are recorded and dealt with.

The machine is furnished with three pairs of recording cylinders and disks, numbered consecutively 1 to 6, which give the coefficients for the first three pairs of terms of the expansion, and in addition a seventh cylinder and disk from which the mean is obtained. In the thermograms which supply continuous photographic records of the march of temperature, the trace for twenty-four hours covers a length of 8·75 inches, while a vertical height of about 0·7 inch[1] corresponds to a range of ten degrees in temperature; each thermograph sheet contains the record for forty-eight hours.

Conveniently placed in the machine is a cylinder or drum, the circumference of which is equal to the length of twenty-four hours upon the thermograms. Round this cylinder the thermograms are rolled, the fluctuations of temperature indicated by the curves being followed, as the cylinder revolves, by a combination of the movement of the cylinder with that of a pointer moving in a line parallel to its axis.

The handle by which the cylinder is turned gives motion at the same time to the seven disks of the machine, and the operator thus controls by his left hand both the speed with which the curves are paid through the machine and the consequent velocity of the angular motion of the disks, while, by a suitable contrivance, the movements of the pointer, governed by his right hand and following the curve, produce on the face of the disks corresponding movements to the right or left of the balls by which the motion of the disks is conveyed to the recording cylinders.

[1] This value varies slightly for each observatory.

At the commencement of an operation all the cylinders are set to zero; the twelve months curves are then passed consecutively through the instrument; the first pair of cylinders, which gives the coefficients of the first order, and also the mean cylinder, 7, being read for each day, while cylinders 3 and 4, and 5 and 6, which give the coefficients of the second and third orders respectively, are only read for each five days and at the end of each calendar month. The numbers on the cylinders are, however, progressive, so that the increments upon them for any given period could very easily be obtained.

At present only the monthly increments of the readings have been dealt with, so as to obtain the coefficients of the mean daily variation for each month of the year. The process followed is, therefore, simply to divide the monthly increment by the number of days in the month, and then to multiply the quotient by a factor which is determined by the scale-value of the thermograms, and which will therefore be different for each observatory.

As an illustration, the case of Kew for July 1882 may be taken. The increments for the month obtained from the final readings of the cylinders are as follows:—

Cylinder	1.	2.	3.	4.	5.	6.	7.
Observed increment	+ 2·198	− 2·671	− 0·101	− 0·198	− 0·797	− 0·564	+ 56·839
Divided by 31 (the number of days).........	+ 0·071	− 0·086	− 0·003	− 0·006	− 0·026	− 0·018	+ 1·834
Factor	− 53·52	+ 53·52	− 26·76	− 26·76	− 17·84	− 17·84	+ 6·69
Coefficient deduced......... =	− 3·80	− 4·60	+ 0·08	+ 0·16	+ 0·46	+ 0·32	+ 12·27
Add constant.........							48·17
Mean temperature.........							60·44

After some trials with the curves for the year 1871, the year 1876 was taken up, inasmuch as for that year the records had been discussed by Mr. H. S. Eaton, F.R.Met.Soc., who had calculated the hourly means of the various meteorological elements for each month separately, and who kindly placed his results at the disposal of the Council.

The working of the machine was thus subjected to an exact test by comparing the results obtained by it with the coefficients in the harmonic series which were calculated from Mr. Eaton's means; and their trustworthy character, and the adequacy of these calculations to serve as a standard with which the coefficients obtained by means of the machine might be compared, was established by calculating them from the *odd* and *even* hours, quite independently, for all the seven observatories.

The outcome of this experiment was thoroughly satisfactory, and the entire series of results obtained both by calculation and from the machine was published as Appendix IV. to the Quarterly Weather Report for 1876, together with a Report prepared by Prof. Stokes, the concluding paragraphs of which may be quoted here, since they sum up in a few words the conclusions arrived at.

"Disregarding now the systematic character of some of the errors, and treating them as purely casual, we get as the average difference between the constants as got by the machine and by calculation from the twenty-four hourly means 0°·065. It may be noticed, however, that the numbers are unusually large (and at the same time very decidedly systematic) in the case of the second cylinder of the first order b_1, for which the average is as much as 0°·150, the seventh of a degree.

"If b_1 be omitted, the average for the remaining cylinders of the machine is reduced to 0°·047.

"We see, therefore, that, with the exception perhaps of b_1, the constants got by the machine for the mean of the days constituting the month are as accurate as those got by calculation, which requires considerably more time, inasmuch as the hourly lines have to be drawn on the photograms, then measured, then meaned, and the constants deduced from the means by a numerical process by no means very short."

The curves for the twelve years 1871 to 1882 inclusive have now been passed through the machine, and the results obtained have been carefully checked so far as the arithmetical work involved is concerned, upon a plan approved by the Council. No *direct* check, short of passing the curves a second time through the machine, can however at present be put on any portion of the results except as regards the means, which have been compared with the means calculated from the hourly readings obtained by measurement from the curves. The results of this work will be published in the Hourly Readings for 1883, but the general results may here be stated.

As a rule, the monthly means yielded by the harmonic analyser agree well within a tenth of a degree with those obtained by calculation from the hourly measurements of the curves; and although in some exceptional cases larger differences have been found, amounting in rare instances to as much as half a degree, it is probable that generally these are less due to defects in the working of the instrument than to other causes. In some cases large breaks in the curves, due to failure of photography, &c., were interpolated when the curves were passed through the machine, but not when the means were worked out from measurements of the curves. Some differences rather larger than usual, and confined chiefly to the earliest years dealt with, have been ascertained to have arisen from the circumstance that when the curves were first measured, to obtain hourly values, the method of making the measurements was not the same as that found by subsequent experience to be the preferable; and also that in some cases the scale-values first used were less accurately determined than has since been found possible.

In both these respects the two methods were on a par in the later years dealt with, and therefore the fairest comparison is to be had with their means.

For 1880, the average difference of the monthly mean for all the seven observatories is 0°·09; for 1881 it is 0°·05; and for 1882 0°·06; and in these three years a difference of 0°·3 between the analyser and calculated means occurred but once, and of 0°·2 but five times.

What has been said is sufficient to show that the instrument is completely applicable to the analysis of thermograms.

It has also been employed on the discussion of barograms, and the curves for the years 1871, 1872, and 1876 have been passed through the machine.

The year 1876 was selected owing to the existing facilities for comparing the resulting figures with those obtained by calculation from Mr. Eaton's means, and the result in this case was equally satisfactory with that for temperature already mentioned.

May 27.—"Family Likeness in Eye-Colour." By Francis Galton, F.R.S.

This inquiry proved that certain laws previously shown by the author to govern the hereditary transmission of stature also governed that of eye-colour: namely, that the average ancestral contributions towards the heritage of any peculiarity in a child are from each parent $\frac{1}{4}$, from each grandparent $\frac{1}{16}$, and so on; also that each parent and each child of any person will on the average possess $\frac{1}{3}$ of that person's peculiarity. The eye-colours were grouped into light, hazel (or dark gray), and dark; then it was shown that $\frac{2}{3}$ of the hazel were fundamentally light, and $\frac{1}{3}$ of them were dark, and they were statistically allotted between light and dark in that proportion. The desired test of the truth of the laws in question was thus reduced to a comparison between the calculated and observed proportion of light- and dark-eyed children born of ancestry whose eye-colours presented various combinations of light, hazel, and dark. The inquiry was confined to children of whom the eye-colours of both parents and of all four grandparents were known. There are six possible combinations of the three eye-colours in the parents, and fifteen in the grandparents, making a total of ninety possible classes, but of these one-half were wholly unrepresented in the returns, and many others were too scantily represented to be of use. The remainder were discussed in six different ways: that is to say, in two groups, *a* and *b*, and each group by three methods. In *a* the families were classified and grouped according to their several ancestral combinations of eye-colour, but only those groups that consisted of twenty or more children were used; there were 16 of these groups and 827 children. In *b* the families were treated separately, but only large families were taken, viz. those that consisted of at least six children: they were 78 in number. In both *a* and *b* separate calculations were made on the suppositions (1) that the parental eye-colours were alone known; (2) that the grandparental were alone known; (3) that the parental and the grandparental were alone known. The conformity between the calculated and the observed numbers throughout every one of the six sets of calculations was remarkably close, and the calculated results obtained by the method (3) were the best.

"Notes on Alteration induced by Heat in Certain Vitreous Rocks, based on the Experiments of Douglas Herman, F.I.C., F.C.S., and G. F. Rodwell, late Science Master in Marlborough College." By Frank Rutley, F.G.S., Lecturer on Mineralogy in the Royal School of Mines. Communicated by Prof. T. G. Bonney, B.Sc., F.R.S.

In this paper an endeavour has been made to ascertain the nature of the changes which are induced in a few typical vitreous rocks by the action of heat only. The specimens experimented upon were—

(1) The pitchstone of Corriegills, Arran.
(2) Black obsidian from Ascension.
(3) Black obsidian from the Yellowstone District, U.S.A.
(4) Glassy basalt lava of Kilauea, Hawaii.
(5) Basalt of the Giant's Causeway, Antrim.

The Arran pitchstone was heated for 216 hours at a temperature ranging from 500° to about 1100° C. The clear, greenish belonites of hornblende, so plentiful in the unaltered rock, were found to have turned to a deep rusty brown through peroxidation of the protoxide of iron which was present in the hornblende. The dusty matter mixed with clear spiculæ of hornblende, which occurred between the belonites and shaded gradually off into the clear glass which immediately surrounded the belonites in the normal state of the rock, has segregated to some extent, a sharp line of demarcation now existing between the dusty matter and the areas of clear glass, while the spiculæ of hornblende have somewhat increased in size if not in number. No actual devitrification of the glass has resulted from the heating.

The obsidian from Ascension showed only a banded structure coupled with streams of colourless microliths and a few felspar crystals when a section of the unaltered rock was examined microscopically. Two specimens of this rock were artificially heated, the first for the same period and at the same temperature as the Arran pitchstone, while the second was kept for 701 hours at a temperature ranging from 850° to 1100° C.

In the first specimen the banded structure disappeared entirely, or almost entirely, but numerous microliths are present in the altered rock, in which the most remarkahle change consists in the development of an excessively vesicular structure.

In the second specimen a vesicular structure is also developed, an outer crust consisting of a very thin layer of clear brownish glass, followed by a nearly opaque layer composed of greenish-brown microliths, which shades off into a colourless glass containing similar microliths, which are probably some form of amphibole or pyroxene. The remainder of the specimen has been completely devitrified.

The Yellowstone obsidian in its normal state shows little else but trichites and globulites when examined under a high power.

Two specimens of this rock were heated: the first at from 500° to 1100° C. for a period of 216 hours, the second from 850° to 1100° C. for 701 hours. In the first case a remarkably vesicular structure has been developed; the trichites have entirely disappeared, and small granules and crystals of magnetite have been formed. In the second specimen the changes are very peculiar. The fragment retained its original form, but the surface showed minute blisters or elevations, which, when when cracked open, revealed a cavernous structure produced by

the coalescence of vesicles averaging from ¼ to ½ inch in diameter. These cavities were often lined with a white crystalline crust, and generally contained white crystalline pellets, each about one-third the size of the cavity in which it occurred. Minute crystals of specular iron were detected upon the surfaces of these pellets. The glassy part of the rock, which still remains clear, contains trichites and globulites similar to those in the unaltered obsidian, but they are more numerous in the artificially-altered rock.

The vesicular glassy basalt lava of Kilauea, when examined under the microscope, is seen to contain crystals of olivine and minute crystallites which have not hitherto been referred to any particular mineral species. A specimen of this lava, kept for 960 hours at a temperature ranging from 750° to 1200° C., shows that the olivine crystals have undergone no appreciable alteration, but the glass itself has become perfectly opaque and black, owing to the separation of magnetite.

The specimens of basalt from the Giant's Causeway were fused in Stourbridge crucibles in a gas furnace. One, which was cooled rapidly, appears under the microscope as a clear glass containing vesicles; another, cooled slowly, is black and opaque, except in certain spots where a prismatic structure is visible, the marginal portions of the prisms having a radiating crystalline or fibrous character.

In another case some of the powdered basalt was again fused, and a fragment of cold basalt was placed on the surface and allowed to sink into the molten mass. The result was a glass, which, under the microscope, appears perfectly clear except in the immediate vicinity of the immersed fragment, which is surrounded by an opaque black border containing divergent groups of colourless transparent or translucent crystals. The black border, where it joins the clear glass, is sharply defined, and its opacity and blackness must be regarded as due to a separation of magnetite, as in the case of the altered Kilauea lava.

The first series of experiments were made by Mr. Herman. The specimens from the Giant's Causeway were experimented upon by Mr. Rodwell.

Zoological Society, June 1.—Dr. A. Günther, F.R.S., Vice-President, in the chair.—Dr. A. Günther, F.R.S., exhibited and made remarks on a specimen of a small fish of the genus *Fierasfer* embedded in a pearl oyster.—The Secretary made some remarks on the most interesting objects he had observed during a recent visit to the Zoological Gardens of Rotterdam, Amsterdam, Cologne, Antwerp, and Ghent.—A letter was read from Mr. J. M. Cornely, of Tours, C.M.Z.S., stating that his pair of Michie's Deer had bred in his park, and that a young one had been born on May 15.—Mr. Beddard read notes on the convoluted trachea of a Curassow (*Nothocrax urumutum*), and on the form of the syrinx in certain Storks.—Mr. W. F. Kirby read a paper containing an account of a small collection of Dragon-flies which had been formed by Major J. W. Yerbury at Murree and Campbellpore, N.W. India. The collection contained examples of about twenty species.

Physical Society, May 22.—Prof. Balfour Stewart, President, in the chair.—Messrs. C. A. Bell, W. C. Johnson, and James Swinburne were elected Members of the Society.—The following communications were read:—On the sympathetic vibrations of jets, by Mr. Chichester A. Bell. It has been assumed hitherto that a gaseous or liquid jet vibrates under the influence of a limited range of tones only; effective tones being those which do not differ greatly in pitch from the normal or proper tone of the jet, discovered by Savart and Sondhauss. The author has found, however, that, when the pressure under which a jet escapes is not too great, the latter is affected by all tones lower than the normal, as well as by some above it. Changes may be excited in a jet of either kind by vibratory motions of the jet orifice, or of the fluid behind or external to the orifice. These changes take the form of slight swellings and contractions, which become more pronounced as the fluid travels away from the orifice, and finally cause the jet to break or become discontinuous at a distance which depends upon the intensity of the initial disturbances. At any point within the continuous portion of the jet the successive swellings and expansions represent both the form and the relative intensities of vibrations impressed upon the orifice, and the jet is therefore capable of reproducing very complex sounds, such as those of speech and music. A vibrating jet of air does not, however, emit sound when it plays into free air, or into the wide end of a tube communicating with the ear; but when it plays against a very *small* orifice in the end of a hearing tube, loud sounds may result. This reproduction is most intense when the hearing orifice is placed in the axis of the jet, just within the breaking point, but becomes gradually feebler as the hearing orifice is moved towards the jet orifice or out of the line of its axis. Beyond the breaking point the sounds from the jet at first become confused, and finally are lost. A jet of gas, like a liquid jet, only vibrates so as to produce its normal tone when it strikes upon some obstacle which serves to diffuse the disturbances due to impact, or throw them back upon the orifice. The vibrations of an air jet are also loudly reproduced as sound when it is directed against a small flame below the apex of the blue zone. Liquid jets are but slightly sensitive to aërial sound-impulses, but become highly sensitive when the jet tube is rigidly attached to a sound-board. The vibrations of a jet so mounted are best perceived as sound when the stream strikes upon a rubber membrane tied over the end of a narrow tube which communicates with the ear. For accurate reproduction of speech and sounds in general the jets should be at such a pressure as to respond visibly to a note of about 4000 vibrations per second; and the membrane should be at such a distance from the orifice that the jet never breaks or becomes discontinuous above its surface. The vibrations of very fine jets of any conducting liquid become loudly audible when a portion of the jet, or the "nappe" formed when it strikes upon a flat surface, is included in circuit with a battery and a telephone. This may be accomplished by letting the jet impinge on the end of an ebonite rod, through the centre of which passes a platinum wire; the upper end of the rod is surrounded by a short tube or ring of platinum, the upper margin of which forms a continuous, slightly convex surface with the exposed end of the central wire and the ebonite. The wire and ring form the terminals of the circuit which is completed through the "nappe." Distilled water containing 1/300 of its volume of pure sulphuric acid is recommended as the jet liquid. The author advances a new theory to account for the growth of the vibratory changes in liquid and gaseous jets.—On some thermo-dynamical relations, part 5, by Prof. W. Ramsay and Dr. S. Young. In parts 1 and 2 of this series of papers it was shown that the ratio of the absolute temperatures of any two bodies corresponding to a given vapour-pressure bears a simple relation to the ratio at any other pressure, which may be expressed by the equation $R' = R + c(t' - t)$; where R' and R are the two ratios, c is a constant, and t' and t are the temperatures of one of the two bodies. The determination by Schumann (*Pogg. Ann.*, N.F. 12, 46) of the vapours of methyl-formate and twenty-seven homologous ethers made it possible to compare the vapour-pressures of a large number of bodies belonging to the same class. It was found that when the ethers were compared with ethyl acetate, which was taken as the standard, in every case $c = 0$, and therefore $R' = R$. The temperatures corresponding to the three pressures 260, 760, and 1300 mm. are given by Schumann. Taking the mean value of R for those pressures as correct, and recalculating the temperatures, the greatest difference between the found and recalculated temperatures is 0°·7 C. The vapour-pressures of water or any one of the ethers being accurately known, it is sufficient to determine the boiling-point of any ether belonging to this class, in order to construct its vapour-pressure curve. The absolute temperatures corresponding to the pressures 200 and 1300 mm. for any ether are ·89795t and 1·0488t, where t is the boiling-point at normal pressure in absolute temperature.—A gridiron slide-rule by Mr. Stanley, designed by Mr. Thacher, was explained by Mr. C. V. Boys. It was equivalent to a slide 60 feet long, and performed multiplication and division with an error not exceeding the 1/40,000 part.—Specimens of safety explosives and their results in shattering blocks of lead were exhibited by H. Sprengel.

Geological Society, May 12.—Prof. J. W. Judd, F.R.S., President, in the chair.—Matthew Blair was elected a Fellow, and Prof. H. Rosenbusch, of Heidelberg, a Foreign Correspondent of the Society.—The following communications were read:—On the maxilla of Iguanodon, by J. W. Hulke, F.R.S.—Notes on the distribution of the Ostracoda of the Carboniferous formations of the British Isles, by Prof. T. Rupert Jones, F.R.S., and J. W. Kirkby. Although all the Ostracoda of the Carboniferous formations are not yet described, there are 170 species and notable varieties known, belonging to thirty-three genera of nine families. About twenty-five of these species, not yet described, but determined by the authors, are introduced into

their lists as giving a fuller idea of the value of this manifold Crustacean group. In the first place they referred to the classification of the Carboniferous strata in Scotland and in England, according to the local differences, taking in succession "Scotland West," "Scotland East," "England North, with the Isle of Man," "England Central and South, with South Wales," as the several districts from which they have obtained good groups of Ostracoda from different members of the Carboniferous series. In Fife the lowest local Carboniferous strata contain *Beyrichia subarcuata*; higher up come in *Carbonia fabulina*, *C. Rankiniana*, *Bairdia nitida*, and *Leperditia Okeni*; the last, accompanied by other species, occurs throughout this lowest series, in which the record is more complete than in Midlothian and Linlithgowshire, where the same species also occur. In Dumfriesshire and Ayrshire *L. Okeni* and *L. subrecta* have been found in beds even lower than the above-mentioned, and are therefore probably the oldest Carboniferous Ostracoda; other species accompany them higher up, and in Roxburghshire some localities of the Carboniferous Sandstone series are very rich in species. The Carboniferous Limestone series of South-West Scotland has been highly productive of Ostracoda, particularly the shales of the lower beds; thirty-six species are common or characteristic. The middle or coal-bearing portion has yielded but few, chiefly *L. Youngiana*, one Beyrichia, *C. fabulina*, and *C. Rankiniana*. The Upper Limestone group contains many recurrents from below and a few others, including *Youngia rectidorsalis* (MS.). The Millstone Grit equivalents have no Ostracoda, but the overlying Coal-measures are rich in *Carboniæ*, with a few others, such as *Cypridina radiata*. A great variety of genera and species come from beds at or near the base of the Scar Limestone and its equivalents in North Lancashire, Westmoreland, Cumberland, and Northumberland. The calcareous shales of the Yoredale series have several interesting forms, including *Primitura concinna* (MS.); none from the Millstone Grit. The Lower Coal-measures give *Beyrichia arcuata* and *Carbonia*, sp. The middle beds have *B. arcuata* and *Carbonia fabulina*, common; rarer, *C. Rankiniana*, *C. secans*, *C. scalpellus*, *C. Wardiana* (MS.), and *Philomedes elongata*. In the Upper Coal-measures *B. subarcuata* reappears; and in the *Spirorbis*-limestone *Leperditia inflata* is the latest Carboniferous Ostracod in England. In Northamptonshire the deep Gayton boring (at 730 feet) has given *Kirkbya variabilis*, *K. plicata*, *Bythocypris sublunata*, *Macrocypris Jonesiana*, *Cythere lla extuberata*, and *C. attenuata*, all but one belonging to the Lower Carboniferous series. In Salop, South Wales, and Somerset the Carboniferous Limestone has yielded several good species of *Leperditia*, *Kirkbya*, *Moorea*, *Bythocypris*, *Bairdia*, &c. *Carbonia Agnes* and *C. Evelinæ* belong to the South Welsh Coal-measures. The distribution of the Carboniferous Ostracoda in Ireland requires further work; but the Lower Carboniferous Shales and the Mountain Limestone near Cork and elsewhere are very rich, as are also some parts of the latter in the Isle of Man. The Ostracoda of the Permian Formation were then treated of in relation to their Carboniferous allies, and the range of the British Carboniferous Ostracods in Europe and North America was noticed in some detail. The results of the examination were shown in two extensive tables.—Note on some Vertebrata of the Red Crag, by R. Lydekker, F.G.S. This communication contained briefly the results of a re-examination of the specimens from the bone-bed of the Red Crag in the British and Ipswich Museums, a series of casts from the latter having been added to the former. The forms noticed were *Hyæna striata*, with which *H. antiqua* and *H. arvernensis* were considered probably identical, *Mastodon*, of which the author thought three species—*M. arvernensis*, *M. longirostris*, and *M. borsoni* were represented; *Sus*, of which two forms, the larger probably *S. erymanthius* or *S. antiquus*, the smaller *S. palæochœrus*, had been detected; a Tapir, which was probably *Tapirus arvernensis* or *T. elegans* rather than *T. priscus*; *Hipparion gracile*; a *Rhinoceros* referable to the hornless *R. incisivus* rather than to *R. schleiermacheri*, though the latter probably also occurred; and a species of Albatross (*Diomedea*) represented by a right tarso-metatarsus, and the associated proximal phalangeal bone of the fourth digit.—The Pleistocene succession in the Trent Basin, by R. M. Deeley, F.G.S. The beds of the lowest division were distinguished from those of the middle and upper by the absence of Cretaceous rock-debris. Older Pleistocene: Early Pennine Boulder-clay, Quartzose Sand, Middle Pennine Boulder-clay; Middle Pleistocene: Melton Sand, Great Chalky Boulder-clay, Chalky Sand and Gravel; Newer Pleistocene: Interglacial River-alluvium, Later Pennine Boulder-clay. Each of the separate stages was then described separately, with details of exposures and sections throughout the area.—On the existence of a submarine Triassic outlier in the English Channel off the Lizard, by R. N. Worth, F.G.S.

Anthropological Institute, May 25.—Francis Galton, F.R.S., President, in the chair.—Mr. Reginald Stuart Poole read a paper on the ancient Egyptian classification of the races of man. This was defined by the famous subject of the four races in the tombs of the kings at Thebes (B.C. 1400 1200). The types were (1) Egyptian, red; (2) Shemite, yellow; (3) Libyan, white; (4) Negro, black. By comparison with monuments of the same period and of a somewhat earlier date, the first race, clearly an intermediate type, was seen to comprehend the Phœnicians, the Egyptians, and the people of Arabia Felix with the opposite coast. The Libyan race included an aquiline type, with marked supra-orbital ridges and receding foreheads, as well as a straight-nosed type. These two types inhabited the south coast of the Mediterranean, and some of the islands. The Negro race included the Negro and Nubian types. The Hittites and Hyksos, or shepherds, were as yet unclassed. Prof. Flower pointed out the resemblance of the aquiline Libyan type to that of the Neanderthal crania, and the oldest European type, and saw in the Hyksos head distinctly Mongolian characters. These two points are of the highest consequence in historical anthropology.—Mr. C. W. Rosset exhibited a large collection of photographs and other objects of ethnological interest from the Maldive Islands and Ceylon.

Paris

Academy of Sciences, May 31.—M. Jurien de la Gravière, President, in the chair.—Observations of the small planets made with the large meridian instrument at the Paris Observatory during the first quarter of the year 1886, by M. Mouchez.—Note on a new general method of determining directly the absolute value of refraction at all degrees of altitude, by M. Lœwy. This is a further development and more general application of the author's recent communication on the means of determining some absolute values of refraction with a sufficient degree of accuracy.—Researches on the densities of liquefied gases and of their saturated vapours, by MM. L. Cailletet and Mathias. In this memoir the authors' studies are limited to the protoxide of nitrogen, ethylene, and carbonic acid. It is shown that at the critical point the density of the fluid is equal to that of its vapour, whence a practical means of determining graphically the density at the critical point when the critical temperature is known. It is also shown that the expansion of the liquefied gas is greater than that of the gas itself. The methods here described are applicable to all gases whose critical point is higher than the freezing-point of mercury.—On MM. Albert A. Michelson and Edward W. Morley's recent experiments to ascertain the influence of motion of the medium on the velocity of light (*American Journal of Science*, May 1886), by M. A. Cornu. The author briefly describes the American physicists' experiments, which show that the result announced by Fizeau in 1851 is essentially correct, and that the luminiferous ether is entirely unaffected by the motion of the matter which it permeates. At the conclusion of the paper M. Fizeau took occasion to remark that he had never ceased to prosecute his studies on the nature and properties of the ether, and hoped soon to announce the existence of a peculiar variation in the magnetic force of magnets apparently in relation with the direction of the earth's motion through space, and calculated to throw great light on the immobility of the ether and its relations to ponderable matter.—On an arc tangent to the solar halo of 46°, observed on May 30, by M. A. Cornu. Of the numerous halos observed during last month this is described as the most remarkable. It was visible towards 5 p.m. under the form of an extremely vivid iridescent arc concentric with the zenith at a distance of 15° to 20° in a circular sphere of from 60° to 80°. The author considers that from the systematic observation of these phenomena some valuable data might be obtained regarding the condition and movements of the upper atmospheric currents, which would be useful in weather forecasting.—On the heats of combustion and of formation of the solid carburets of hydrogen, by MM. Berthelot and Vieille. The method announced by the authors two years ago for measuring the heat of combustion of the fixed or but slightly volatile organic compounds is here applied to the study of naphthaline, acenaphthene, anthracene, phenanthrene, dibenzyl, and some other

carburets chosen on purpose with a view to determining certain general relations.—On the ammonia present in the ground: a reply to MM. Berthelot and André, by M. Th. Schlœsing. The points in dispute are re-stated, and the author deals fully with the chief objections urged by MM. Berthelot and André against his view that generally speaking the ammonia passes from the atmosphere to the earth.—Note accompanying M. Silvestri's report on the eruption of Mount Etna on May 18 and 19, by M. Daubrée. It is noticed that the present lava-stream, like that of 1883, flows from the crevasse which was opened on the flank of the volcano in the direction from north-east to south-west in the year 1875. But it rises at an elevation of about 1400 metres some 7 kilometres above the town of Nicolosi, which has thus so far escaped destruction.—On the influence of magnesia on Portland cements, by M. G. Lechartier. The author's extensive researches amongst public buildings and structures of all sorts fully confirm the conclusion already arrived at by Rivot regarding the destructive effects of the magnesia present in these cements. The more they are exposed to the direct action of water, the more rapid is the process of decay.—Observations of the new comet Brooks III., made at the Observatory of Nice with the Gautier equatorial, by M. Charlois.—Note on the theoretic calculation of the composition of vapours, their coefficients of expansion, and vaporising heats, by M. M. Langlois. The formulas which in the author's theory give the specific heats of the gases or vapours are as under:—

	Atm.		
Molecules at	1	... ...	$C = \frac{2}{3}\frac{V\pi Pg}{9}\alpha = \frac{2}{3}A\alpha$
,,	2	... ...	$C = A\alpha$
,,	3	... ...	$C = \frac{4}{3}A\alpha$
,,	4	... ...	$C = \frac{6}{3}A\alpha$

where α is the coefficient of expansion of the vapour, V the volume occupied by 1 kilogramme of this vapour under the pressure F. The complete theory will be explained at the next Congress of the French Association at Nancy.—On the diffusion of heat and physical isomorphism, by M. L. Godard. The property of being athermochroic, hitherto supposed to be peculiar to pure common salt and sylvine (natural chloride of potassium), is shown to be also characteristic of the isomorphous and anhydrous chlorides, bromides, and iodides. These substances have the same chemical formula and crystallise in the same system, and thus is once more confirmed the analogy presented by the physical properties of isomorphous bodies.—Law of the product corresponding to the maximum of useful work in an electric distribution, by M. Vaschy. It is shown that Jacobi's law (product $= \frac{1}{2}$) is inapplicable to the case of a dynamo-generator whose electromotor force is a function of the current traversing it.—On the cyclonic whirlwind of May 12: influence of the Guadarama mountain-range on its progress through the Iberian peninsula (second note), by M. A. F. Nogués. The fresh impulse given to this Atlantic cyclone by contact with the cold dry upland currents in the neighbourhood of Madrid show that under special conditions the central Spanish plateau may be compared to certain tropical regions, producing in Spain meteoric phenomena analogous to those of the torrid zone.—On the heats of combustion of the fatty acids and of some fatty substances derived from them, by M. W. Louguinine. Completing his previous studies on the heat of combustion of the fatty acids and their derivatives, the author here treats of caprylic, nonylic, lauric, myristic, and palmitic acids, and of the trilaurine and trimyristine glycerines.—On the dissociation of the carbonate of lime, by M. H. Le Chatelier.—On a new gaseous body, the oxyfluoride of phosphorus, with the formula $PhFl_3O_2$, by M. H. Moissan.—On the direct chloridation of methylbenzoyl, by M. H. Gautier.—Action of oxygenated water on benzoic acid in the presence of sulphuric acid, by M. Hanriot. Having previously shown that oxygenated water reacts on the benzenic hydrocarburets in sulphuric solution, transforming them to phenols and diphenols, the author here extends this reaction to the aromatic acids, and especially to benzoic acid.—On jaborine, by MM. Hardy and Calmels.—Absorption of the bicarbonates of potassa and lime by the roots of beetroot during the first year's growth, and their transformation to organic acids in combination with the potassa and lime diffused throughout the different parts of the plant during vegetation, by M. H. Leplay.—On the superficial measurement of the underground parts of plants, by M. Aimé Girard. A method is proposed by which the superficial development of vegetable roots may be approximately determined within about $\frac{10}{100}$ above or below the reality.—Fresh observations on the Jurassic bilobites, by M. Stan. Meunier. Several new forms of these curious fossil vestiges are described, tending more and more to show that they are of organic origin, and not merely animal footprints.—On the existence of the Lower Eocene formation in the Chalosse district, and on the position of the Bos d'Arros strata, by MM. Jacquot and Munier-Chalmas.

BOOKS AND PAMPHLETS RECEIVED

"Report of the Second Hudson's Bay Expedition, 1885."—"Charts showing the Ocean, Monthly and Annual Temperatures of Hudson's Bay Region and Eastern Canada, September 1884 to October 1885," by A. R. Gordon.—"Algebraical Exercises and Examination Papers," by H. S. Hall and S. R. Knight (Macmillan).—"Annalen des k.k. Naturhistorischen Hofmuseums," Band i., No. 2 (Hölder, Wien).—"Indice Alfabético de la Encyclopedia Popular Ilustrada de Ciencias y Artes," by F. Gillman (Gras, Madrid).—"Embryologische Studien an Medusen, Atlas," by E. Metschnikoff (Hölder, Wien).—"Report of the Metropolitan Board of Works, 1885."—"Electric Transmission of Energy," by C. Kapp (Whittaker and Co.).—"Physiology of Plants," by S. H. Vines (Cambridge University Press).—"A West Indian Sanitorium and a Guide to Barbadoes," by Rev. J. H. Sutton Moxly (S. Low).—"A Year in Brazil," by H. C. Dent (K. Paul).—"A Year with the Birds" (Blackwell, Oxford).—"Microbes, Ferments, and Moulds," by E. L. Trouessart (K. Paul).—"Catalogue of the Birds in the British Museum," vol. xi.—"Catalogue of the Fossil Mammalia in the British Museum," Part 3, by R. Lydekker.—"A Book of Duck Decoys," by Sir R. Payne-Gallwey (Van Voorst).—"Report of Experiments in the Manufacture of Sugar at Magnolia Station, Lawrence, La., Season of 1885-86," by G. J. Spencer (Washington).—"Principles and Methods of Soil Analysis," by E. Richards (Washington).—"Methods and Machinery for the Application of Diffusion to the Extraction of Sugar from Sugar-Cane and Sorghum," by H. W. Wiley (Washington).—"Proceedings of the Windsor and Eton Scientific Society, 1885" (Oxley, Windsor).—"Hydrophobia, M. Pasteur and His Methods," by Dr. T. M. Dolan (H. K. Lewis).

CONTENTS

THURSDAY, JUNE 17, 1886

EARTHQUAKES AND OTHER EARTH-MOVEMENTS

Earthquakes and other Earth-Movements. By J. Milne. "International Scientific Series," Vol. LVI. (London: Kegan Paul, Trench, and Co., 1886.)

THE object of this work is "to give a systematic account of various earth-movements": these are classified as (1) Earth-quakes, (2) -Tremors, (3) -Pulsations, and (4) -Oscillations, which are severally defined as (1) sudden or violent, (2) minute, (3) slow, (4) secular movements of the ground. The earthquakes occupy 305 pp., whilst only 43 pp. are given up to the other three motions. After an introduction follows a description (22 pp.) of about twenty different kinds of instruments for recording earth-movements (seismo-scopes, -meters, and -graphs), beginning with an ancient Chinese seismoscope (A.D. 136). The construction of a proper seismo*graph* which shall record period, amplitude, and direction of movements is difficult, the inertia of the moving parts masking the earth-movement. A set of tipping columns seems to be the simplest seismoscope, and some form of pendulum the simplest seismograph: these can be made "astatic," so as to retain any deflected position. Of recorders of motion a smoked-glass plate seems the simplest. The "Gray and Milne seismograph" is an elaborate instrument, recording continuously and simultaneously the times, and also the three rectangular components, of any displacement.

The explanation of earthquake propagation by waves of elastic compression and distortion, shadowed forth by Dr. T. Young, is fully discussed and illustrated for the general reader: the former are compared to sound-waves and the latter to light-waves; unlike these, however, the direct waves travel the quicker. It appears that about seven-eighths of the speed of wave-transit through homogeneous rock is lost in actual rocks as found *in situ*.

Experiments on artificial earthquakes have—as might be expected—been few. Only three sets appear to have been published. Those by Mr. Mallet (published in 1851) were based on explosions of large masses of powder up to 12,000 lbs. Those by General Abbot in America (published in 1876) were based on explosions of from 70 to 400 lbs. of dynamite. Those by Mr. Milne in Japan, since 1880, were based on the fall of a ton weight through from 10 to 35 feet, and on explosions of 1 to 2 lbs. of dynamite. The general conclusions are that the wave-speed is quicker for direct than for transverse vibrations, is quicker through hard than through soft rock, increases with increase of the shock, and decreases with distance. It is clear that opportunities for experiment on the effect of explosions such as the above must often occur, and that a small expenditure on seismographs alone is needed to embrace the opportunities.

There appears to be no known limit to the frequency and duration of earthquakes. Shocks occur continuously in the Andes (p. 246), and have lasted for weeks in New Zealand at the rate of 1000 a day (p. 72). Continuous shaking has been *felt* in Japan during 4½ minutes (p. 73). Recorded vibration-periods vary from two-tenths of a second to one second, and the amplitude may amount to 1 foot without rupture. Some calculations are given of *velocity* of motion of the ground derived from fall of buildings and projection of copings, caps, &c.: these are open to large error from omission of resistance to fracture and of friction. The conclusions as to the rate of earthquake propagation agree with those derived from the artificial earthquakes quoted above.

The most practically useful chapter is that on effects on buildings (a subject recently discussed at the Institution of Civil Engineers), as to which the most important conclusions are—(1) local knowledge alone will guide to the safest site; thus hills, valleys, plains, hard strata, soft strata, &c., are each safe in some, unsafe in other countries. (2) If the direction of shocks be definite, place the blank walls parallel, and the pierced walls perpendicular thereto. (3) Openings at different levels in the same wall to be échelonned. (4) Avoid flat arches, or place wood lintels over them. (5) Avoid heavy copings, caps, &c., and tall chimneys. (5) Use roofs of low pitch. (7) Structures of different vibration-period should not be connected.

Structures of bamboo and timber are seldom injured by earthquake-shock directly; earthquakes are only indirectly destructive to them through fires (caused by the upsetting of lamps) and floods (which sometimes follow).

The earthquake-effects on land are the opening of cracks and fissures, with occasional discharge of water, mud, &c., landslips, and disturbances of lakes, rivers, &c.; also upheavals and depressions of whole tracts: these are —with the exception of the last—seldom extensive. The chief effect on the ocean is the raising of a great sea-wave, sometimes very large, *e.g.* 60 feet high at Lisbon (1761), 80 feet at Callao (1724), 210 feet at Lupatka (1737). These waves are often more destructive on land than the actual shocks; the influx is usually preceded by an outflow, which in fact acts as a warning. One of the most remarkable effects is the distance to which these waves are propagated as "great waves," *e.g.* right across the Pacific. Thus most large earthquakes on the east or west coasts of the Pacific produce waves which are recorded on the opposite coast about twenty-four hours after. From the recorded time of transit of these waves and the known distance the average depth of the ocean (supposed uniform) can be calculated by Russell's formula ($v^2 = gh$); the calculated depths are generally less than the soundings (which err necessarily in excess).

As to source of earthquakes eight methods are given for finding the "epicentrum" or *surface*-origin, and four for finding the "centrum" or *actual* origin, on various assumptions: *e.g.* radiation from a point (viz. the epicentrum), uniform propagation, homogeneous strata, &c. Results depending on such doubtful assumptions can only be very rude approximations.

As to distribution over the world, it appears probable that all parts of the world have been shaken at various times, but that in the historic period the regions most

liable to earthquake are those near to active or recently active volcanoes, especially the Pacific border (which actually contains 172 out of a total of 225 now active volcanoes); also earthquakes are propagated chiefly *along* valleys or ridges.

The distribution of earthquakes in time has been much discussed, but no periodic law either secular, seasonal, or diurnal, either for the world in general or for any one place, is very clear.

After discussing the synchronism of earthquakes with numerous physical phenomena (positions of heavenly bodies, states of air, &c.), the causes of earthquakes are considered, and the conclusion is drawn that the primary causes are probably terrestrial, such as (1) sudden cracks consequent on over-stretching of the earth's crust during elevation; (2) explosions of steam; (3) collapse of hollows produced by volcanic ejection and by the continuous solution and removal of matter by springs; (4) change of load over large areas due to rise and fall of the tides and to changes in air-pressure.

As to prediction of earthquakes, nothing certain is yet known. In many cases there are noticeable changes in springs and wells preceding earthquakes. One useful warning is, however, obviously possible, viz. the report of an actual earthquake on one side of the Pacific could be at once telegraphed to the other side, thus giving twenty-four hours' warning of the probable advent of a great sea-wave.

As to earth-tremors, two curious cases are quoted: (1) the extra crowds of people in Greenwich Park on public holidays cause extra shaking in the Greenwich Observatory instruments; (2) certain delicate observations projected at Cambridge in 1880-82 proved futile in consequence of the continuous earth-tremors masking the delicate effects sought. To these might now be added in London the tremors produced by the Underground Railway. Systematic record of earth-tremors (micro-seismography) has now been made in Italy in many towns for ten years: these tremors appear to be periodic, and to be connected with the sun's and moon's motions, and with the state of the barometer, and to increase *before* earthquakes, so that there is some hope of possibility of earthquake prediction from this research.

The phenomena of earth-pulsations and -oscillations quoted are numerous and interesting, but space fails to enumerate them.

The work begins with an earthquake-map, and ends with a list (10 pp.) of earthquake-literature.

This work is well worthy of its place in the International Scientific Series, and may be accepted as a monograph on its subject by an accomplished seismologist, who, from his residence in Japan, has had ample opportunities of studying the actual phenomena.

ALLAN CUNNINGHAM, Major, R.E.

FRICTIONAL ELECTRICITY

Frictional Electricity. By Thomas P. Treglohan. (London: Longmans, Green, and Co., 1886.)

THIS is a little book written for first beginners in the study of electricity. On the whole it is satisfactory; although the writer betrays curious want of knowledge or want of judgment here and there. The diagrams are good, and the descriptions fairly clear; and from place to place instructions are given to teachers as to experiments they may make before elementary classes for the purpose of illustrating and bringing home to the learners the various parts of the subject. The construction of simple pieces of apparatus, such as a boy may make for himself, is also described throughout the book and in a number of paragraphs at the end.

There are, however, certain points to which we take serious exception. First, we cannot regard Mr. Treglohan's mode of looking at inductive phenomena as correct or satisfactory. For example, speaking of the electrophorus, he says: "If, while the conductor rests upon the excited cake and is under the inductive action of it, the upper surface of the conductor be touched by the finger, the free negative passes to the earth, and an equal quantity of positive enters the disk from the earth." The same statement is made on the following page, so that there is no doubt whatever that the statement about the "equal quantity of positive" is really meant. In the diagrams throughout the book too, where discharge as the result of induction is going on two little arrows are shown, one marked + and the other −, and pointing in dissimilar directions. This seems to us particularising with a vengeance the action of two fluids.

On p. 35, under the heading "specific inductive capacity," we are told that "It was established by Prof. Faraday that, for an excited body to act upon a conductor by induction, some substance must exist between the two through which the electricity may be imparted." Shade of Faraday!

At the end of this paragraph, speaking of specific inductive capacity, we are told that "dry air is superior to moist air in this respect." We do not think that any difference has been proved to exist between dry air and moist air, either as to induction or as to conduction; though there is a common misapprehension (shared by the present author, p. 30) of a difference as to this latter quality.

The use of the condensing electroscope seems to be misunderstood by the author. It cannot be used in the way described for testing "lightly charged bodies." Its main use is for testing a weak but continuous source.

There is also an extraordinary paragraph about a white-hot iron ball on p. 107, for which the author seems to make Prof. Guthrie responsible, and in which the experimenter is told to put a white-hot iron ball on the electroscope in order to prove certain statements! We fear the experimenter will not obtain much valuable information from the experiment; and it is *not true* that, with respect to the supposed indifference of a white-hot body to electrification, "in this it resembles the indifference to magnetism of a white-hot iron ball."

A few misprints we have also noticed. Sir William Thomson's name should be spelled without a "p"; iodine and starch paper is coloured blue, not brown, by ozone. Putting aside these defects, however, this little book will probably be found useful to teachers for the elementary stage in the Science and Art Department, for whose benefit, as we are told in the preface, it has been compiled.

J. T. B.

OUR BOOK SHELF

The Gallery of Marianne North's Paintings of Plants and their Homes, Royal Gardens, Kew. Descriptive Catalogue compiled by W. Botting Hemsley, A.L.S. Fourth Edition. Pp. 160. (London: Printed under the Superintendence of Her Majesty's Stationery Office, and sold at Kew Gardens, 1886.)

THIS is a much enlarged and improved edition of the previous excellent catalogue of these valuable and interesting paintings. The whole collection having been rearranged and as many as 220 additional paintings added, the value of the catalogue, as may be supposed, is considerably enhanced, and the more so as many of the new paintings are the result of Miss North's more recent travels in such noteworthy countries as the Seychelles. The description of each of the pictures, with notes on the habits and uses of the plants represented, are both interesting and useful, the whole being carefully condensed within reasonable compass, but beyond this the present edition is made doubly useful by the addition of a most carefully drawn up list of plants referred to in the catalogue, arranged alphabetically under their natural orders, together with the native country of each species. The sketch of the "general features of the vegetation of the countries visited" is a most valuable addition, each country being treated of separately. These are, as Mr. Hemsley says, "short paragraphs describing the prominent features and peculiarities of the vegetation of the various countries whose floras are illustrated with some degree of fulness therein." Thus, under Chili we have first a general description of its position, character, climate, and meteorological conditions, followed by notes on the vegetation, with references to the more important genera. This part of the book, which forms the introduction, and extends to thirty-one pages, will be extremely useful to all students of geographical botany; indeed the whole book has a value besides that of a mere "guide" to the visitor to the gallery.

We cannot conclude our brief notice of this excellent catalogue without referring to another important feature in this edition, namely, the introduction of a really good map of the world, showing in red the countries visited by Miss North, and in green other floras partially illustrated in the collection.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Thomson Effect

IT affords me much satisfaction to find that my statement of the facts of the Thomson effect is in the main accepted by Prof. Tait. The errors to which I called attention had been copied into at least one recent electrical text-book, and were in a fair way to obtain general recognition as fundamental principles.

The only objection which Prof. Tait raises to my statement is my omission to include a correction for the variability of one of the coefficients with temperature. This is no valid objection, as the limits of space forbade me to encumber my explanation with any unnecessary detail, and my mathematical investigation was avowedly only approximate. The correction thus supplied by Prof. Tait and embodied in his equation (1) is to the effect that, in flowing through a copper conductor, the electric current, while displacing the whole temperature curve in the forward direction (as stated by me), displaces the maxima more than the minima, so as to make the descending gradients steeper and the ascending gradients less steep, the displacement of each point being proportional to its absolute temperature. In iron the same rule holds except that "backward" must be substituted for "forward." Prof. Tait agrees with me that the ordinates are not increased or diminished, but are simply shifted. The current does not tend to diminish the difference between maxima and minima, as a real fluid would do.

The phrases "electric convection of heat" and "specific heat of electricity" have served their purpose, as provisional terms, furnishing a short and easily-remembered way of expressing certain new facts, which would have required for their full expression a long periphrasis; but to retain them any longer in our text-books is to place a needless stumbling-block in the way of teachers and students.

Let Thomson's coefficient σ (hitherto called the specific heat of electricity) be called the *Thomson coefficient*, and let the numbers tabulated by Prof. Tait under the heading "Specific Heat of Electricity" ("Heat," p. 180) be called *tangents of slope*, a name which speaks for itself when the meaning of a thermo-electric diagram is understood. The Thomson coefficient will thus (in the ordinary case) be equal to the absolute temperature multiplied by the tangent of the slope; and the amount of the Thomson effect between two given temperatures will be their difference multiplied by the Thomson coefficient for the mean temperature.

A good name is wanted for the ordinate of any point in a thermo electric diagram. In the first edition of "Units and Physical Constants," being driven to give it some name, and not being able to think of a good one, I employed the makeshift term, "thermo-electric value." In the forthcoming edition I propose to denote it by the more appropriate name, "thermo-electric height." J. D. EVERETT

Belfast, June 12

Black Rain

THE heaviest shower on record fell yesterday afternoon between 6 and 7 p.m. It began at 6.36, and almost six-tenths of an inch fell in the first quarter of an hour. The wind was shifting rapidly at the time from north through west to south. The water collected was very dark, but not so black as that which fell on April 26, 1884. S. J. PERRY

Stonyhurst Observatory, June 10

Meteor

YESTERDAY (Sunday), June 13, at 10.12 p.m., looking eastward, I saw a magnificent meteor, extremely brilliant, darting from southward to northward, at an altitude of about 30°. It must have been a minute or two in view, as I had time to stop walking and watch it describe a long track. When it had passed the prime vertical it burst into a shower of sparks which, falling in a second or two, became invisible. The colour of the meteor was intense white, with a bluish tinge in rear, and only a very slight trail was visible. On exploding the light was crimson for an instant, and the sparks were red.

Should you receive any other notices of this meteor, its height, distance, and magnitude may be computed. It seemed to me of the diameter of a cricket-ball. I have never seen so large a meteor before, or any describe so long a path. The memorable meteor-shower of 1866 (?) exhibited none so large, though possibly many had longer tracks. You may hear of it from the North Sea. R. STRACHAN

11, Offord Road, London, N., June 14

"Arithmetic for Schools"

MR. LOCK is a little loose, not to say unfair, in the drawing of his inferences; I prefer, therefore, to stand by my own words.

(1) When I said "the purely arithmetical part" (not Part) of the book, I meant what I said, viz. those sections where such *theoretical* matters as the finding of the highest common factor, the extraction of the square root, &c., are treated of. The possibility of any reader of NATURE drawing the inference which Mr. Lock succeeds in drawing, viz. that the book is expressly divided into Pure Arithmetic and Practical Arithmetic, is surely a very trivial matter.

(2) At p. 181 the following definition occurs:—"Rate of interest is the ratio of interest to the principal." This I gave as an instance of "slight inaccuracy." It may be a fundamental misconception, and not a slight inaccuracy; but if so Mr. Lock

has no cause for complaint. He ought to know that interest is a function of two variables.

(3) In reply to Mr. Lock's request for other slight inaccuracies I might ask, without leaving the subject of interest, what under the sun "inverse interest" is: but though inaccuracies of language are not desirable in a school-book, I prefer to draw his attention to more important matters. Every arithmetician knows that the practical questions which come under such headings as Simple and Compound Interest, Exchange, Discount, Stocks, &c., are not questions of a different kind *arithmetically*, being all so-called "proportion" questions, and that no more important fact can be taught to the student of arithmetic regarding them. Now here is Mr. Lock's treatment. Simple and compound proportion questions are put under the headings "Problems" and "Complex Problems"—names, by the way, quite illogically chosen and not consistently adhered to. Exchange is tacked on to Complex Problems by the words, "examples in Exchange can be worked by the above method"—indeed, these words and a worked example constitute the sum total of information given in the book on this subject. No one could object to the union here indicated, but surely the same is equally true of several of the other subjects. After Exchange comes a section headed "On Problems concerning Time: I., Time and Distance." These are not problems in the sense previously specified, but belong to the genus of examination questions which concern bodies moving in the same path with different speeds. A like remark applies to the section which follows, headed "II., Time and Work." Late in the day, after Interest, Discount, &c., there appears a chapter "On the Use of the Term *Per Cent.*" So far as it is on anything (for it consists of seven or eight lines of introduction, three worked and forty-five unworked examples), it is on the calculation of rates of gain and loss. Now all this, one is bound to affirm, is strangely illogical, and tends to give a most erroneous conception of arithmetic as applied to practical affairs. I used the expression "slight inaccuracies of thought" in referring to such a mode of treatment, because it was impossible to be more severe without going into detail, and because it seemed imperative to say something against a practice, which our examination system fosters, of forming text-books by collecting all the kinds of exercises met with in examination papers and separating them into carelessly ticketed groups prefaced by a definition or two. The purely arithmetical, and larger, part of Mr. Lock's book is not of this character, and is, especially as regards the definitions, very carefully prepared; he would considerably enhance the value of the whole by wisely modifying the rest in the second edition.
THE REVIEWER

I THANK you for your courtesy in permitting me to see the reply of your reviewer to my letter which appeared in NATURE of June 3 (p. 100). That my letter was written under very exceptional circumstances will be clear to any one who will take the trouble of comparing your reviewer's defence of his criticism with the book itself. I will, however, with your permission, make one or two comments on his reply.

(1) That a wrong inference was suggested by the words of the reviewer is, no doubt, of little consequence, except that it afforded me a ground for an appeal to you for further information.

(2) Your reviewer did not quote in his review, as he now does, my definition of rate of interest; he asked whether rate of interest is totally independent of the time, implying that I stated that it was so, and ignoring the fact that the manner in which time is involved in Interest (not in *rate* of interest, on which point your reviewer seems a little confused) is gradually explained in the next few pages. Might I ask your eviewer whether in Compound Interest the Interest varies simply as the Time?

(3) In his third paragraph your reviewer gives his answer to my request that he should quote *verbatim* the other instances on which he based his unfavourable criticism. There is little or nothing here for me to answer, except that I am compelled, in justice to myself, to point out the reviewer's own mistakes. (i.) He suggests that Exchange ought logically to be placed between Compound Interest and Discount. It would seem necessary to remind him that in questions on Exchange there is no reference to *time*, and that it is the peculiar manner in which time is involved, which distinguishes Interest and Discount from other Problems involving money. (ii.) He states that questions which I have called "Problems concerning Time" are improperly so-called. It will be clear to any one who reads the chapter on "Problems" that a Problem is a question on Variation; so that problems concerning time are exactly what their name indicates. But (even supposing your reviewer were right on these two points) in charging me with being strangely illogical as regards the order of my chapters, he must have overlooked the fact that in the preface I expressly state that "novelty in arrangement has been avoided as much as possible," but that "the order in which his chapters are taken may be varied at the discretion of the teacher." For my part I think that the established order of subjects is not to be lightly upset, certainly not without more sound and weighty reasons than those adduced by your reviewer.

But besides this your reviewer draws an unfair inference, due I suppose, to mere carelessness. The words "Inverse Interest" appear only as the heading of pp. 187, 188, and are obviously an abbreviation for convenience of printing of the words on p. 186, "Inverse questions on Interest." As far, however, as I can understand the general effect of your reviewer's explanation, his objection to my book seems to be this—that it fails to bring into sufficient prominence the fact that the Practical Applications of Arithmetic (which, in accordance with established custom, I have collected under the heads of Exchange, Problems concerning Time, Interest, Proportional Part, &c.), really present the same idea under different circumstances, expressed in different language. I entirely agree with him as to the importance of this fact, and endeavoured, as far as the scope and object of my book would allow, to give it due prominence. For example, for this reason, it seemed unnecessary in Exchange and in the chapter on Profit and Loss to give more than a few words of explanation in addition to the examples worked out.
JOHN B. LOCK

Gonville and Caius College, June 14

PASTEUR'S RESEARCHES

IN the current number of the Royal Agricultural Society's *Journal* (vol. xxi. part 1) is a full and able account of the work of the great French experimenter from an agricultural and veterinary point of view, by Dr. George Fleming. The development of Pasteur's genius is traced from his early chemical researches on dextro- and lævo-tartrates to fermentations in milk and in malt. The combination of microscopic with chemical modes of investigation led him to the definite determination of the part played by living organisms in acetic, butyric, and alcoholic fermentations. In these inquiries his own labours were almost entirely original, but it must not be forgotten that a few microscopists in England and many in Germany were working on the same lines, and contributed to the establishment of the modern doctrine that fermentation and putrefaction are both processes dependent on the presence and growth of minute parasitic plants. Pasteur's experimental investigations led him in two directions—in one to the establishment of the now accepted theory of biogenesis: that every living thing is the product of a living parent; in the other to the practical application of the facts ascertained to the manufacture of vinegar and the process of brewing.

Ingenuity in devising experiments and patience in carrying them to a successful issue belong more or less to every successful investigator, but the union in addition of clear theoretical conceptions with skill in the useful application of results is characteristic of Pasteur as it was of Faraday and a few other of the highest intellects.

His investigation into the cause of *pébrine*, or silkworm disease, was undertaken against his will, in deference to the urgency of the eminent chemist Dumas. Pasteur wished to return to his original department of chemistry, and it is remarkable that having once left it he has been drawn further and further into biological researches, while Dumas, who began with valuable work on the development of the ovum, was diverted to chemistry and there made his enduring reputation. Perhaps no instance more remarkable than Pasteur's work on the *pébrine* can

be cited of the value of science in a commercial and national point of view. A great industry was all but extinguished, and the impending catastrophe became a question for parliaments and statesmen. A scientific investigator was appealed to; he set to work in 1865, and after four years' continued application he had solved the problem, and delivered his country from the incubus on her industry. It has been well said that Jenner by his discovery of vaccination saved more lives than Napoleon ever destroyed; so Pasteur saved France in 1869 from a far greater tribute than the Prussian conqueror imposed on her in the following year.

This brilliant success, which could be neither concealed nor depreciated, led to the successful experimenter (barely recovered from an attack of paralysis, which ended his last laborious research) being called on to devise a means of checking the ravages of splenic fever (*anthrax*) among horses and cattle. Dr. Fleming gives an interest-account of this terrible scourge, and explains the methods adopted by Pasteur to investigate it. He discovered a method by which its virus may be "attenuated," and thus used for protective inoculation in the same way as vaccination protects against small-pox. This method, though often successful, has not proved uniformly so, and more must be done before its general efficiency is established. Dr. Fleming refers to the results in Algeria, in Prussia, and in Hungary, and to these he might have added those obtained by Dr. Roy in Buenos Ayres.

On the other hand, the treatment by inoculation of a contagious disease among poultry (ill-named *choléra des poules*), a method which was also discovered by Pasteur, appears to be uniformly successful.

The last investigation of the great French experimenter is that upon hydrophobia, which the world is still anxiously watching. This also is described by Dr. Fleming. We have kept our readers informed of the progress of this vast practical trial of a scientific mode of treatment on the victims of a hopeless malady. Every month brings fresh accumulation of evidence on the subject, and we hope soon to have the report of the Commission sent from this country to ascertain M. Pasteur's precise methods and their results. If he should be honoured to be an instrument in the hands of Providence for averting one of the most shocking and terrible diseases to which mankind is subject, the name of Pasteur will live as one of the greatest benefactors of our race. But in any case his work already achieved and its results established form an ample title to the admiration and the reverence of all who can estimate genius or value its conscientious devotion to the service of mankind.

LYCOPODS[1]

THE attention of the readers of NATURE has already been directed towards recent work on the Lycopodiaceæ by the publication of a *résumé* of the researches of Dr. M. Treub, Director of the Botanic Gardens of Buitenzorg, Java. He is the first botanist who has succeeded in giving a connected account of the prothallus, sexual organs, and development of the embryo of any species of Lycopodium, and now his first paper, which dealt with *L. cernuum*, has been rapidly followed by a still more complete and successful study of *L. Phlegmaria*, L. It might be expected that the second paper would be in great measure a repetition of the first, but this is not so; and it may be regarded as one of the most interesting results of this very suggestive and luminous investigation that it brings into prominence the greatness of the possible differences in development of two plants which have hitherto passed, and will continue to pass, under the same generic name. The observations detailed in this second paper are so important in their bearings on our views regarding other allied forms that it is desirable that at least the more striking points should be recorded here.

[1] "Études sur les Lycopodiacées," par M. Treub. Part II. *Annales du Jardin Botanique de Buitenzorg*, vol. v. 2ième partie.

Attempts to germinate the spores of *Lycopodium Phlegmaria* were at first unsuccessful, but after more than a year young plants were found on one of the tree trunks on which spores had been sown, and subsequently similar young seedlings were found in large numbers in the forest. The germination of the spores appears to be slow, and Dr. Treub is of opinion that the culture of prothalli from spores will never be easy, a view which is supported by the fact that the oophore is capable of various modes of asexual multiplication; indeed it appears that the majority of the prothalli found owed their origin to this source, and not directly to the germination of spores. An autonomous existence of the prothallus, independent of the formation of sexual organs, has been demonstrated by Goebel in the case of *Gymnogramme leptophylla*, and a similar, but still more pronounced condition is found in this Lycopod. The prothallus grows in the dead external layers of the bark of trees; it is as a rule devoid of chlorophyll, and consists of cylindrical branches, covered with absorbing hairs. These cylindrical organs branch monopodially, the branches being usually formed in acropetal order; they have a terminal growth with two initial cells, each of which gives rise to half of the cylindrical organ. It is worthy of note that there is a great similarity between the structure of this apical meristem and that of the stem of the sporophore. In the fully-differentiated parts of the prothallus a peripheral tissue one layer of cells in thickness may be distinguished; this gives rise to the rhizoids. The mass of tissue inclosed by this superficial layer, though it shows some slight varieties according to the mode of development of the branch, never attains any high state of differentiation.

The lateral branches, which are not very numerous, take their origin from the peripheral layer, several cells taking part in the formation of each. The growth of these branches may be long-continued, and it is not arrested on the formation of an embryo on another branch. By progressive rotting of the older parts branches may be separated from one another, and this constitutes the simplest mode of increase in number of individuals But, besides this, two other modes of vegetative propagation are known—(*a*) by ordinary propagating organs: these are small ovoid multicellular bodies, which originate from single superficial cells, and are set free by rupture of their pedicels; (*b*) by thick-walled organs, smaller than the above, which only appear on weakly prothalli: these may undergo a period of rest. Among the vascular Cryptogams the only organs hitherto known of a similar nature to these are those described by Cramer; Dr. Treub is, however, of opinion that a truer comparison may be be made to the gemmæ of the Hepaticæ, and especially of *Blasia*, while in many of their general characters, which may be recognised on inspection of the twenty beautiful plates, the prothalli of *L. Phlegmaria* show points in common with the oophore of certain of the Muscineæ.

The sexual organs of *L. Phlegmaria* are produced on the upper surface of the prothallus, and are always accompanied by paraphyses, structures which are absent in other Vascular Cryptogams, but frequently present in the Muscineæ. The position of the antheridia is variable; sometimes they are scattered singly on the vegetative branches, sometimes they are associated in groups, and are then often borne on the considerably thickened extremities of branches. Their development is similar to those of *L. cernuum*, while the antherozoids have two cilia, and resemble those of *Selaginella*. The archegonia have a more definite position, and they appear subsequently to the antheridia, on those thickened extremities of branches which have already borne antheridia: they project from the surface of the prothallus, and have three to five canal cells, while the highest number hitherto recognised

among Vascular Cryptogams is three; this is again a point in common with certain Muscineæ.

This would not be the place to enter upon those details of the mode of development of the embryo, which Dr. Treub has worked out with such signal success. It must suffice, while referring those who are specially interested in the subject to the original paper, to state merely the most prominent facts. In the first place there is a considerable difference between the development of the embryo in *L. cernuum*, and that of *L. Phlegmaria*, while in certain points the latter corresponds to *Selaginella Martensii*. Thus the ovum in *L. Phlegmaria* divides first by a wall perpendicular to the axis of the archegonium into two: of these, the cell next the neck becomes the suspensor, the other is the mother-cell of the embryo; the latter develops ultimately into a multicellular mass arranged in two tiers: the lower tier forms only the massive "foot," while from the upper (*i.e.* that further from the neck of the archegonium) are derived the stem and single cotyledon, and ultimately also the first root. The mode of origin of the root is interesting in connection with my own recent observations of the exogenous origin of the root in Phylloglossum. According to Dr. Treub's observations, the first root of *L. Phlegmaria* is at first covered by an envelope a single layer of cells in thickness, which cannot rightly be regarded as the outermost layer of the root-cap; accordingly we have the barest possible example of endogenous formation, only a step removed from the exogenous. These and other results of the investigation of the development of the embryo of *L. Phlegmaria* afford fresh material of the greatest value for comparison, not only with other groups of the Vascular Cryptogams and with the Muscineæ, but also with other species of the genus Lycopodium. Further, the full account given of the prothallus provokes a comparison which Dr. Treub has embodied as follows (p. 88):—"As far as it is possible to judge at present, we find in the sexual generation of the Lycopods, more clearly than elsewhere, transitional terms between the great series of the Muscineæ and that of the Vascular Cryptogams." Some readers will doubtless call to mind, in connection with this, a striking passage by a well-known botanist, Prof. Goebel, written a few years ago (Schenck's *Handbuch der Botanik*, Bd. ii. p 401), which runs thus:—"We must then satisfy ourselves by asserting that the gulf between the Mosses and Pteridophyta is the deepest that we know in the vegetable kingdom, and bridging it over by hypotheses and explanations does not make it one whit the less."

In this treatise of Dr. Treub we are put in possession of those positive observations which, beyond their intrinsic and independent interest, acquire the highest possible value from the fact that they fit into this wide and deep gulf, and materially help to fill it up. Such observations, and the theoretical considerations which follow them, are sure of a hearty welcome among the fellow-countrymen of Charles Darwin.

I cannot close this article without a brief reference to the peculiar case of symbiosis found in the prothalli of *L. Phlegmaria*. Endophytic Fungi have already been described in prothalli of other species, and here Dr. Treub finds the tissues constantly infested by a fungus, apparently one of the Peronosporeæ. Its thin filaments inhabit the interior of the cells themselves, but without killing them, the nuclei of the cells remaining normal, while the growth of the prothallus does not appear to be visibly hindered by its presence. It would appear that we have here a case of "commensal" symbiosis, in the strictly literal sense; unfortunately it is impossible as yet to follow out the subject thoroughly into its details, but we may hope that Dr. Treub may be able shortly to give us some more general insight into the economic relations of the two organisms thus amicably associated together.

F. O. BOWER

THE UNITED STATES FISHERIES[1]

THESE two volumes, with the familiar black cloth binding, shiny paper, and plates of photo-engravings, characteristic of American official publications, are the first instalment of a series, which is to contain the results of an exhaustive survey of the United States fisheries from all possible points of view. The purpose and method of the survey, and the history of its origin and progress, are sketched in a prefatory note by Mr. Spencer F. Baird. In 1879 it was arranged that the Tenth Census, which is under the direction of General Francis A. Walker, should co-operate with the Commission of Fish and Fisheries in carrying out an historical and statistical investigation of the fishery industries. The direction of the whole survey was intrusted to Mr. G. Brown Goode, Assistant Director of the National Museum, who had for some years previously devoted a large portion of his time and energies to the study of the fisheries. The work to be carried out was divided by Mr. Brown Goode into seven departments:—(1) Natural history of aquatic products; (2) the fishing grounds; (3) the fishermen and fishing towns; (4) apparatus and methods of capture; (5) products of fisheries; (6) preparation and manufacture of fishery products; (7) economy of the fisheries. The co-operation of every person who had any special knowledge of the subjects under consideration was secured. The field-work was so divided that each portion could be assigned to men who were most competent from their previous experience to undertake it. The shad and alewife fisheries, for example, were assigned to Colonel Marshall MacDonald, the Alaska fisheries to Dr. T. H. Bean.

It was understood from the beginning that the results obtained should be set forth in a series of finished reports, of which those referring principally to the exploited organisms, namely, fish and aquatic animals, should be presented to and published by the Fish Commission, while those dealing with the exploiting organisms, the fishermen and manufacturers, should be the property of the Census Office. The expenses of the work have been shared between the Commission and the Census. The reports prepared for the Fish Commission being too bulky for publication in the annual reports, permission was obtained from the Senate and House of Representatives to publish them separately. The series will be as follows:—Section i. natural history of useful aquatic animals (the two volumes now before us); ii. the fishing grounds; iii. the fishing towns; iv. the fishermen; v. the apparatus of the fisheries and the fishing vessels and boats; vi. the fishery industries; vii. the preparation of fishery products; viii. fish culture and fishery legislation; ix. statistics of production, exportation, and importation; x. the whale fishery; xi. a catalogue of the useful and injurious aquatic animals and plants of North America; xii. a list of books and papers relating to the fisheries of the United States; xiiii. a general review of the fisheries, with a statistical summary.

The statistical reports prepared for the Census Office are ten in number. The results they contain have been already partially published in Census bulletins and in statistical tables scattered here and there in various volumes. The prefatory note concludes with a brief summary of the statistics of the fisheries. In 1880 the number of persons employed in fishery industries was 132,426, of whom 101,684 were fishermen. The total value of the capital invested was $37,955,349.

After the prefatory note we find the letter of transmittal from Mr. Brown Goode to Prof. Baird. In this it is stated that the work is intended especially for the use of the reading public, and technical zoological discussions

[1] "The Natural History of Useful Aquatic Animals of the United States," forming Section i. of "Fisheries and Fishery Industries of the United States." 1 vol. Text; 1 vol. Plates. 4to. (Washington, Government Printing Office, 1884.)

and descriptions have not therefore been included. On another fly-leaf is a list of the authors who have had a share in the production of the work. The number of these is no less than twenty, and among them are such familiar names as Tarleton H. Bean, John A. Ryder, and R. Edward Earll.

The work is divided into five parts: i. mammals; ii. reptiles and batrachians; iii. fishes; iv. mollusks; v. crustaceans, worms, radiates, and sponges. Of the mammals the whales and porpoises are described by G. Brown Goode; seals and walruses, by Joel A. Allen, and Henry W. Elliott, who contributes a chapter on the life-history of the fur-seal; manatees and the Arctic sea-cow, by Frederic W. True. The reptiles and batrachians are also treated by Mr. True. Mr. Brown Goode is responsible for the greater part of the portion dealing with the fishes. The part on mollusks consists of two chapters, one on mollusks in general, by Ernest Ingersoll; one on the oyster, by John A. Ryder. Part V. is the work of Richard Rathbun.

The work of Mr. Brown Goode is always lucid, systematic, and complete. In his account of the whales and porpoises he does not give technical zoological diagnoses, these being, as we have already mentioned, intentionally omitted throughout the work, but he gives the accepted name with its authorities accurately indicated. He describes fully, with references to all the literature of the subject, the distribution, habits, food, and reproduction of all the species having an economic value. Figures of nearly all the species are given; these are taken from various sources, some prepared specially for the present work, some copied from the plates of existing zoological memoirs.

A discrepancy occurs between the title of one of the figures and the description contained in the text: the porpoise sperm whale is stated to have been described by Prof. Gill, under the name *Kogia Floweri*, while the figure given is entitled *Kogia Goodei*, True, the pygmy sperm whale. Two sketches illustrating the whale fishery are reproduced in Plates 3 and 10. The account of the "right whales" is not altogether clear. It takes some time to find out that the species generally known as the "right whale" is *Balæna mysticetus*, L., which is the Arctic whale, or bowhead; while the true right whale is Eubalæna, Cope: but the assertion that *Eubalæna cisarctica*, Cope, is not remotely related to *Eubalæna biscayensis* of the Eastern Atlantic, remains a puzzle.

Mr. Allen's work on the seals is thoroughly satisfactory, and the history of the fur-seal at the Pribylov Islands, given by Mr. Elliott, contains the results of accurate personal observation, which has at last elucidated the meaning of the peculiar and long-known habits of this species. The movements of *Callorhinus ursinus* when absent from its breeding places remain for the present obscure, but the reason why it seeks its breeding places so regularly, and the facts of its reproduction—knowledge of which is necessary in order that a permanent diminution of the numbers of the animal may be avoided—are clearly set forth in this essay.

The illustrations of the account of the seals and of Mr. Elliott's essay are particularly good. Among the former are two maps of the world, showing at a glance the geographical distribution of the useful seals. Mr. Elliott's original sketches of the fur-seal at home in the Pribylov Islands are very spirited and interesting.

Mr. True gives an account of the South American manatee, and reviews lucidly the history of the extinct Rhytina of Behring's Strait.

The chapter on the reptiles and amphibians is entirely unillustrated, for what reason does not appear. The reptiles which afford products useful to man are the alligator, the turtles and tortoises, and one frog—*Rana catesbiana*, Shaw—the bull-frog. This last animal is cultivated in several localities, the eating of the hind-legs being common in most towns of the States.

The note at the commencement of Part III. on the food-fishes is a little inconsistent. "We anticipate the criticism that the book is of no use in identifying the different kinds of fish, by the statement that we expressly desire that it shall not be," is one sentence; and another is, "Most of our important species can be identified by reference to the plates." What the writer evidently means to say is that each species mentioned is accurately figured and receives its correct technical name, so that any one interested in fishes can find out the zoological name of his specimens from the plates, and can read all about range and economical uses, while for more detailed scientific treatment reference must be made to speciegraphical works in ichthyology. Various ichthyologists have contributed to this portion of the work. The fishes of the Pacific coast are the special province of David S. Jordan, while one or two species, like the Californian salmon and the carp, have been allotted to pisciculturists specially familiar with them. Many vexed questions in the biology of fishes are discussed by Mr. Brown Goode with his usual lucidity and comprehensiveness. The pages on the reproduction of the eel, for example, are very interesting reading, and this is by no means a solitary example. The food-fishes naturally take up a large portion of the whole work. They occupy more than half of the volume of text, extending to more than 500 pages. In the plates there is one feature which we have after serious efforts completely failed to understand. On nearly every plate there is a straight line below each figure, apparently intended as some standard of measurement; but the meaning of these lines is not explained.

In his chapter on the mollusks Mr. Ingersoll has not always observed the rule strictly followed in the rest of the work of giving the authority for each specific name used. He gives an account of the distribution of the numerous other species of Lamellibranchs used as food in the United States, but gives no description of oyster-beds. In Mr. Ryder's account of the life-history of the oyster there is a great deal of interesting detail about anatomy and development, and about the writer's own experiments in oyster-culture, but a general account of the distribution of *Ostrea virginica* is wanting. This is a surprising omission, and one much to be regretted.

Why Mr. Rathbun, even in a work intended for general readers, should unite together Echinoderms and Cœlenterates as Radiates is a question which it would be difficult to answer. The name Radiata would require to be considered in a history of zoology, but it is impossible to justify its use in the classification of animals for any purpose in the present state of science. But this and the other slight defects we have pointed out do not make a very great reduction in the value and completeness of the whole work. The labour spent in its preparation has been very great, and the result is a lasting monument to the industry and scientific capacity of Mr. Browne Goode and his numerous fellow workers.

REMARKS ON THE EGGS OF BRITISH MARINE FISHES[1]

THE majority of marine fishes, in regard to reproduction, readily range themselves into certain groups according to the condition of the eggs on deposition. Thus (*a*) a considerable number have delicate pelagic ova, which are generally separate, though in the frog-fish, for instance, they form gelatinous masses. (*b*) Others are characterised by the deposition of thick-walled ova, connected together in more or less firm masses, on or near the bottom, or in special nests. (*c*) A third group is distinguished by laying ova which have filamentous processes or adhesive surfaces for attachment to foreign structures; and some place them in brood-pouches of the males, in which case, however, the capsules appear to be

[1] By Prof. McIntosh, F.R.S., &c., St. Andrews Marine Laboratory.

more delicate. (*d*) A fourth series have their large eggs enveloped in dense horny capsules, which either are fixed by their twisted filaments to marine bodies or find sufficient protection on the extensive sandy flats where they are deposited. (*e*) Finally, a few produce living young, this condition ranging from the well-marked ovo-viviparous *Zoarces* to the even more complex state in the sharks.

It would seem, as far as present observations go, that in those fishes which shed their eggs on the bottom, or in brood-pouches, the ova are matured simultaneously in the ovaries, so that the act of deposition is performed rapidly. This is exemplified in the *Cotti*, in *Agonus* (*Aspidophorus*), *Cyclopterus*, *Liparis*, the herring, and others. In the case of fishes with pelagic ova, on the other hand, the ovaries mature and shed their contents at intervals, so that the process of spawning occupies a period of greater or less duration.

There is little difficulty in the case of the pelagic ova of our shores, such as those of the cod, haddock, whiting, bib, ling, rockling, gurnard, and others in artificially impregnating and hatching them, even from fishes that have been dead for some hours. The mortality, however, from excessive cold and heat is very considerable in a marine laboratory, since the limited quantities of sea-water contained in vessels a foot or even a yard or two across are much more subject to such influences than the vast body of water in the sea. It has to be borne in mind also that the sea-water usually employed in such researches is shore-water, and liable to considerable contamination from the estuaries of rivers and streams—besides other impurities. The difference, indeed, between such water and that of the open sea was illustrated in 1884 (NATURE, vol. xxxi. p. 536), when the pelagic ova of the cod could be more successfully hatched in the large glass vessels ("drop" bottles) in which they were conveyed from the fishing-ground without change, than in the ordinary water dipped from the shore and frequently renewed. In like manner eggs of plaice fertilised on the same ground this year (for which I have to thank Capt. Burn, of St. Andrews) were conveyed quite safely, even after a week's vicissitudes in a stoneware jar amongst sea-water—lightly tied over with "cheese" cloth. During the late winter ova of various kinds suffered severely, however, and the effects of such changes of temperature on the embryos were even more pronounced.

The first series of eggs of the haddock were fertilised on the 15th, and the second on the 16th of February, but the rigorous weather proved ultimately fatal to both. The earlier stages proceeded satisfactorily, but the water in the vessels by and by was frozen on the surface —softish flakes of ice forming a thick coating—on which many of the ova were elevated. No sooner was this ice broken than all or almost all the ova were observed to present the whitish patch and sink to the bottom. Some of those which had floated in mid-water or under the trickle from the supply-pipe escaped destruction, but in a few days they also succumbed after a night of unusual severity, and after the embryos had been outlined. On the other hand, a few ova carelessly thrown at the same period into a small vessel of sea-water in the window of a library escaped injury and developed quickly, though the water remained unchanged.

In the sea the danger from such extreme cold would be minimised, since these pelagic eggs in winter and spring do not float quite at the surface, but always some distance beneath it.

Under the same circumstances in the laboratory the intense frost proved fatal to many adult viviparous blennies and Montagu's suckers, though only the surface of the sea-water in the large glass vessels was coated with the softish flakes of ice. The fluid in the ovaries of the pregnant examples of the former was frozen into a solid mass, as was also the liquid in the urinary bladder, yet the animals were surrounded in all cases by sea-water.

In the Report to H.M. Trawling Commissioners in 1884 reference was made to the statement by Alex. Agassiz (*Proceed. Americ. Acad. of Arts and Sci.* xvii. p. 289, 1882) that the ova of several species of *Cottus* float. In his recent beautiful memoir along with Whitman (*Mem. Mus. Comp. Zool.* xiv. part i. 1885), he again returns to the subject—giving figures and descriptions of the ova of the so-called *Cottus grœnlandicus*, Cuv. and Val., which he found in a pelagic condition abundantly during the summer months, especially in July. The authors, indeed, appear to have met with the ova only on the surface of the sea, and do not seem to have identified them with those in the ovary of the species indicated, which in our country is supposed to be only a variety of *Cottus scorpius*, L. Unless, therefore, the *Cottus grœnlandicus*, C. and V., of Prof. Agassiz, is a form very different, there is room for doubt in regard to this interpretation of its oviposition.

The spawning of the *Cotti* in this country wholly diverges. Instead of the issue of the eggs in detachments, as in most fishes with pelagic eggs, the ovaries of the *Cotti* become distended at the breeding-season with ripe eggs of a uniform size, which are generally deposited in a mass at once—along with a transparent mucous secretion. When ejected into the water the eggs adhere together, but at first they can hardly be lifted on account of the soft and yielding nature of the connecting medium, though they do not readily separate. In a few hours the hardening of the connecting medium and the egg-capsules stiffen the outer layers of eggs, but the central region is still soft. The process of hardening is thus somewhat slow, and apparently depends on free contact with sea-water. These eggs are comparatively large and thick-walled, as well as slow in development, the embryo being ushered into the world in a much more highly organised condition than in the embryos from pelagic eggs. There is, indeed, little resemblance between Agassiz's form and the young *Cottus*, which is considerably larger, is variegated with much pigment, has rudimentary lamellæ (papillæ) on the branchial arches, complex circulatory organs, and a small yolk-sac possessing a single large oil-globule; and it shoots upward into the surrounding water like the young *Liparis* and *Cyclopterus*.

While the newly-hatched *Cottus* therefore greatly surpasses Agassiz's type in complexity, there are certain marine forms, *e.g. Anarrhichas*, which as greatly surpass *Cottus*. This will be evident when it is mentioned that the strongest embryos of the wolf-fish are much more highly developed on their escape from the egg than the salmon is for a week or two subsequently. Artificial stocking of the sea with the valuable food-fishes, such as the cod and haddock, would have been comparatively easy if their ova and embryos had been as readily handled and reared. However, since a noteworthy increase in tenacity has been observed in certain forms as soon as the yolk-sac has been absorbed, there is room in this respect for further investigation.

THE HONG KONG METEOROLOGICAL OBSERVATORY[1]

THIS first-class meteorological observatory was erected in 1883, and the regular work of observing began on January 1, 1884. Weather Reports appear monthly, and we have now before us the observations and work of Mr. Doberck and his staff for the first two years. For the first two months the work was restricted to eye-observations, but meanwhile no time was lost in erecting the barograph, thermograph, anemograph, pluviograph, and sunshine recorder, which are similar to those in use at Kew; and from April 1, 1884, the Monthly

[1] "Observations and Researches made at the Hong Kong Observatory in the Years 1884 and 1885." By W. Doberck, Government Astronomer.

Reports include a continuous hourly record of the more important elements of the climate of Hong Kong. The buildings are erected on the peninsula of Kaulung, facing the harbour, on the top of Mount Elgin, a small eminence rising from the plain to a height of about 110 feet above mean sea-level. It may also be noted that the ground has been carefully turfed where the instruments are placed. In addition to the usual tabulations and their averages, the Monthly Report gives a carefully observed log of non-instrumental phenomena, such as dew, fog, unusual visibility, halos, and thunderstorms.

The results show that the amplitude of the daily range of the barometer is greatest from November to February, when the rainfall is least and the air driest, the mean difference during these four months between the morning maximum and afternoon minimum amounting to 0·102 inch. On the other hand, the mean of the four months from June to September, when the monthly rainfall nearly equals 12 inches, only amounts to 0·069 inch. The diurnal range of temperature is small, being for the year only 5°·5, the maximum, 7°·2, occurring in December, and the minimum, 4°·0, in February. The daily minimum occurs at all seasons shortly before sunrise, and the maximum from 1 to 2 p.m. during the dry season, but an hour later during the wet season. The hourly means for the tension of the aqueous vapour are very interesting, as showing very clearly for those months when the sunshine is daily practically constant and the air relatively dry a minimum period during the hottest hours of the day; whereas when the sunshine is much interrupted, the rainfall frequent, and the air moist, the daily maximum tension occurs at these hours.

For the twelve months beginning March 1884, the greatest amount of sunshine was from noon to 2 p.m., and the least from 4 to 5 p.m., the former being per hour nearly double the latter. During the 22 months the greatest monthly number of hours of sunshine for any hour of the day was 26·3 hours from 9 to 10 a.m. of October 1884 out of a possible 31 hours. From midnight to noon the mean monthly rainfall has been 4·98 inches, but from noon to midnight the amount has only been 2·73 inches. The four consecutive hours of largest rainfall are from 5 to 9 a.m., amounting to 1·91 inch, and the four consecutive hours of least rainfall from 8 p.m. to midnight amounting only to 0·76 inch, or considerably less than half the former time of the day. The diurnal period of the rainfall of Hong Kong is remarkable as showing the maximum fall during the period of rising temperature, and the minimum when temperature is rapidly falling, the amounts for the six hours ending noon being 2·66 inches, and for the six hours ending midnight 1·24 inch. Future observations will doubtless modify in some degree the curve of daily rainfall, but from the general accordance of the fall of the individual months with what is indicated above, it is not likely that the change of the curve will be very material.

The daily curves for the winds, both as regards velocity and direction, are very decided. The daily curve for wind velocity has, for Hong Kong, owing to its peculiar position with reference to the island and the continent, peculiar features of its own. Thus for the year the maximum velocity extends from 10 a.m. to 2 p.m., the means for these four hours being the same, while the minimum velocity extends from 6 to 10 p.m., the hour of least movement being from 7 to 8 p.m. From midnight the wind rises to the daily maximum at 10 a.m. The month of greatest force of wind is March, and of least August, the air-movement in the former month being nearly double the latter. As regards direction the wind is about E.N.E. in the winter and E.S.E. in the summer season. For the whole year, the mean direction is E. 3° S., and the diurnal variation from E. 5° N. at midnight to E. 15° S. at noon, the mean variation being thus through 20°. During 1884 the total distance travelled by the wind was 103,237 miles, and of these 63,349 miles, or more than half the whole, was east wind. The least frequent wind is N.W., which showed only 2053 miles.

At a distance of about two miles from the Observatory an important station has been established on Victoria Peak, at which observations are made at 10 a.m. and at 4 and 10 p.m., and the results are published *in extenso* in the Monthly Report. The height of this station is 1823 feet above sea-level. These two almost contiguous stations, the higher being on a peak and the lower also on an eminence sloping directly down to the sea, form an admirable pair of stations for furnishing, in the best procurable form, the observational data necessary for some of the more important physical inquiries of meteorology. So far as we are aware, no pair of stations can be placed side by side with Hong Kong Observatory and Victoria Peak as affording the data for the physical inquiries referred to, except Ben Nevis Observatory and the station at Fort William.

Of these inquiries the important practical question of the rate of decrease of temperature with height may be cited as an example. The remarkable suitability of these two groups of stations for advancing this inquiry lies in the circumstance that in each case the upper station is situated on a true peak, thus reducing to a minimum the influence of the land in changing the temperature of the winds before arriving at the Observatory; and that the lower station is on a rising ground near the sea and sloping down to it, thus minimising the disturbing effects of radiation. At Hong Kong the rate of decrease of temperature with height is 1° for 261 feet in winter; 347 feet in spring; 262 feet in summer; 254 feet in autumn; and 281 feet for the year. At Ben Nevis the rates are for the seasons 279, 251, 268, and 290 feet, and for the year 270 feet—the results being thus closely accordant. On the other hand, such a pair of stations as Obirgipfel in Austria, on a peak 6706 feet high, and the neighbouring station at Klagenfurt, 1437 feet high, cannot furnish the data necessary to this inquiry owing to the circumstance that the lower station is situated in a deep valley. The result is that in January the difference of the mean temperatures of the two stations is less than 1°, although the one is 5269 feet higher than the other; whereas in May the difference of their mean temperatures is 22°·0.

It is earnestly hoped that the publication *in extenso* of the hourly observations at Hong Kong will not be limited to ten years, as seems to be hinted at in the Report, but that the meteorological observations and their publication will be made a permanent part of the work of the Observatory. The unique position of Hong Kong with respect to the great continent of Asia and its meteorology will no doubt secure this object.

CHOLERA IN ITS RELATION TO WATER-SUPPLY

THE epidemic of Asiatic cholera, which has been raging in Spain during the last two years, and which appears even yet to be lurking in some portions of that peninsula, has furnished some interesting data as regards its connection with water-supply, to which it would be wise in us to direct our attention, not only from the interesting nature of the facts as such, but also because it is not improbable that ere the disease quits Europe it may visit our own shores.

Broadly speaking, it would appear that in Spain this formidable disease never became truly epidemic or dangerous in any city in which there was a pure and good supply of water, and proper means were taken to guard against the sources being polluted by any of the specific choleraic poison.

In support of this idea I would desire to call attention to the cities of Toledo, Seville, Malaga, and Madrid, in contradistinction to such places as Aranjuez, Saragossa, Granada, and Valencia. I will commence with Madrid.

This city, whose population at the last census was 397,816, suffered very severely indeed under the last epidemic of 1865, when during several days immediately following a very severe thunderstorm the number of cases varied from 800 to 1200 per day. The first invasion of last year took place in Madrid on May 20, and the disease ran its course during the whole of the summer, gradually disappearing towards the end of the month of September. The total number of cases during the whole of the period was 2207, and the deaths 1366. The total number of cases, therefore, during the five months that the disease never abandoned the city was barely more than what occurred during two days only of the epidemic of 1865, being little more than ½ per cent. of the population. I think, therefore, we may safely say that the disease never assumed a truly epidemic form. The greatest number of cases, as was to be expected, took place during the months of July and August; the first notable increase took place on July 25, and the first notable decrease on August 13.

In connection with this it is interesting to note that Madrid was subject to severe thunderstorms during the latter end of July, and that 119 millimetres of rain fell during the month. These storms began on the 13th, and were especially severe on the 23rd, 24th, 26th, 27th, and 31st, the first notable rise in the cases of cholera occurring between the 25th and 28th. As a general rule, no rain falls in Madrid in July, and the occurrence of these severe thunderstorms and heavy falls of rain was quite phenomenal.

The new water-supply from the Guadarama Mountains was completed shortly before 1865, and the greater part of the drainage was also finished; but at that time the new water supply had scarcely come into use, the large majority of the houses being supplied from the old fountains which existed in various parts of the city. During the last twenty years the use of the Lozoya water has become very general, and an ample supply has been provided for washing the streets and flushing the sewers.

Madrid is now well drained; the sewers are built upon the Paris model, and are not what an English engineer would consider as a good type for self-cleansing purposes, but the fall is, in almost every case, very great, and it is not probable that there can be any collection of fœcal matter at any point. The connection of the street gulleys with the main sewers is made without any trap, and good ventilation is thus provided. As regards the outfall of these sewers, nothing satisfactory can be said. The mouths of the main sewers, which are seven in number, all discharge on the southern side, between the station of the Saragossa Railway and that of the Northern.

The question of the proper disposal of the sewage in Madrid, as in London, has never been decided, and pending this decision the sewers were completed only as far as the outlying houses of the city, and the sewage was then allowed to find its way down to the Manzanares, in the best way it could. During the time the question has been awaiting a solution the town has extended, and houses have been built along the course of these open sewers. As might have been expected, the first serious outbreak of cholera occurred about these spots, the original germ of the disease having been imported from the neighbourhood of Valencia, where the cholera was then raging.

The existence of the disease having been established beyond doubt, one of the first acts of the Municipality was to attend to the water-supply. There existed 12 ancient sources, which supplied 85 taps or fountains, 22 of which were public ones, at which water-carriers were allowed to fill their barrels, and the remaining 63 belonged to groups of houses. In spite of the excellent supply brought in from the Lozoya, these old sources were still a good deal used by the inhabitants—many, from old habits, preferring to use the same water which their fathers had used, many not being willing to incur the expense of laying on the new supply. In view of the impossibility of effectually guarding against the possible contamination of so many sources of supply, the Municipality, by decree on June 18, closed all the old ones with the exception of that of La Fuente de la Reina, which supplied five public fountains and four private ones. The Central Government undertook the custody of the Lozoya aqueduct, the Municipality took charge of the Fuente de la Reina. The Lozoya water is drawn from the sources of the River Lozoya in the Guadarama Mountains, some 50 miles to the north of Madrid.

The river takes its rise in the granite formation; the water is excellent, and from the uninhabited condition of the country through which the river flows before the intake, it is not exposed to direct contamination from any specific poison. From the intake to Madrid the water is conducted by a series of magnificent works, partly covered, partly uncovered, to Madrid, where it is received in covered reservoirs before being distributed in the city; the service is continuous, no cisterns being used. During the whole time of the existence of cholera in the city the uncovered portion of the aqueduct was patrolled by armed guards, no one being permitted to approach without a special order.

Accompanying the extensive Report of the Mayor of Madrid, Don Alberto Bosch, amongst other plates is an excellent map of the city, showing, by a red dot, the situation of every case of cholera that occurred; they are seen pretty thickly scattered about the uncovered exits of the sewers, and on both sides of the River Manzanares—which is, in fact, in summer an open sewer—and in the lower portion of the city overlooking the river, and there is scarcely any part of the town where a dot is not to be found; but, with the exception of the points mentioned, the cases occurring in the remainder of the town seem to be all isolated ones; in extremely few cases do two dots occur together, showing that the disease was more of a sporadic than of an epidemic character.

Now let us take the case of Toledo. This ancient capital of Spain is certainly not a city that could be taken as a model of sanitary arrangements; on the contrary, it seems to be admirably adapted to form a good nest for any wandering epidemic, and yet, although the cholera entered it in the summer of 1884, and did not finally leave it till the autumn of 1885, the total number of cases, according to official returns, did not exceed 200, of which about one-half were fatal. The population of Toledo is over 20,000, so that the percentage of choleraic disease was only about 1 per cent. of the population for the two seasons.

Toledo was supplied with water from the River Tagus, which flows round the city, the water being lifted by pumps. Above Toledo, on the same river, is situated Aranjuez, and above Aranjuez again, on the Manzanares, which is a feeder of the Tagus, is situated Madrid, in both of which towns the cholera existed in 1885, being unusually severe in Aranjuez. The Governor of the province, recognising the suspicious character of the water, stopped the pumps, and obliged the inhabitants to send for their drinking-water to a distant spring; he even forbade any one to bathe or wash clothes in the river. The measure was a strong one, but it saved the city.

Let us next take Seville. Seville is an important city, the third in rank in Spain; it contains, according to the census of 1877, 134,318 inhabitants; it has, strictly speaking, no drainage; a few ancient sewers exist for carrying off the rain-water from the lower portion of the city, but sewerage for houses does not exist. The sewage goes into cesspools, which are, in most cases, situated just outside the house, and under the street; the inhabitants are extremely cleanly in their habits, and the outsides of their dwellings are constantly whitewashed, but it is not

a healthy city—typhoid fever is endemic, and the death-rate rises in some parishes to 35 per mil.

Seville is situated on the River Guadalquiver, of which the Rivers Darro and Genil, that flow through Granada, are feeders; as regards its water-supply, one suburb of the city, called Triana, containing about 30,000 inhabitants, is situated on the western side of the river. This portion is almost entirely inhabited by the poorer class, and they drink generally the water of the river.

The rest of the town is supplied from an ancient Roman or Moorish aqueduct, the water being brought from an underground spring near the town of Alcalà, about nine miles to the east of Seville; this water is carried by a tunnel about two miles in length under the town of Alcalà; it is then carried in a covered conduit to within a short distance of Seville, and from thence by an aqueduct made by the old Moors. The water is excellent.

An English Company has quite lately erected engines at Alcalà, by means of which they pump up to a covered reservoir above the town the water from two other springs, situated also at Alcalà, but on the opposite side of the river Guadaira, which flows past the town. This water is carried from the reservoir into the town by iron pipes, and distributed under considerable pressure; in character it is pure and excellent; the springs rise from the base of the sandstone at a short distance from the engine-house, and are carried across the river by an iron pipe. The cholera broke out in Granada on July 14, 1885, but already on June 14 of the same year the authorities of Seville, by way of prevision, had prohibited the use of any water from the river, either for dietetic or other purposes; had authorised the English Company to lay a temporary pipe across the bridge which connected the city with the Triana suburb, and had opened a number of free taps from which the inhabitants of this suburb could draw the new water.

The old Moorish supply was scarcely susceptible of contamination, as the conduit was covered for the greater part of the way, and where it ran over the aqueduct no one but the Municipal guards had ever been allowed to pass; guards, however, were stationed day and night on the springs from which the English Company derived their water, and no one was allowed to approach them without permission.

The cholera raged fearfully in Granada during the months of July, August, and September; it descended the River Genil, which runs through Granada, and attacked the towns of Herera, Ecija, and others in the province of Seville. It broke out also at Cordova and other towns on the Guadalquivir, of which the Genil is an affluent, and it broke out in Palma, Utrera, Puerto Real, Puerto Santa Maria, and Cadiz, forming a circle round Seville, but the city itself escaped almost completely. Towards the end of September nine cases occurred in one quarter of the city, of which seven were fatal, but the disease did not spread; none of the five houses in which these cases occurred were connected on to the water-supply, and it is possible they may have used well or river water, although this is not known. Jerez, which lies about halfway between Seville and Cadiz, and close to the town of Puerto Santa Maria, which was attacked by cholera, escaped also from the disease. This town possesses a very excellent water-supply, brought down some few years ago from a spring in the mountains by a native Company, at a cost of 300,000*l*.

Malaga has a population of 115,882. This city is in even a worse sanitary condition than Seville as regards its drainage, and a great deal worse as regards its cleanliness. In the old portion of the town the streets are narrow, unventilated, and intolerably filthy; the climate in summer is almost tropical.

It is difficult to obtain reliable data as to the cases of cholera in Malaga, as attempts were made to prove that no real cholera existed in Malaga; but there can be no doubt that from June to September the cholera did exist, and it is probable that during the whole of the summer there occurred some 200 or 300 real cases of Asiatic cholera. But the disease never became epidemic, although to all appearances the city offered a most excellent medium for the propagation of the disease, and on all former visitations had suffered very severely. But Malaga during the last few years has been provided with an excellent water-supply drawn from some springs situated at Torremolinos, on the coast to the westward of the city, and piped from thence into the city; and although the precautions adopted were not so complete as those at Seville, yet a more or less successful attempt was made to prevent the use of any other water than that brought from Torremolinos.

We have now examined the case of the few towns in Spain that possess a pure supply of water drawn from springs not liable to any specific contamination, and we have seen that, in all cases where such a supply existed, the cholera, although present in all of them, never made any headway or became truly epidemic, although in every case, except that of Madrid, there was no proper drainage, and the sanitary conditions were in many cases as bad as they could be.

Let us now look on the other side of the picture. We will commence with Granada—population 76,005. As regards its sanitary arrangements this city is on a par with Malaga; about one-tenth of the town is drained, but the sewers are of a very inferior class. The city is supplied with water by canals derived from the Genil and Darro, the two rivers which serve to irrigate the magnificent plain which spreads round it. A small portion is supplied from a spring called La Fuente Grande de Alfacar. The canals are uncovered, and are exposed to all kinds of contamination.

Through the streets the water is conducted in earthenware pipes, after the style of the Moors; many of the pipes are the original ones put down by these people before the conquest of the city by Ferdinand and Isabella. The cholera broke out about the middle of July. It is supposed to have first been brought in by some labourers who had arrived from Murcia, where the cholera was raging. It spread with frightful rapidity, and by the middle of August the official number of cases reported was over 450 per day. It died out, or rather wore itself out, about the middle of September. The total official returns give a total of 6471 cases and 5093 deaths, but in the city itself these returns are said to be much under-estimated; some, indeed, say the numbers should be doubled.

No attempt was made, as was done at Toledo with such excellent results, to suppress the old water-supply, and the epidemic took in a short time such alarming proportions that the local authorities were completely paralysed. It was difficult to carry on the interment of the bodies, and at one time from 400 to 500 corpses were lying piled up in the cemetery, awaiting interment.

The course of the cholera may be followed down the Rivers Darro and Genil, the infected waters carrying death wherever they were used for drinking-purposes.

Murcia—population 91,805—from which the cholera was imported into Granada, suffered heavily also. It was carried into the plains of Murcia by the waters of the River Segura from the baths of Archena, and it was imported into Archena by some invalid soldiers who were sent to the baths from the infected district round Valencia. The plain of Murcia is irrigated by the waters of the Segura, and the disease commenced in this district with the death of a labourer who had drunk the water of one of the irrigation channels. The inhabitants of Murcia and of the plain use principally water from the irrigation canals or from the river; this water is usually stored in large jars similar to those

which held Ali Baba and his forty thieves, and amongst well-to-do people it is customary to keep a year's supply in hand; that is to say, the water is allowed to repose for one year before use in a reservoir or "algibe," constructed on purpose, or in some of these large jars sunk up to their necks in the ground; by this means it becomes perfectly clear, cool, and palatable. The poorer classes are, as a matter of course, not able to take these precautions, and have to drink the water from the canals, or after a few days' repose only.

The epidemic raged principally amongst the little cottages scattered thickly over the plain or garden, as it is called, but the disease never developed itself in Murcia as it did in Granada, and the city itself escaped better than might have been expected. May this not be attributed to the fact that the greater part of the people in the city were drinking water collected in the foregoing year, before the cholera had appeared on the sources of their water-supply? And if this be so, may we not anticipate a fresh outbreak this year, if the choleraic poison or germs are capable of outliving a year's repose and darkness?

In reference to water-supply and cholera, no case is so instructive as that of Valencia. This city is fairly well drained, as drainage goes in Spain, and as regards cleanliness is certainly in a better situation than Malaga or Granada. The water-supply is derived from the River Turia; it is taken from the river near the town of Manises, about three miles and a half above Valencia; it is passed through sand filters situated between Manises and Mislata, and is stored in a covered reservoir, from whence it is conducted by iron pipes, a distance of about one mile and a half into the city.

In one of the interesting letters written by the special correspondent of the *Times* during his tour of inspection of the cholera districts, a very clear description is given of the track taken by the cholera from its starting-point in Alicante, where it had broken out at the latter end of 1884, to Valencia in 1885. During the course of the year 1884 the disease had crossed the frontier of the provinces of Alicante and Valencia, and established itself at Jativa, a somewhat important town, situated on one of the affluents of the Jucar—this and the Turia being the two rivers whose waters are used for the irrigation of the wonderful "Huerta," or Garden of Valencia. During the winter the disease lay dormant, but it broke out in the spring of 1885, and travelled rapidly down the river to Alcira, attacking the various towns situated on the river itself, or on the canals derived from it.

The epidemic was severe at Alcira, but, as the *Times* correspondent suggestively remarked, it ceased so soon as the inhabitants gave up drinking the river-water, and took their supply from a spring situated at a considerable distance from the town. From Alcira it travelled across the network of canals till it reached the river Turia. The *Times* correspondent says:—"It came very near Valencia, and yet never touched the capital till it had worked right round."

At last, in the middle of May, having crossed the water-supply of the city and thoroughly infected the river, it attacked the city right royally, and by the end of June the number of cases had risen to 700 daily, out of a population of 143,861. The disease died out in September, having, according to the official accounts, attacked during the four months 4234 people.

We will now turn to Saragossa. Saragossa, the capital of the ancient kingdom of Aragon, is situated on the right bank of the River Ebro; it contains 84,575 inhabitants, and is an important city. Like most Spanish towns and cities it has no sewers: fœcal matter is collected, as in Seville, in cesspools, which are periodically emptied.

Its principal water-supply is derived from the Canal de Aragon, which in its turn draws its supply from the Ebro, near Tudela. This canal was intended principally for navigation, and is now used for this purpose, and also for irrigation. It passes at a short distance above Saragossa, and the town supply, after being drawn from the canal, is stored in reservoirs, and, after depositing its mud, is then passed through charcoal filters. Some of the inhabitants of the city drank the water from an irrigation canal taken from the River Jalon; some used the waters of the Ebro, which flows close past the old walls of the city.

The disease broke out in Saragossa shortly after the middle of July, and the number of cases during the time the epidemic raged was close upon 10,000. The proportion of deaths was small, thanks to the heroic and energetic conduct of the authorities and the people. Some time before the commencement of the disease in the city a number of small towns on the banks of the Ebro and the Jalon had been attacked by the cholera; there was therefore ample opportunity for the infection of the water-supply. Against such contamination the only protective measure as regards the general supply was the filtration through charcoal; as regards the Jalon water, there was no protection. This source of supply was, however, ultimately stopped by the authorities, who prevented the water reaching the city, with a notable result as regarded the decrease of the epidemic in the quarter served by them.

It would be interesting to follow out still further the line of inquiry I have adopted, but the examination would be too prolix for the present purpose. The cases I have presented are typical ones; they might be increased *ad libitum*, but I think they are sufficient for my purpose. From an examination of them it would appear as though, in the case of cholera, drainage and sewerage is a secondary subject, the primary one being the water-supply. We have seen that the cities of Toledo, Seville, and Malaga, although in bad conditions as regards their sewerage and general sanitary arrangements, yet escaped from any serious attack of cholera, whilst Murcia, Valencia, and Saragossa suffered most severely, although in their case the sanitary arrangements were certainly not worse, but if anything better, than the three former cities. But, in the case of the three first-named cities, each one enjoyed a supply of water drawn from springs situated at a distance from the city, and carefully watched and guarded to prevent any contamination, and the exclusive use of this water was rendered imperative by the authorities.

In the case of Valencia, Saragossa, and Murcia we have a supply drawn from rivers subject to contamination from various sources, against which the only protection was that furnished by the doubtful process of filtration.

There can be no doubt that the cholera attacks in preference those who live under unsanitary conditions, and whose habit of body is by this means prepared to receive the germs of any disease that may be prevalent.

There is no doubt that the virus can be conveyed about from one place to another, like small-pox, typhus, and various other diseases, either by clothes or in the human body, and where it finds a proper medium it will develop itself and extend; but, like these other diseases, it can in these conditions be isolated, fought, and conquered, but without doubt the medium *par excellence* for the spread of cholera-poison is water, and more particularly so when water so infected is used for dietetic purposes.

When it gets possession of the water-supply of a city, no bounds can restrain it; there is but one resource, and that is the cutting off of the water.

We do not yet know in what the choleraic poison consists; it is in all probability a micro-organism of some sort which is capable of very rapid development in water, but it cannot be yet said what is the particular micro-organism which produces cholera. The "comma Bacillus" of Koch has not been accepted by the scientific authorities; on the contrary, very high ones deny

altogether its identity with cholera, and assert that it is to be found in the mouth of every healthy person. Whatever the specific germ may be, it is at least doubtful whether any filtration will intercept it; from the experience obtained at Valencia and Saragossa it appears evident that neither sand nor charcoal will do so.

In a paper read recently at the Institute of Civil Engineers, Dr. Percy Frankland asserts that the London Water Companies do, at the present moment, eliminate 96 per cent. of all the micro-organisms in the Thames water by simple filtration through 3 feet of fine sand. This may be so, but it is equally certain that filtration through sand, even at a very slow speed indeed, will not eliminate the minute particles suspended in waters of a deltaic character, and which gives such waters their peculiar colour. If sand is incapable of intercepting these particles, it may also be incapable of intercepting the specific germs or poison that produce cholera in the human body.

Filtration is, at the best, but a doubtful proceeding for the purification of water. It is impossible to control effectually the speed of the filters; they vary at every moment, and although a mean term may be arrived at by taking the area of the filter-beds and the volume of water filtered in the twenty-four hours, yet this really affords no reliable guide as to the actual speed at which the water has passed the filters. It is probable—nay, almost certain that, out of a given quantity of water, no two gallons have passed at the same speed, and it is possible and probable that one-half of the total volume may have passed the filter at double or treble the speed of the rest.

To insure immunity from contamination, the only real and practical method appears to be that of capturing the water at a pure source and conducting and delivering it in such a way as to render it impossible that any specific germ or poison should have obtained access to it. In the matter of cholera, for instance, with the experience of Valencia and Saragossa before us, one cannot feel any confidence in water which is taken from a river liable to so many sources of contamination as is the Thames, and it is at least doubtful whether any system of filtration would be capable of eliminating cholera-poison from such waters. It is extremely probable that simple filtration through sand will not do it.

The very interesting series of letters published by the *Times* on the subject of cholera in Spain afford much valuable data as to the causes of the disease, or rather as to its mode of propagation. It is unfortunate that the writer seems to have gone out with a preconceived idea that the cause of the propagation of cholera was defective drainage, and consequently to have devoted the greater part of his time to the examination of the sewerage of the various towns he visited, and of their general sanitary arrangements, the water-supply being as a rule relegated to the second place. He appears to be a strong advocate for traps, and not to be aware that the best sanitary authorities of the present day are beginning to doubt very strongly the utility of traps, and to rest their practice rather on the thorough ventilation of sewers, the rapid discharge of their contents, and a complete disconnection between the house drainage and the main sewers.

It is not too late for some scientific investigator to go over the track of the cholera invasion in Spain, to trace the progress of the disease in the towns it visited, and ascertain all the facts connected with their drainage and water-supply, and also, what is not less important, examine the conditions of those towns which so far have enjoyed a practical immunity from the epidemic. As much is to be learned from this negative evidence as from the other.

Pending the discovery by scientific men as to the particular germ or poison that creates cholera, such a practical examination as I suggest would be of immense value to us, by teaching how the propagation of the disease is principally brought about, and what are the best means of preventing it.

GEORGE HIGGIN

NOTES

THE Royal Society *conversazione*, on June 9, was in all respects satisfactory. We can only afford to refer briefly to a few of the exhibits which attracted the interest of the numerous visitors, who were received by Professor and Mrs. Stokes. A room was devoted to telephones connected with the Savoy Theatre, and the company were delighted to hear the Mikado under such novel conditions. The models of the Romano-British village near Rushmore, on the borders of Dorset and Wilts, between Salisbury and Blandford, exhibited by Lieut.-Gen. A. Pitt-Rivers, F.R.S., attracted much attention. The rare earths from samarskite, gadolinite, &c., with illustrations of their phosphorescent spectra, exhibited by Mr. W. Crookes, F.R.S., were magnificent. The pumice, volcanic ash, drawings, diagrams, &c., illustrative of the effects produced by the great eruption of the island of Krakatão, Java, in August 1883, exhibited by the Krakatão Committee of the Royal Society, proved very attractive, as did the fine collection of astronomical photographs exhibited by Mr. Common, Dr. Gill, the Solar Physics Committee, and others. At 9.30 and 10.30 the stellar and solar photographs were demonstrated, and at 10 Mr. Common demonstrated the photographs of nebulæ and comets. The first series included the stellar photographs recently taken by the Brothers Henry at the Paris Observatory. The remaining photographs had reference to solar phenomena, and consisted of two series, one from Meudon, the other from Kensington; the former, contributed by Dr. Janssen, had reference to the minute portion of the solar surface; the latter, to some recent attempts to photograph the spectra of sunspots and prominences. The photographs of planets, comets, and nebulæ, exhibited by Mr. A. A. Common, F.R.S., consisted of (1) series of photographs of Saturn; (2) series of photographs of Jupiter; (3) photograph of Mars; (4) nucleus of the great comet 1882; (5) the Dumb-bell Nebula; (6) the Crab Nebula; (7) the Spiral Nebula; (8) the Great Nebula in Andromeda; (9) series of photographs of the Great Nebula in Orion, with exposures of 1 min. to 80 min. (the above were all taken with the 3-foot reflector at Ealing); (10) recent photographs of Saturn, Jupiter, and the nebulæ in the Pleiades, by the Brothers Henry.

AT the annual meeting of the American Academy on May 25, it was voted to present the Rumford gold and silver medal to Prof. Langley, of the Alleghany Observatory, for his researches on radiant energy.

THE thirteenth annual meeting of Scandinavian naturalists will take place in Christiania between July 7 and 12.

WHILE Mount Etna has again quieted down during the past week, volcanic energy has manifested itself at the Antipodes in an unexpected quarter. Though the North Island of New Zealand is known to be greatly volcanic, and has in Tongariro an active volcano, there has been no destructive eruption within the memory of man. The eruption therefore telegraphed on June 10 was quite unexpected. It occurred in the Tarawera district, on the east side of the Tarawera Lake, lying in a line between the Bay of Plenty and the mouth of the Wanganui River. It is a long way north from Tongariro, and in the midst of the wonderland of Rotomahana's hot springs and many-coloured terraces. The country is stated to be in a disturbed state for many miles around, and it is estimated that a hundred natives and ten Europeans have perished.

A SHOCK of earthquake was felt on Friday night at Sandy Hook and Coney Island, New York, U.S.

AN earthquake was felt at Bougie, Algeria, on June 10; no accidents are recorded. On the same day a heavy thunderstorm raged in the vicinity of Versailles. The lightning struck a tent at Sautenay camp; sixty soldiers were lying under it; many were hurt, but none killed.

ARRANGEMENTS are in progress for the establishment of an aquarium and winter garden in Stockholm.

DURING the present summer a university will be opened at Tomsk, in Siberia, the first of its kind in this part of the Russian Empire. At first it will consist of two faculties—a historical-philological one and a physical-mathematic. It already possesses a library with 50,000 books, a very valuable palæontological collection, presented by Duke Nicolaus of Leuchtenberg.

EARLY last year the East Indian section of the Dutch Royal Institution of Engineers published some prize questions for essays. One of these subjects was the theoretical methods and calculations used when making deductions from observations on earthquakes, together with positive data as to the situation of the point of egress of a given shock. The first prize of 150 guilders and a diploma was awarded to Prof. Milne of Japan. He also received honourable mention for an essay on another question respecting the application of the theoretical principles of seismological science to the art of house-building, especially in the Netherlands Indies, the prize committee at the same time asking for permission to use his observations and suggestions in a work under preparation by one of the members of the committee, who has studied Javanese seismological phenomena and Javanese methods of architecture.

IN a paper in the last number of the *Journal* of the American Oriental Society Dr. Martin, the head of the Foreign College at Pekin, writes on the "Northern Barbarians" of Ancient China, or the tribes which harassed the northern frontier. The ethnology of these and other tribes inhabiting China and the adjacent regions is at present engaging much attention from Oriental scholars, and especially in England that of Prof. de Lacouperie. Dr. Martin confesses that we are still in darkness respecting the ethnology of these northern tribes. Even in regard to the great tribe of Hiongnu, which is conspicuous in history for many centuries about the commencement of the Christian era, it has been much disputed whether they were Turks, Huns, or Mongols. Dr. Martin thinks that these tribes of prehistoric times were probably not inferior to the Chinese in form, feature, or natural intelligence, as their descendants, the Manchus and Mongols, are not inferior in any of these respects. In reply to the question were they originally of one mould, or have the lines of distinction become gradually effaced by the intercourse of ages, he thinks the latter the correct hypothesis. He believes that the primitive Chinese type, that imported by the immigrants who founded the civilisation of China, is no longer to be discerned. In the southern and central regions it has everywhere been modified by combination with the aboriginal inhabitants, leading to provincial characteristics, which the practised eye can easily recognise. It has probably undergone a similar modification in the northern belt, where it met with tribes akin to those of Mongolia, and gradually absorbed them. This process was going on in prehistoric times; history at its earliest dawn shows us the unassimilated fragments of these tribes, and at the same time discloses a vast movement southward all along the line. In the historic period these tribes, organised into great states, established in China a dominion enduring for centuries. They have, Dr. Martin thinks, stamped their impress on the people of this region as thoroughly as the Saxons have theirs on the people of England, or the Vandals theirs in that part of Spain which still bears their name in the form of Andalusia. In their turn the invaders have been subjected, in all ages, to influences under which they exchanged barbarism for such civilisation as they found among the more cultivated race.

THE assertion that from the top of the Eiffel tower communication could be established with Dijon, a place situated 304 kilometres from Paris, is not quite correct. The altitude of this proposed monument being only 300 metres, the radius of the horizon could not exceed 80 kilometres, if we disregard the inequalities of the surface. But the mountains which separate the basin of the Seine and that of the Rhone, which are in Côte d'Or, although not very lofty, may possibly be perceived from an elevation of 300 metres at Paris, under the most favourable circumstances, and they are not very far from Dijon. Practically such a tower could be used for placing Paris in communication with any army occupying these mountains. These facts are sufficiently proved by the success of the great triangulation executed by Col. Perrier between the province of Oran and the Sierra Nevada at a distance of over 200 kilometres. The question is, if it is possible to erect this structure, whether it is worth the money required to build it, and whether the effect will not be to destroy all harmony in the great Exhibition. This question is not settled yet.

WITH reference to Mr. Caldwell's observations in which he found that Monotremes are oviparous with mesoblastic ovum, a correspondent sends us the following quotation from a work by Robert and Thomas Swinburn Carr, entitled "The Literary Pancratium," foot-note on p. 8 (London, 1832).—"But this is New Holland, where it is summer with us when it is winter in Europe, and *vice versâ*; where the barometer rises before bad weather, and falls before good; where the north is the hot wind, and the south the cold; where the humblest house is fitted up with cedar; where the fields are fenced with mahogany, and myrtle-trees are burnt for fire-wood; where the *swans are black* and the eagles white; where the kangaroo, an animal between the squirrel and the deer, has five claws on its fore-paws, and three talons on its hind-legs, like a bird, and yet hops on its tail; where the mole lays eggs, and has a duck's bill; where there is a bird with a broom in its mouth instead of a tongue; where there is a fish one half belonging to the genus *Raja* and the other to that of *Squalus*; where the pears are made of wood, with the stalk at the broader end; and where the cherry grows with the stone on the outside.—Field's *New South Wales*, p. 461."

JUST as improved machinery, adopted in a locality to which the old trade was a stranger, through not being there hampered with old customs and much invested capital, may bring with it the future trade, so an intelligent and rapidly-progressing nation like Japan, by the free choice of the latest improvements in educational organisation from both Europe and America, may even have something to teach. Hence the United States Bureau of Education has lately published a circular containing a statistical survey of the system now adopted there. There is a Minister of Education over all; candidates for school committees are nominated by each locality—either a large city or a province—into which the country is divided; a selection is made from them by the Governor, and the chosen members are paid. There are schools of general education divided into three grades, to pass through all of which occupies eight years. The study of literature gives the choice of either Japanese or Chinese, the former requiring three years, the latter four. English, or if preferred, French or German, is required to be learnt in all middle-class schools, as well as in the highest. At the one University a course of instruction in the department of science is provided in mathematics, physics, chemistry, biology, astronomy, engineering, geology, mining, and metallurgy. There are astronomical and meteorological observatories, botanical gardens, and museums. Courses are provided in medicine, leading up to a special degree after a course of five years, and in

pharmacy after a course of three. Independent of the University are the Military Academy and the Engineering College, the last two out of the six years' course of the latter being spent in practical applications. Technical education is divided into chemical and mechanical There are higher schools in the country for nearly every special purpose; but with all this carefully-proportioned system the titles and objects of thirty societies show how thoroughly the English system of voluntary association is making its way as a method of supply to educational demand.

On presenting to the St. Petersburg Academy of Sciences his new researches into the language of his "Codex Comanicus," published by Count Kunn, Prof. Radloff made a few remarks well worthy of attention (*Bulletin*, vol. xxxi. No. 1). After having carefully catalogued all words appearing in the "Codex," Prof. Radloff has collected, under each separate word, the words akin to it in different Turkish dialets, so as to show their kinship at once. It appears that the Comanic dialect belongs to the great group of Turkish dialects which M. Radloff describes as the Kypchak group; the parent language having been spoken from the ninth to the thirteenth centuries by those Tartars who inhabited the Steppes from the Altai Mountains to the Black Sea. They now comprise the Abakan Tartars, the Barabintsys, the Irtysh and Kazan Tartars, and the Kirghizes. The "Codex" thus offers a sample of the oldest language spoken by the Kypchak stem After having concluded his researches into this dialect, Prof. Radloff will devote his attention to the Uigur languages, for which we have so rich a material; and then he will take up the third group of the Seldjuck languages. Only after such an inquiry, he says, may we hope to attain a thorough knowledge of the whole of the Turkish languages, because all the newer material, and much of the older, belongs to the artificial written languages. The Osman and the Jagatai (or East Turkish) dialects are not representatives of defined groups of dialects, but artificial languages based, the latter on the Uigur language, and the former on the Seljuk, with a mixture of different other dialects. As to the Kazan written language, it is a most varied mixture, in which Osman are mingled with Djagatai forms, while the people are acquainted with neither of them.

We have received the *Proceedings* of the Windsor and Eton Scientific Society for the past year. It contains reports of a few lectures on general scientific subjects. We do not see any evidence of that local scientific work for which these societies are so remarkable, and which is the most beneficial outcome of their activity. Still, the President, who must be a good judge, in his address for the year states that the Society is steadily but surely making its mark as one of the many aids to intellectual improvement which are offered to the people of Windsor and the neighbourhood by the Albert Institute and the various societies associated with it.

Comment was made in this journal on a recent date respecting the enemies of frogs. Mr. W. August Carter, of the Fisheries Section of the Colonial and Indian Exhibition, has made further observations upon the subject, and finds that the tortoise must be added to the list of foes. With a view of substantiating this fact Mr. Carter placed some medium-sized frogs with several tortoises of the same dimensions, when the latter immediately attacked them ferociously, and held them firmly by the legs, notwithstanding their efforts to escape. The tortoises were, however, unable to devour more than a portion of the leg, which they did with much apparent difficulty, the frogs afterwards escaping, but only to be recaptured and similarly treated. Considering the tortoises measured only 1¼ inches in length, they displayed remarkable courage, whilst their agility was certainly greater than that usually displayed by these members of the Chelonian family.

A very large specimen of the Ascension turtle died at the Colonial and Indian Aquarium last week. It was the only one of this species on view, and had been the object of considerable notice on account of its colossal proportions. As a further proof of the tenacity of life amongst turtles, it may be remarked that this particular specimen had existed more than two months without food. At its death 100 eggs were found in it, the retention of which doubtless proved fatal to the turtle.

The additions to the Zoological Society's Gardens during the past week include an Ourang-Outang (*Simia satyrus* ♀) from Borneo, presented by Mr. H. H. Riccard; a White-handed Gibbon (*Hylobates lar*) from the Malay Peninsula, a Binturong (*Arctictis binturong* ♂), a White-whiskered Paradoxure (*Paradoxurus leucomystax*) from Malacca, presented by Mr. Dudley Hervey; a Binturong (*Arctictis binturong*) from Malacca, presented by Capt. Robert Hay; a Common Genet (*Genetta vulgaris*), South European, presented by Mr. J. Church Dixon; a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Miss Grace Balfour; a Green Monkey (*Cercopithecus callitrichus* ♂) from West Africa, presented by Mr. Duncan Armstrong; an Indian Civet (*Viverricula malaccensis* ♂) from India, presented by Capt. Archibald Douglas, R.N.; a Herring Gull (*Larus argentatus*), British, presented by Mr. C. A. Marriott; two Black-billed Tree Ducks (*Dendrocygna arborea*), a Violaceous Night Heron (*Nycticorax violaceus*), a Brazilian Cormorant (*Phalacrocorax brasilianus*), a Fugitive Snake (*Dromicus fugitivus*) from the Bahamas, presented by Mrs. E. Blake; two Mexican Guans (*Penelope purpurascens*) from Mexico, presented by Mr. E. A. Clowes; a Garden's Night Heron (*Nycticorax gardeni*) from St. Kitts, West Indies, presented by Dr. A. Boon, F.R.C.S.; seven Common Vipers (*Vipera berus*), from Hampshire, presented by Mr. Walter Blaker; four Three-toed Sand Skinks (*Seps tridactylus*), South European, presented by Mr. J. C. Warbury; a Puma (*Felis concolor*) from South America, a White-handed Gibbon (*Hylobates lar*) from the Malay Peninsula, ten Adorned Ceratophrys (*Ceratophrys ornata*) from Buenos Ayres, deposited; two Viscachas (*Lagostomus trichodactylus* ♀ ♀), two Crossed Vipers (*Craspedocephalus alternatus*) from Buenos Ayres, a South American Flamingo (*Phœnicopterus ignipalliatus*), a Roseate Spoonbill (*Platalea ajaja*) from South America, a Harnessed Antelope (*Tragelaphus scriptus*), two Balearic Cranes (*Balearica pavonina*) from West Africa, two Lineated Kaleeges (*Euplocamus lineatus* ♂ ♀) from Tenasserim, a Porose Crocodile (*Crocodilus porosus*) from Ceylon, a Bald Ouakari (*Brachyurus calvus* ♂) from Brazil, purchased; a Burrhel Wild Sheep (*Ovis burrhel*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

The Melbourne Observatory.—We have received Mr. Ellery's Report, dated October 6, 1885, which refers to the year ending the previous June 30. The great reflector, after some slight repairs, readjustments, &c., is stated to be in excellent working order. The work done with this instrument has been chiefly confined to a revision of southern nebulæ, already observed by former observers, preliminary to publication. One hundred and seventy-two nebulæ have been re-observed and re-drawn to compare with the plates to be published. Many of these nebulæ have been observed twice, and some three times, and none were completed until they had been observed on a first-class night. The new transit-circle with object-glass of 8 inches aperture, constructed by Messrs. Troughton and Simms, has been in continuous use for all the meridian work of the Observatory since August 22, 1884, and has proved very satisfactory in every respect. The number of right ascension observations obtained with this instrument since its erection was 2287, and the number of declination observations 983, comprising observations of a list of stars selected by Dr. Auwers for reduction of zone and Transit of Venus observations, stars observed with comets, and stars selected from the Melbourne zones. All the individual observations are completely

reduced. The second Melbourne general catalogue, containing the meridian results from 1871 to 1884 inclusive, thus incorporating the whole of the results obtained with the old transit-circle up to the date of its disuse, is in process of formation. An alteration has been made in the photo-heliograph, so as to secure a picture of 8 inches diameter instead of 4 inches, as formerly. There have been several interruptions to the continuity of the sun-photographs during the year, owing to derangement of the instrument and dome, and only 130 pictures were obtained up to June 11, when the instrument was dismantled for repairs. The sixth volume of the results of astronomical observations for the years 1876 to 1880 inclusive, was published in February 1885, and has been distributed. The first part of the observations with the great Melbourne telescope (NATURE, vol. xxxiii. p. 538), from its erection in 1869 to the present date, has also been published during the year to which this Report refers.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *JUNE* 20–26

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on June 20

Sun rises, 3h. 44m.; souths, 12h. 1m. 14·5s.; sets, 20h. 18m.; decl. on meridian, 23° 27′ N.: Sidereal Time at Sunset, 14h. 14m.

Moon (four days after Full) rises, 22h. 7m.*; souths, 2h. 49m.; sets, 7h. 36m.; decl. on meridian, 15° 34′ S.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	4 17	12 45	21 13	24 57 N.
Venus	1 50	9 14	16 38	15 14 N.
Mars	11 28	17 45	0 2*	2 41 N.
Jupiter	11 40	17 56	0 12*	2 19 N.
Saturn	4 40	12 50	21 0	22 37 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich)

June	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
21	B.A.C. 7487	6½	0 36	1 55	89 258
24	24 Piscium	6½	0 43	1 47	87 241

June

21 ... Sun at greatest declination north; longest day in northern latitudes.

Variable Stars

Star	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·2	81 16 N.	June 24,	1 14	*m*
R Virginis	12 32·7	7 37 N.	,, 25,		*m*
W Virginis	13 20·2	2 47 S.	,, 26,	2 20	*M*
δ Libræ	14 54·9	8 4 S.	,, 26,	23 32	*m*
U Coronæ	15 13·6	32 4 N.	,, 20,	20 41	*m*
U Ophiuchi	17 10·8	1 20 N.	,, 21,	2 14	*m*
			and at intervals of	20 8	
X Sagittarii	17 40·4	27 47 S.	June 26,	2 0	*M*
U Sagittarii	18 25·2	19 12 S.	,, 22,	3 0	*M*
β Lyræ	18 45·9	33 14 N.	,, 26,	21 30	*M*
η Aquilæ	19 46·7	0 43 N.	,, 23,	22 0	*m*
T Delphini	20 40·1	15 59 N.	,, 23,		*M*
δ Cephei	22 24·9	57 50 N.	,, 22,	2 30	*m*
R Pegasi	23 0·9	9 56 N.	,, 25,		*M*

M signifies maximum; *m* minimum.

GEOGRAPHICAL NOTES

THE paper on the aborigines of Formosa, by Mr. G. Taylor, in the *China Review*, to which we have already adverted, is continued in the last number (vol. xiv. No. 4), and as it progresses it contains more and more information, especially with regard to the number of different tribes and their various customs, which is wholly new, either in European publications or in those of the Far East. The number last noticed concluded with the Paiwans, the tribe with which the Dutch came in contact in the seventeenth century, during their temporary occupation of part of Formosa, and of which therefore we had the most information. The present instalment deals with several other tribes, including one very peculiar and hitherto unknown people, the Caviangans, who are comparatively few in number, inhabiting lofty mountains, and having many superstitions with regard to hills and the spirits which inhabit them. We have also an account of the Tipuns, the most powerful tribe in southern Formosa, inhabiting the great plain inland from the headland marked Double Peak on the charts of the east coast. These have a tradition that they came from some other country hundreds of years ago, but they appear now to differ little from their neighbours the Paiwans. But there is one very radical distinction, viz., that when a man marries he enters his wife's family, whereas amongst the Paiwans the reverse is the case. Amongst them tattooing is a mark of rank, and is strictly prohibited to the commonalty. Another tribe described is the Amias. The Chinese class these as aborigines, but the true aboriginal tribes look on them as foreigners. They have a curious tradition of their origin, but the aborigines have the more prosaic one of shipwreck, and it appears that the Amias do not consider themselves entitled to equal social rank with the other savages. In appearance and customs they differ much from their neighbours, and worship one Supreme Being, not a multitude of spirits. They believe in an after state, dependent on personal conduct in this life, and they have a sort of purgatory amongst their beliefs. They have a vague notion of lands and peoples where communication is carried on by means of other than oral speech. This, says Mr. Taylor, is the only trace in South Formosa of any original idea of writing. Their explanations of certain natural phenomena, such as thunder and lightning, sunset and sunrise, are curious. Earthquakes they believe to be caused by a pig scratching itself against an iron bar stuck into the earth. This paper leaves on the mind, even more strongly than its predecessor, the impression that in the future Formosa will offer ethnological problems as interesting and complicated as any equal area on the earth's surface. It is clear, too, that all the divisions of the inhabitants of the island hitherto given by writers, whether Chinese or Europeans, are wholly incorrect and unscientific. There are wider differences amongst the tribes, and a far greater number of different tribes, than has ever been supposed. Moreover, it is obvious that in the present state of our knowledge of the tribes, it would be idle to theorise about them. Mr. Taylor, dealing only with a very small section in the south of the island, has described six or seven tribes; amongst these we find some calling themselves aborigines, and looking down as strangers and new-comers on others who have been generally supposed to be aborigines. In view of the wild and inaccessible nature of a large part of the eastern half of Formosa, and of the danger of entering it on account of the chronic state of war which exists between the natives and their Chinese masters, it must be a long time before a clear or trustworthy ethnological account of Formosa can be written. It is quite possible that some of the largest ethnological problems of the Far East may be involved in Formosa; the knot may, perhaps, lie there. Meantime, Mr. Taylor deserves thanks for his careful and interesting collection of new facts which are vital to the discussion of Formosan ethnology.

A REPORT addressed by Col. Fontana, the Governor of Chubut, to the President of the Argentine Republic, gives details of the exploration of Chubut up to the Andes lately made by the Governor. The Expedition, consisting of thirty men, left Raiwon, the chief town of Chubut territory, on October 14, and returned on February 8, having traversed about 1000 leagues in four months. It first followed the tortuous course of the Rio Chubut to its source in the Cordilleras, about the 42nd degree south latitude, the northern limit of Chubut, and then, crossing well-watered and fertile prairies and enormous forests, reached the 46th parallel. It discovered three passages into Chili, and laid down accurately the courses of several rivers heretofore fixed by guess-work. Col. Fontana believes he was the first to quench his thirst in the spring from which the River Senger takes its rise: he has removed the doubts which existed respecting Lakes Colne and Musters, and verified their positions; and he has determined the geographical position of the spots at which the Senger and Chico debouch into the lake. He promises in a short time to have completed maps which will correct many errors concerning the hydrography and orography of this region.

We have received the annual report for 1885 of the Russian Geographical Society, which contains short accounts of the expeditions of M. Prjevalsky to Central Asia, M. Potanin to China, M. Grum-Grzimailo in the sub-Pamir region, MM. Wolter and Trusman; and the usual notices on works for which the medals of the Society were awarded. Geographers surely will be sorry not to find in this report any notice of the work done by the Caucasian and Siberian branches of the Society, which usually so greatly increases the value of the annual report of the Russian Geographical Society.

We are glad to learn from the last Annual Report of the Russian Geographical Society that the Appendix to the *Russian Gazetteer*, by P. P. Semenoff, is in course of preparation. The full edition of the observations at the Polar Stations on Novaya Zemlya and on the Lena; the remarkable collection of maps dealing with the delta of the Amu-daria, Baron Kaulbars; and a geological map of the shores of Lake Baikal, are also in preparation.

At the last meeting of the Paris Geographical Society, Dr. Maurel read a paper on his travels in Cochin China and Cambodia, on a mission from the Minister of Public Instruction. By means of a series of maps representing the Indo-Chinese peninsula in the seventh, eleventh, eighteenth, and nineteenth centuries, he showed the relative importance at different epochs of each of the peoples inhabiting this region. He then gave a general account of the country, its geography, climate, population, &c. A large collection of ethnographical objects which he had with him added much interest to that part of his paper. The young Cambodians at present being educated in Paris were present, clothed in the national costume.

THE DETERMINATION OF THE INDEX OF REFRACTION OF A FLUID BY MEANS OF THE MICROSCOPE

Of the various means adopted hitherto for the determination of the refractive index of a fluid, the most usually adopted has been that of the hollow prism, telescope, and collimator.

This method involves (α) the determination of the angle of the prism; (β) the position of minimum deviation; (γ) the use of monochromatic light, if errors arising from the different dispersive qualities of the substances are to be avoided. These preliminaries render the labour of determining the index a very difficult task, and the observer will scarcely expect to accomplish more than one observation at a sitting.

Cleaning the prism is not the least of the troubles, and when we add to them the fact that many liquids are so opaque that sufficient light can scarcely be passed through them for the observation, it is not surprising that so few have been found to possess the courage necessary for attacking the problem. The writer having had occasion for frequent determination of the index of refraction, has found the use of the microscope far surpasses the usual method in giving results of the greatest delicacy combined with a minimum of cost and of time.

Starting with the well-known fact, that an object viewed through a medium whose refractive index is different from that of air will occupy a different position from its image, or in the language of the text-books, $v = u + \frac{t}{\mu}$, where v determines the position of the geometrical focus of a pencil after direct refraction through a plate whose thickness is t, the writer was led to adopt the following plan.

On an ordinary "slip" as used for mounting preparations for the microscope a delicate mark is made with a writing diamond. A large but very thin "cover-glass" is cut in half, and its pieces cemented to the "slip" on either side of the mark, leaving a space of about one-eighth of an inch; then, resting on these supports, and bridging over the intervening space, is placed a small but very thin "cover-glass," and a drop of the fluid to be examined is run under this.

The fine mark made on the "slip" is now viewed through this with the microscope, using as high a power as possible, for the higher the objective the more delicate will be its focal adjustment; when the object is in focus the position of the "fine adjustment" must be read off. The microscope must then be left, and the slip removed for the examination of any other fluid. The top cover-glass is lifted off, the slip cleaned, the *same* cover-glass replaced, and a drop of a different fluid run under. Replacing now the slip upon the stage, and looking for the mark which was previously in focus, it will be found that an alteration of the fine adjustment is necessary to bring it into focus.

If the medium is of lower refractive index, the objective will have to be lowered, and conversely. Thus a rapid comparison of the relative refractive indexes of two media may easily be made.

But not only can the relative refractive powers of different bodies be thus obtained; the absolute numerical values may with the greatest accuracy be determined. For this it is essential that the fine adjustment screw should have accurate micrometer divisions, and this is usually the case now that immersion objectives are in common use. Two fluids must be selected whose refractive indexes present a wide difference, say oil of cassia and water; focus the mark, first viewed through water, secondly viewed through oil of cassia, and read off the number of divisions the screw has been turned through in the alteration of the focus. The refractive indexes of oil of cassia and water being known from the tables, a numerical value will by the formula be obtained for each division of the screw-head, and thus the absolute numerical index of any medium easily be determined.

By this simple and inexpensive method the writer has obtained from fifteen to twenty absolute indexes in a sitting of an hour's duration.

The importance of obtaining suitable media of high refractive index for mounting objects to be viewed with very high powers cannot be overestimated, for not only is a wider cone of light thus brought to bear upon the object, but its image is advanced, so that a greater working distance is obtained between the front lens of the objective and the cover-glass.

Gordon Thompson

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UNIVERSITY AND EDUCATIONAL INTELLIGENCE

Cambridge.—The twentieth annual report of the Museums and Lecture-Rooms Syndicate, lately issued, recounts continuous progress in many scientific departments. The number of students attending demonstrations in the Cavendish Laboratory reached 100 last winter, and during the year twelve persons have done original work in the Laboratory.

The Plumian Professor (Mr. G. H. Darwin) introduced a new feature last summer by giving a course of lectures in the Long Vacation, and the attendance (thirteen) was encouraging. Few students attend the Plumian Professor's advanced lectures on the orbits and perturbations of planets.

In mechanism Prof. Stuart reports that the temporary museum and lecture-room has become very insufficient.

In chemistry there has been a considerable increase of students in advanced classes and special departments. The new laboratory is now being vigorously advanced. The classes in mineralogy maintain an average of sixteen students. The acquisition of 250 specimens from Mr. Field's collection has added some minerals previously unrepresented, and has improved the collection considerably for students' use.

In geology Prof. Hughes regrets the disadvantages of his present accommodation for teaching and lecturing, and finds the specimens of value are lost to the Museum because of its inadequate means of displaying them. A valuable collection of Cretaceous Cambridge fossils, many of them type-specimens, has been presented by Mr. James Carter of Cambridge.

Mr. Marr, Fellow of St. John's College, is engaged upon the arrangemext of the Foreign and British Cambrian fossils, of which it will be desirable soon to publish a new catalogue. The petrological series has been rearranged, and also the collection of microscope slides. The Upper Jurassic fossils have been largely added to and rearranged. Many interesting additions to the museum are chronicled in the report. It shows how largely the Museum gains from the interest of present and former students at Cambridge.

Prof. Babington has been chiefly occupied with the study of different parts of the Herbarium—especially the magnificent collection of European Rubi—and the identification of plants sent by botanists from a distance. Dr. Vines's students have numbered nearly sixty, and the Botanical Laboratory is inconveniently crowded. The commencement of a botanical museum has been made by Messrs. Potter and Gardiner, with the object

of illustrating the ordinary text books in the hands of students. Many interesting specimens have been given by Sir Joseph Hooker and Mr. Thiselton Dyer, Messrs. Potter, Vines, Gardiner, Hillhouse, and Miss B. K. Taylor of Girton College.

In the Museums of Zoology and Comparative Anatomy some most useful work has been done by the Strickland Curator (Mr. Gadow) in exhibiting the characteristic parts of birds, labelled and illustrated by printed descriptions. A lecture-room for animal morphology is urgently required. The attendances in the Lent Term this year were:—Elementary Biology, 163; Elementary Morphology, 94; Advanced Morphology, 16; total 273. Besides the two lecturers, nine graduates and advanced students took part in demonstrating to the classes. Prof. Macalister reports that the new iron dissecting-room has been very satisfactory, and far more anatomical work has been done than ever before in the University.

The number of students in the elementary physiology classes have averaged 130 each term; while an average of over 30 attended advanced lectures. In pathology Prof. Roy has given systematic lectures on general pathology, a demonstration course on morbid anatomy, a practical pathology course, morbid histology classes, &c., and has found it necessary to engage Mr. Joseph Griffiths, M.B. Edin., as his assistant. Space and other accommodation being deficient hampers the extension of the work.

Vigorous work in natural science will go on during July and August. Mr. Fenton will give a course of chemistry, and the University and Cavendish Laboratories will be open. Mr. Potter will lecture on systematic botany with practical work. Repetition classes in histology and physiology will be given by a demonstrator, and Dr. Hill will conduct a class for practical histology. Prof. Macalister will give demonstrations in osteology; and other lectures will be given regularly in connection with the medical school by Prof. Humphry, Prof. Roy, Dr. Anningson, Dr. Ingle, &c. The courses will begin from July 7 to 12.

Mr. W. H. Caldwell, Fellow of Caius College, and Balfour Student, having returned to Cambridge from Australia with a large supply of valuable material, asks for a room in which to prosecute his original researches. This it is proposed to supply at a cost of 110*l*. on the roof of a portion of the Museum Buildings.

SCIENTIFIC SERIALS

American Journal of Science, May.—The columnar structure in the igneous rocks on Orange Mountain, New Jersey, by Joseph P. Iddings. This paper, read before the Philosophical Society of Washington, June 1885, deals especially with the large vertical columnar formations of O'Rourke's Quarry south of Llewellyn Park, and with the still more interesting case of curving and radiating columns in the Undercliff Quarry near the north gate of the same park. These lava sheets are studied in connection with the general theory of columnar formation, which is attributed to a cracking produced by the shrinkage of the mass upon further cooling after it has consolidated into rock, which still retains a great amount of heat. As the consolidation due to surface-cooling proceeds inward, the resistance to contraction parallel to the surface increases at a greater rate than that normal to it, a point may then be reached where resistance in the first-named direction will exceed that in the second, and the resulting rupture will be *perpendicular* to the cooling surface. The wavy form of some of the columns in Orange Mountain suggests irregularities in the mass which disturbed the uniform advance of the lines of maximum strain, and caused them to deviate from parallelism.—Larval theory of the origin of tissue, by A. Hyatt. This is an abstract of a paper published in the *Proceedings* of the Boston Society of Natural History (1884), in which an attempt is made to trace a phyletic connection between Protozoa and Metazoa, and also to show that the tissue-cells of the latter are similar to asexual larvæ and related by their modes of development to Protozoa, just as larval forms among the Metazoa themselves are related to the ancestral adults of the different groups to which they belong. In the abstract the suggestion is added that Volvox and Eudorina are true intermediate forms entitled to be called Mesozoa or Blastrea. The author's conclusions bear directly on the results already obtained by Semper, Dohrn, and others in tracing the origin of the vertebrates to some worm-like type.—Cretaceous metamorphic rocks of California, by George F. Becker. During a recent investigation of the Californian quicksilver deposits by the United States Geological Survey, the crystalline and serpentinoid metamorphic rocks of the coast-ranges have been subjected to an elaborate examination. Pending a complete report, a summary of the results is given in the present paper, all detailed proofs being deferred until final publication. The field-work was carried out by the author and Mr. H. W. Turner, the chemical analyses by Dr. W. H. Melville; and the microscopical examinations jointly by the author and Mr. Waldemar Lindgren. The question of metamorphism has perhaps never before been studied under more favourable conditions: a solid basis has been obtained for further inquiry, while the results already secured are sufficiently definite to form an important aid for the investigation of metamorphic areas in other geological regions. One important result is the full confirmation of von Rath and Bischof's views regarding the probable conversion of feldspar into serpentine. There seems to be no doubt that the phenomenon occurs in the Californian coast-ranges where the feldspars are corroded externally, cracks widened irregularly and filled with serpentine, and in some cases elongated teeth of serpentine may be seen biting into the clear feldspathic mass. It is impossible to explain these and many similar occurrences, except on the supposition that a reaction between some fluid and the feldspars has yielded serpentine. Quartz also, which is well known to be sometimes converted into talc, is in the same region transformed into serpentine.—Arnold Guyot, by James D. Dana. This is a biographical sketch of the distinguished Swiss naturalist, brought down to the year 1848, when he settled in the United States.—On the determination of fossil dicotyledonous leaves, by Lester F. Ward. The writer offers some critical remarks on the views, and especially on the system of nomenclature, advocated by Dr. A. G. Nathorst of Stockholm in a paper on fossil floras recently published by him in the *Botanisches Centralblatt* (xxvi., 1886).—Pseudomorphs of limonite after pyrite, by Erastus G. Smith. It is shown that the common hydrated oxides of iron generally referred to limonite are undoubtedly alteration products of ferrous oxide, or decomposition-products of other iron-bearing minerals. Their secondary nature is clearly shown in the various occurrences where crystalline form is yet retained, giving clearly-defined pseudomorphs of ferric hydrate after the original mineral. An interesting case is described of such an alteration of pyrite into ferric hydrate, in which the crystalline form of the pyrite is sharply defined.—Influence of motion of the medium on the velocity of light, by Albert A. Michelson and Edward W. Morley. A series of important investigations are described, tending fully to confirm Fizeau's classical experiment of 1851, which proved that the luminiferous ether is entirely unaffected by the motion of the matter which it permeates.—Note on the structure of tempered steel, by C. Barus and V. Strouhal. The results are given of some experiments on the structure of steel, a full report on which will appear in *Bulletin* No. 35 of the U.S. Geological Survey.—Brookite from Magnet Cove, Arkansas, by Samuel L. Penfield. A description is given of a fine crystal of brookite from the collection of Prof. G. J. Brush. It belongs to the variety classed as arkansite by C. A. Shephard.

Bulletin de l'Académie Royale de Belgique, March 6.—Determination of the direction and velocity of the motion of the solar system through space, by M. P. Ubaghs. So far from being a constant quantity, the systematic aberration of the sun and its satellites was already shown to vary with time in right ascension and declination. It was also seen that, by taking into account this fact in studying the motion of the solar system, it might be possible to determine not only the direction and velocity of the motion, but also its extent and even the mean distance of the stars selected for the purpose of comparison. The author here undertakes to apply the principle to certain groups of stars of like magnitude, and although the results are not absolutely uniform, the agreement is sufficiently close to justify the conclusion that theory and practice are, on the whole, in harmony. The direction of the motion has been somewhat accurately determined, but the mean velocity expressed by the fraction 0·109 of the mean radius of the earth's orbit would appear to be far less than that usually attributed by astronomers to the motion of the solar system.—On the study of "arithmetical events," by M. E. Cesàro. In explanation of the expression "arithmetical events," this young and profoundly original mathematician remarks that the systems with which he is here occupied are constituted by numbers taken at hap-hazard. When such a system happens to

enjoy a property capable of being stated in advance, it constitutes for him an event (*un événement*). By means of some extremely difficult and subtle analytical transformations he arrives at a very general and remarkable formula, by means of which he solves with the greatest ease a number of curious arithmetical problems, such as: "What probability is there that in any given division the most approximate quotient will be the quotient by default (*par défaut*)? What probability is there that, if an integer taken at hazard be divided by the sum of two other integers taken at hazard, the quotient by default will be an odd number?"—On the oxidation of hydrochloric acid under the influence of light, by M. Leo Backelandt. This paper deals with the phenomenon observed by the author, that concentrated pure hydrochloric acid exposed to the action of sunlight in a badly-stopped flask after some time turns yellow, and emits an odour of chlorine. The change is shown to be due to a process of oxidation, the atmospheric oxygen consuming the hydrogen of the hydrochloric acid and liberating the chlorine. Under analogous circumstances hydriodic acid acts in the same way, liberating its iodine.—Notes on the rocks of Kantavu Island, Fiji Archipelago, by M. A. Renard. The author deals mainly with the andesites of the port of Kantavu, where they assume a columnar disposition.—Examination of the objections made by M. Hirn against the kinetic theory of the gases, by M. R. Clausius. While admitting the general care and accuracy with which M. Hirn has conducted his extensive experiments, the author argues on theoretical grounds that they are in no way opposed to the now generally accepted kinetic theory.

Rendiconti del Reale Istituto Lombardo, April 15.—On the permanent magnetism of steel at various temperatures, by Dr. G. Poloni. In this paper, which is supplementary to the two memoirs published by the author in 1878 and 1882, several interesting experiments are described with a series of magnets subjected to the action of heat within the limits of 15° and 300° C.—Note on a new acid isomerous with aspartic acid, by Prof. G. Körner. The formula of this acid, which the author proposes to name α-iso-aspartic or α-amido-isosuccinic acid, is—

$$\begin{array}{l} CH_3 \\ | \\ C{-}CO_2H \\ \;\; {-}NH_2 \\ | \\ CO_2H. \end{array}$$

Rivista Scientifico-Industriale, April 15.—A new method of measuring the thermic expansion of solid bodies, by Prof. Filippo Artimini. The author describes an ingenious apparatus which he has constructed for the purpose of determining with sufficient accuracy the increase in the linear dimensions of solids, derived from the internal motion communicated to matter by thermic energy.

April 30-May 15.—On the real atomic heat of simple bodies in the mechanical theory of heat and the formulas relating to it, by Prof. Alessandro Sandrucci. In Hirn's "Mechanical Theory of Heat" the expression *real atomic heat* is applied to the product of the atomic weight a of a simple body by its absolute calorific capacity K, and it is shown that this quantity should be independent of temperature, and equal and constant for all existing simple bodies; but the deductions are established independently of any hypothesis on the nature of heat. Prof. Sandrucci now inquires whether, given a certain hypothesis on the nature of heat, and determining the physical concept of *real atomic heat* in said hypothesis, it might be possible to obtain general and numerical results equal, or very nearly equal, to those already found by Hirn.—On a new saponiferous plant, by Prof. G. Licopoli. To the *Saponaria officinalis*, the *Quillaja Saponaria*, and a few other plants of this class Prof. Licopoli now adds the *Enterolobium Timbouva*, Martius, which is widely diffused throughout South Brazil and Uruguay.

SOCIETIES AND ACADEMIES

London

Royal Society, May 20.—"On the Lifting Power of Electro-Magnets and the Magnetisation of Iron." By Shelford Bidwell, M.A.

If an electro-magnet be excited by a gradually increasing current, a limit is soon reached beyond which the ratio of increase of sustaining power to increase of current becomes rapidly smaller; and it has generally been assumed that this ratio continues to diminish indefinitely, so that an infinite current would not impart to a magnet much greater lifting power than that which it possesses when an approach to "saturation" is first indicated. Joule estimated that the attraction would never be as much as 200 lbs. per square inch of sectional area; and, much later, Rowland assigned 177 lbs. per square inch, or 12,420 grms. per square centimetre as the limit for iron of good quality.

Having reason to doubt these conclusions, the author made some experiments with an iron ring cut into two equal parts, each of which was surrounded by a coil containing nearly 1000 turns of insulated wire. When one-half of the ring was used as an electro-magnet, and the other half as an armature (no current being passed through its coil), the weight supported was with a current of 4·3 amperes 13,100 grms., and with 6·2 amperes 14,200 grms., per square centimetre of surface. The lifting power therefore exceeded that which had been previously considered the greatest possible; nor was there any indication that a limit was being approached. But it was of greater interest to observe the effects produced when *both* portions of the ring were brought under the influence of gradually increasing currents, the conditions then being nearly the same as in Rowland's experiments. It was found that when the magnetic force had reached 50 C.G.S. units, at which point the weight sustained was about 10,000 grms. per square centimetre, the falling off in the rate of increase of the lifting power was well marked. And it continued to diminish until the magnetic force was 250 units and the weight supported 14,000 grms. But from this point *the magnetising current and the weight that could be carried increased in exactly the same proportion*, and continued to do so until the magnetic force had been carried up to 585 units, when the experiment was stopped, the maximum weight supported having been 15,905 grms. per square centimetre, or 229·3 lbs. per square inch. Detailed results are given in the first and second columns of the table. A curve plotted with the magnetic forces as abscissæ, and the weights lifted as ordinates, becomes, when the magnetic force is greater than 240 units, a sensibly straight line inclined to the horizontal axis.

It occurred to the author that these results might be applied to the investigation of the changes of magnetisation which correspond to changes of magnetic force. For if W = the grms. weight supported per square centimetre, H = the magnetic force, and I = the magnetisation, then for the divided ring

$$Wg = 2\pi I^2 + HI;$$

and by giving to W and H the values found to correspond, it becomes possible to find corresponding values of H and I. These are contained in the first and third columns of the table. When H has exceeded about 200, the ratio of I to H no longer continues to diminish, and the curve expressing the relation between them apparently becomes a straight line. Were the experiment carried much further, a tendency to a limit would probably be indicated; but if there is one it must be considerably higher than it is generally believed to be.

If k denote the susceptibility, μ the permeability, and B the magnetic induction, then $I = kH$, $\mu = 1 + 4\pi k$, and $B = \mu H$. Hence the values of k, μ, and B corresponding to different values of H can be found, and are given in the table. The figures in the last two columns are of great interest. Rowland, in order to exhibit the results of his well-known experiments in the form of a curve which (as he believed) would be of finite dimensions, plotted the values of μ as ordinates against those of B as abscissæ. The curve of μ thus obtained, after reaching a maximum for $B = 5000$, fell rapidly and in an almost straight line towards the horizontal axis. Assuming that the line would continue to be straight until it actually met the axis, Rowland concluded that the maximum of magnetic induction was about 17,500 units.

Now the greatest magnetic force used in Rowland's experiments was only 64 C.G.S. units; the imaginary part of his curve, therefore, corresponds to values of H ranging from 64 to infinity. A part of this exceedingly wide gap is filled by the author's experiments, in which H reaches 585; and if the values of μ and B given in the table are plotted, the curve will be found (after a rapid descent) to *bend round* soon after the limit of Rowland's observations, ultimately becoming, when B = 19,800, almost parallel to the axis of B.

The generally-accepted ideas as to the limits of magnetisation and magnetic induction therefore need modification.

TABLE (*Abstract*)

H	W	I	k	μ	B
3·9	2210	587	151·0	1899·1	7390
5·7	3460	735	128·9	1621·3	9240
17·7	7530	1083	61·2	770·2	13630
30·2	9215	1197	39·7	500·0	15100
78	11550	1337	17·1	216·5	16880
208	13810	1452	7·0	88·8	18470
427	15130	1504	3·5	45·3	19330
585	15905	1530	2·6	33·9	19820

May 27.—"On the Relation between the Thickness and the Surface-tension of Liquid Films." By A. W. Reinold, M.A., F.R.S., Professor of Physics in the Royal Naval College, Greenwich, and A. W. Rücker, M.A., F.R.S.

Plateau, Lüdtge, and van der Mensbrugghe have investigated experimentally the relation between the thickness and surface-tension of thin films. None of these observers, however, have used films thin enough to show the black of the first order of Newton's colours. The authors have therefore made a careful comparison of the surface-tension of black films with that of coloured films, the thickness of which was from 10 to 100 times greater. The principle of the method is the same as that utilised in Lüdtge's experiments. The interiors of the films to be compared are connected, and the relation between their surface-tensions is deduced from measurements by which their curvature is determined. In the authors' experiments a cylindrical film was thus balanced against another, which, though sometimes cylindrical and sometimes spherical, was initially of the same curvature as itself. The necessity for this arrangement arises from the fact that the authors' previous observations have shown that a cylindrical film thins to the black of the first order more readily than one of any other form. The fact that small changes in the forms of cylindrical and spherical films, attached to two circular rings, convert them into unduloids or nodoids, renders the mathematical theory somewhat complicated, but other considerations have been made to give way to the necessity of obtaining films which readily yield the black.

Preliminary experiments were instituted to test the observations of Lüdtge and van der Mensbrugghe as to difference of surface-tension between two films, one of which had been formed more recently than the other. These experiments showed that when one of the films was kept thick by supplying liquid to its upper support (flooding), while the other was allowed to thin, a considerable apparent difference of surface-tension was obtained. Before, however, this could be accepted as a trustworthy determination of an actual difference of surface-tension, several possible sources of error had to be considered. Thus, experiment shows (1) that the fact that the thicker film displays the greater surface-tension cannot be attributed to any peculiarity of the apparatus or mode of thickening adopted; (2) that it is not due to the weight of the thicker film; and (3) that only a small part of the observed difference can be ascribed to any slipping of the film over the liquid attachments to the solid supports.

The cause of the phenomenon cannot at present be assigned with certainty. Perhaps many causes are at work. Reasons are given for the conclusion that it is merely an instance of the difficulty which many observers have found in preserving a liquid surface pure.

On the assumption that the rapid change in the surface-tension of a newly-formed film is not due to its thinning, but to a disturbing cause, attempts were then made to eliminate this cause, or reduce it so as to compare films of very different thicknesses.

Two methods of attacking the problem were carried out. In the first the procedure was as follows:—The diameters of two cylindrical films were measured when they were in the same state; an electric current was passed up one of them in order to thicken it; and then, after a sufficient length of time had elapsed for the direct effect due to the disturbance produced by the current to pass off, the diameters were again measured. By this means it was possible to compare two films, one of which was nearly all black, while the other displayed a little black and the colours of the first and second orders. Both films were then allowed to thin, and assuming (in accordance with previous observations of the authors) that that which was already black remained in a constant state, any change of diameter which took place, as the coloured film became black, could be observed.

In a second group of experiments a cylinder was balanced against a sphere. As a spherical film thins more slowly than a cylinder, a comparison between a thick film (sphere) and a black or partially black film (cylinder) could be made without having recourse to an electric current, and greater differences of thickness were obtained than in the earlier observations.

The differences of surface-tension measured in these observations were very small. They never exceeded 1·5 per cent., and the black films were sometimes more and sometimes less curved than the thicker films with which they were compared. There was no evidence of any regular change in the surface-tension as the thickness diminished, and the average difference between the tension of the black and coloured films as deduced from fifteen experiments was only 0·13 per cent.

The general result of the inquiry, therefore, appears to be that *when the black part of a soap-film forms in the normal way, spreading slowly over the surface, no evidence of any change in surface-tension dependent on the thickness of the film is furnished by a direct comparison of the tensions of thin and thick films over a range of thickness extending from* 1350 *to* 12 *millionths of a millimetre.*

This conclusion is based upon a method of experiment by which a change of ½ per cent. in the value of the tension must have been detected, had it existed, and upon fifteen independent comparisons of the tensions of black and coloured films.

The authors next discuss the bearing of their observations upon the question of the magnitude of the so-called "radius of molecular attraction." They point out that if the mere equality in the surface-tensions of thick and thin films is to be considered conclusive, they have accumulated much stronger evidence for the statement that the radius of molecular attraction is less than half the thickness of a black film, *i.e.* $< 6 \times 10^{-6}$ mm. than Plateau produced for the assertion that 59×10^{-6} mm. is a superior limit to its magnitude. They are, however, unwilling to draw this conclusion from their experiments until an explanation is forthcoming in harmony with it, of the apparent discontinuity in the thickness of the film which always (except under very special circumstances) occurs at the edge of the black.

They are themselves inclined to look upon the sharp edge of the black as evidence of a change in surface-tension due to the tenuity of the film, and to regard the result of their experiments as fixing a superior limit (0·5 per cent.) to the difference of the tension of the black and coloured parts.

As no explanation of the discontinuity at the edge of the black has (as far as the authors are aware) ever been put forward, they conclude by a suggestion which, though no doubt of a speculative character, may serve to draw attention to a subject which is they believe of considerable interest.

They show that the main facts to be accounted for, viz. the discontinuity, the uniform thickness of the black, the wide variations in the thickness of the part of the coloured film which is in contact with the black, and the equality in the surface-tensions of the black and coloured films, could be explained if it were supposed that the surface-tension has a critical value when the thickness is somewhat greater than 12×10^{-6} mm.

The possibility of the existence of such a critical value has been pointed out by Maxwell (*Encycl. Brit.*, art. "Capillarity"). It would be explained by the assumption frequently made in discussions on the nature of molecular forces, that as the distance between two molecules diminishes, the mutual force between them is alternatively attractive and repulsive.

June 10.—"On some New Elements in Gadolinite and Samarskite, Detected Spectroscopically." By William Crookes, F.R.S., V.P.C.S.

The recent discovery by my distinguished friend M. de Boisbaudran (*Comptes rendus*, cii., p. 1003, May 3, 1886) of the existence of a new element which he calls Dysprosium, makes it unadvisable on my part, as a fellow investigator in spectroscopic research, to delay any longer the announcement of some of the results I have obtained during the fractionations of the samarskite and gadolinite earths.

I will first take the earths which give absorption-spectra when their solutions are examined by transmitted light. These occur chiefly at the higher end, beginning with didymium and proceeding, through samarium, holmium, &c., to erbium, which is one of the least basic. The earths which give phosphorescent spectra chiefly occur at the lower end, but each group overlaps the other; for instance, yttria occurs above erbia.

One of the highest of the absorption-spectrum earths is didymia. The spectrum of didymium, as generally met with, is well known, and is given in my paper on "Radiant Matter Spectroscopy: Part 2, Samarium" (par. 135).

It has long been suspected that didymium is not a simple body, and in June 1885 Dr. C. Auer announced that by a series of many hundred fractional crystallisations he had succeeded in splitting up didymium into two new elements, one giving leek-green salts and the other rose-red salts. The green body he called Praseodymium and the rose-red Neodymium. I have not found that my method of fractionation gives a decomposition similar to this; probably didymium will be found to split up in more than one direction, according to the method adopted; but by pushing the fractionations at the didymium end of the series to a considerable extent, a change gradually comes over the spectrum. At the lower end the earth gives an absorption-spectrum such as is usually attributed to didymium, but with no trace of some of the bands in the blue end, the one at λ 443 being especially noticeable by its absence. The intermediate earths give the old didymium spectrum, the relative intensities of some of the bands varying according to the position of the earth in the series, the band 443 becoming visible as the higher end is approached. The highest fractions of all give the band 443 one of the most prominent in the spectrum, being accompanied by other fainter bands which are absent in the lowest didymium spectrum.

I now come to a branch of the subject which promises to yield results even more fruitful than those given by the examination of absorption-spectra: I refer to the spectra yielded by some of the earths when phosphoresced *in vacuo*. This method has been so fully explained before the Royal Society, in my papers on "Radiant Matter Spectroscopy," that I need not repeat it.

In my Bakerian Lecture on Yttrium (*Phil. Trans.*, Part 3, 1885) I described the phosphorescent spectrum of this earth, and gave a drawing of it. In the Samarium paper I gave a similar description and drawing of the samarium spectrum, and also described and illustrated some anomalous results obtained when yttria and samaria were mixed together. Under the conditions described in the paper a sharp and brilliant orange line made its appearance, which at that time seemed as if it belonged to the samarium spectrum, and was only developed in greater intensity by the presence of yttria. This explanation, however, did not satisfy me, and I called the line $\left(\lambda\ 609 = \frac{1}{\lambda_2}\ 2693\right)$ "the anomalous line," intending to return to it at the first opportunity. I have since further investigated the occurrence of this line, with more than usual good fortune in the extent and importance of the new facts thereby disclosed.

Systematic fractionation was carried on with the portions of the general series giving the strongest appearance of line 609, and it soon became apparent that the line closely followed samarium. The presence of yttria was not necessary to bring it out, although by deadening the brightness of the other bands it was useful, not seeming to affect the line 609. Several circumstances, however, tended to show that although line 609 accompanied samarium with the utmost pertinacity, it was not so integral a part of its spectrum as the other red, green, and orange lines. For instance, the chemical as well as physical behaviour of these line-forming bodies was different. On closely comparing the spectra of specimens of samaria from different sources, line 609 varied much in intensity, in some cases being strong and in others almost absent. The addition of yttria was found to greatly deaden the red, orange, and green lines of samarium, while yttria had little or no effect on the line 609; again, a little lime entirely suppressed line 609, while it brought out the samarium lines with increased vigour. Finally attempts to separate line 609 from samarium, and those portions of the samarskite earths in which it chiefly concentrated, resulted in sufficient success to show me that, given time enough and an almost inexhaustible supply of material, a separation would not be difficult.

But what was then practically impossible to me, restricted with limited time and means, Nature has succeeded in effecting in the most perfect manner. I had been working on samarskite, and many observations had led me to think that the proportion of band-forming constituents varied slightly in the same earth from different minerals. Amongst others, gadolinite showed indications of such a differentiation, and therefore I continued the work on this mineral. Very few fractionations were necessary to show that the body giving line 609 was not present in the gadolinite earths, no admixtures of yttria and samaria from this source giving a trace of it. It follows, therefore, that the body whose phosphorescent spectrum gives line 609 occurs in samarskite, but not in gadolinite; thus it cannot be due to samarium, yttrium, or a mixture of these two elements; the only other probable alternative is that the source of this line is a new element.

Chemical fractionation is very similar to the formation of a spectrum with a very wide slit and a succession of shallow prisms. The centre portion remains unchanged for a long time, and the only approach to purity at first will be at the two ends, while a considerable series of operations is needed to produce an appreciable change in the centre.

During the later fractionations of the gadolinite earths another set of facts, formerly only suspected, have assumed consistent form. The spectrum bands which hitherto I had thought belonged to yttria soon began to vary in intensity among themselves, and continued fractionating increased the differences first observed. It would exceed the limits of a preliminary note were I to enter into details respecting the chemical and physical reasons which lead me to the definite conclusions I now bring before this Society. More than 2000 fractionations have been performed to settle this single point. I will content myself with stating the results. The earth hitherto called yttria appears to be a highly complex body, capable of being dissociated into several simpler substances, each of which gives a phosphorescent spectrum of great simplicity, consisting for the most part of only one line.

Taking the constituents in order of approximate basicity (the chemical analogue of refrangibility) the lowest earthy constituent gives a violet band (λ 456), which I have reason to believe belongs to ytterbia. Next comes a deep blue band (λ 482); then the strong citron band (λ 574), which has increased in sharpness till it deserves to be called a line; then come a close pair of greenish-blue lines (λ 549 and λ 541, mean 545); then a red band (λ 619), then a deep red band (λ 647), next a yellow band (λ 597), then another green line (λ 564); this (in samarskite yttria) is followed by the orange line (λ 609) of which I have already spoken; and finally, the three samarium bands remain at the highest part of the series. These for the present I do not touch, having my hands fully occupied with the more easily resolvable earths.

In the *Comptes rendus* for April 19, 1886, M. de Boisbaudran announced to the Academy that M. de Marignac, the discoverer of Yα, had selected for it the name Gadolinium. In February last I gave a short note on the earth Yα (*Proc.* Roy. Soc., No. 243, February 1886, and NATURE, vol. xxxiii., p. 525) in which I described its phosphorescent spectrum (agreeing exactly with that given by Yα of M. de Marignac's preparation). Referring to my paper it will be seen that Yα is composed of the following band-forming bodies:—(451), (549), (564), (597), (609), (619), together with a little samarium. Calling the samarium an impurity, it is thus seen that gadolinium is composed of at least four simpler bodies. The pair of green lines (λ 541 and λ 549, mean 545), being the strongest feature in its spectrum, may be taken as characteristic of gadolinium: the other lines are due to other bodies.

A hitherto unrecognised band in the spectrum by absorption or phosphorescence is not of itself definite proof of a new element, but if it is supported by chemical facts such as I have brought forward there is sufficient *primâ facie* evidence that a new element is present. Until, however, the new earths are separated in sufficient purity to enable their atomic weights to be approximately determined, and their chemical and physical properties observed, I think it is more prudent to regard them as elements on probation. I should therefore prefer to designate them provisionally by the mean wave-length of the dominant band. In this I am following the plan adopted by astronomers in naming the minor planets, which are known by a number encircled by a line. If, however, for the sake of easier discussion among chemists a definite name is thought more convenient, I will follow the plan frequently adopted in such cases, and provisionally name these bodies as shown in the following table:—

Position of lines in the spectrum	Mean wave-length of band or line	Provisional name	Probability
Absorption-bands in violet and blue	443	Dα	New.
	451·5	Sα	Dysprosium.
	475	Sβ	New.
Bright lines in—			
Violet	456	Sγ	Ytterbium.
Deep blue . .	482	Gα	New.
Greenish - blue (mean of a close pair .	545	Gβ	Gadolinium.
Green	564	Gγ	New.
Citron . . .	574	Gδ	New.
Yellow . . .	597	Gε	New.
Orange . . .	609	Sδ	New.
Red	619	Gζ	New.
Deep red . .	647	Gη	New.

The initial letters D, S, and G recall the origin of the earths respectively from Didymium, Samarskite, and Gadolinite.

Geological Society, May 26.—Prof. J. W. Judd, F.R.S., President, in the chair.—John Allen Brown was elected a Fellow of the Society.—The following communications were read:—Further proofs of the pre-Cambrian age of certain granitoid, felsitic, and other rocks in North-Western Pembrokeshire, by Henry Hicks, M.D., F.R.S., F.G.S. In this paper the author gave the results obtained by him during a recent visit to North-West Pembrokeshire. He stated that he had further examined some of the sections referred to in his previous papers, as well as others not therein mentioned, and that he had obtained many additional facts confirmatory of the views expressed by him in those papers. The Lower Cambrian conglomerates and grits, he said, contained pebbles of nearly all the rocks in that area which he had claimed as of pre-Cambrian age; and the fragments of the granitoid rocks, the felsitic rocks, the halleflintas, and of the various rocks of the Pebidian series which he had found, showed unmistakably that those rocks had assumed, in all important particulars, their peculiar conditions before the fragments were broken off. Moreover, he stated that there was abundant evidence to show that the very newest of the pre-Cambrian rocks of the area had been greatly crushed, cleaved, and porcellanised before any of the Cambrian sediments were deposited; hence he maintained that there was in the area a most marked unconformity at the base of the Cambrian. At Chanter's Seat, near St. David's, he found that the Lower Cambrian grits and conglomerates were, in parts, almost wholly made up of fragments of characteristic varieties of the granitoid rocks which form the Dimetian ridge near by. The so-called granite of Brawdy, Hayscastle, and Brimaston, he said, there was good evidence to show, was probably of the age of the granitoid rocks of St. David's. The mass of so-called granite near Newgale, he stated, was composed of rhyolities and breccias, undoubtedly of pre-Cambrian age. The Roch Castle and Trefgarn rocks, he stated, could not possibly be intrusive in Cambrian and Silurian strata, but belonged to a series of pre-Cambrian rocks. He referred to the important evidence bearing on the age of these rocks given in a paper communicated to the Society, since his last paper was read, by Messrs. Marr and Roberts. These authors showed that in a quarry near Trefgarn Bridge a Cambrian conglomerate, overlain by Olenus-shales, is to be seen resting on the eroded edges of the Trefgarn series. The author examined this section lately, and obtained from the conglomerate some very large pebbles of the characteristic rocks called hälleflintas, and of the ash-bands, both of which are found *in situ* in the quarry. He therefore maintained that there was the most ample evidence to show that there was a great group of pre-Cambrian rocks exposed in North-West Pembrokeshire, and hence that he had proved conclusively that Dr. Geikie's views in regard to these rocks, as given in his paper and more recently in his text-book, are entirely erroneous.—On some rock-specimens collected by Dr. Hicks in North-Western Pembrokeshire, by Prof. T. G. Bonney, D.Sc., LL.D., F.R.S., F.G.S. The author stated that he had examined microscopically a series of specimens collected by Dr. Hicks, and compared them with those described by Mr. T. Davies, in vol. xl. of the *Quarterly Journal*, and with some in his own collection. He agreed with Mr. Davies's conclusions in all important matters. The Chanter's Seat conglomerate contained many grains of quartz and felspar, curiously like those minerals in the so-called Dimetian, together with numerous small rolled fragments, about a quarter of an inch in diameter, exactly resembling the finer-grained varieties of that rock, besides bits of felsite, similar to some which occur in the St. David's district, quartzite, a quartz-schist, and an argillite. The rocks *in situ* in the Trefgarn quarry were indurated trachytic ashes, together with the curious flinty rock which was the most typical of the so-called hälleflintas. One of the pebbles from the overlying conglomerate perfectly corresponded with the last-named rock; others appeared to be most probably from an altered trachytic ash, differing only varietally from those *in situ*. After prolonged examination of this "hälleflinta" of Trefgarn and the similar rocks from Roch, he was of opinion that while it was possible that some specimens might be altered ashes, most of them were originally rhyolites or obsidians, devitrified, and then silicified by the passage of water which had contained silica in solution. The Trefgarn group obviously could not be intrusive in the Lower Cambrian, and it was extremely improbable that the Roch Castle series was newer than the basement conglomerate of that district. The Brawdy granitoid rock might be a granite, but at any rate it presented considerable resemblance to the "Dimetian." It was therefore evident that the Cambrian conglomerate of St. David's was formed from a very varied series of rocks, some of them much older than it, and that the Dimetian could not be intrusive in it. Moreover, even if the Dimetian should be proved ultimately to be a granite, and the core of a volcano which had emitted the rhyolites, sufficient time must have elapsed after its consolidation and prior to the making of the conglomerate to remove, by denudation, a great mass of overlying rock. Hence, whatever its nature, it was pre-Cambrian.—On the glaciation of South Lancashire, Cheshire, and the Welsh border, by Aubrey Strahan, F.G.S., H.M. Geological Survey. By permission of the Director-General. The author stated that it may be concluded that (1) the striæ on the English and Welsh sides respectively, while showing variations among themselves, by a marked preponderance in one quarter of the compass, indicate a direction of principal glaciation, this direction being on the English side from about N.N.W., and on the Welsh from about E.S.E. (2) The direction of glaciation in both districts agrees very closely with that of the transportation of the drift, but is only locally influenced by the form of the ground. (3) The striæ are by no means universal, but are found almost exclusively in connection with those beds in the drift which contain evidence of the actual presence of ice. The striæ are not such as can have been produced by valley-glaciers; they go across and not down the valleys, nor are there any moraines. The marine origin of the drifts is indicated by their well-marked stratification as a whole, by the alternations of well-washed sands and gravels with the Boulder-clays, and by the occurrence through all the beds of marine shells.

Royal Microscopical Society, May 12.—The Rev. Dr. Dallinger, F.R.S., President, in the chair.—The President referred to the death of Dr. J. Matthews, a member of Council, and a resolution of sympathy and condolence with the family was adopted.—Mr. J. Mayall, jun., exhibited and described a new pattern of the radial microscope by Mr. Swift, in which a rack was added to the arc, and a removable mechanical stage provided by which the object was clipped without any intermediate plate.—Mr. J. D. Hirst's communication was read referring to the report in the *Journal* of the Royal Society of N.S. Wales, attributing to him the view that a highly refractive mounting medium enabled objectives of small aperture to compete in resolution with wide-angled oil-immersion objectives. Mr. Hirst explained that the report was so worded as to convey a totally erroneous impression of what he claimed, which was only that the highly refractive medium would render difficult test diatoms so easy to a good high angled water lens that the superiority of the oil-immersion objective will not be apparent, except under the very deepest eye-pieces.—Mr. C. D. Ahrens's paper, on a new polarising prism, was read; also Prof. Thompson's letter in commendation of it as unrivalled for use as a polariser, having flat ends, wide angle, and absence of distortion or coloured fringes.—Dr Sternberg's paper on *Micrococcus pasteuri* was read, in which he called attention to the characters which distinguish it in a very definite manner from the microbe of fowl-cholera, it differing from the latter, not only in its morphology, but in the fact that it is not fatal to fowls.—Mr. F. H. Evans exhibited some photomicrographs produced by the Woodburytype process from negatives by himself, and transferred

to glass for lantern illustration. They were shown upon a portable screen by Mr. G. Smith of the Sciopticon Co. Mr. Evans claimed that he had been more than ordinarily successful in overcoming the chief difficulty in the matter, that of obtaining such a focus as would properly represent the various planes of even deep objects, and this without loss of natural effect. The objects illustrated comprised Diatoms and Desmids, Foraminifera, Polycystina, star-fishes, sections of Echinus spines, insect preparations, animal parasites, and anatomical and vegetable sections, the remarkable clearness of most of the photographs calling forth frequent favourable comments from the Fellows present.

Entomological Society, June 2.—Mr. R. McLachlan, F.R.S., President, in the chair.—The following gentlemen were elected Fellows, viz. Mr. C. Baron-Clarke, M.A., F.R.S., Mr. Dannatt, Mr. H. Wallis-Kew, Mr. J. P. Mutch, Mr. B. W. Neave, Mr. A. C. F. Morgan, and Mr. W. Warren, M.A.—Mr. Stevens exhibited an example of *Heydenia auromaculata*, from the Shetlands, a species new to Britain.—Dr. Sharp exhibited certain specimens of *Staphylinidæ*, specially prepared and placed in cells of cardboard, sealed up with layers of bleached shellac.—Mr. Billups exhibited *Meteorus luridus*, Ruthe, a species of *Ichneumonidæ* new to Britain.—Mr. W. White exhibited cocoons of *Cerura vinula*, and made some observations as to the mode by which the perfect insect escapes from these solid structures. He thought that formic acid secreted by the insect was a probable factor in the operation. The question of how the parasitic *Ichneumonidæ* and *Diptera* escaped from these cocoons was also raised, and the President, Baron Osten-Sacken, Mr. Waterhouse, and Prof. Meldola, made remarks on the subject.—Mr. Elisha exhibited living larvæ of *Geometra smaragdaria*, from the Essex marshes. He also exhibited the singular pupæ of *A. bennettii*.—Mr. Howard Vaughan exhibited a long series of *Peronea hastiana*, showing the innumerable varieties of the species. He also exhibited, on behalf of Mr. Sidney Webb, of Dover, an interesting series of *Cidaria suffumata*, and read notes on the varieties of this species, communicated by Mr. Webb. Mr. Jenner-Weir, Mr. Waterhouse, Dr. Sharp, Mr. Distant, and Mr. Stainton took part in the discussion which ensued.—Mr. A. G. Butler communicated a paper on new genera and species of *Lepidoptera-Heterocera* from the Australian region, in which 21 new genera, and 103 new species were described.—Dr. Baly communicated a paper on uncharacterised species of *Diabrotica*.

EDINBURGH

Mathematical Society, June 11.—Dr. R. M. Ferguson, President, in the chair.—Mr. Alexander Robertson discussed a problem in combinations.—Mr. John Alison gave a mnemonic for a group of trigonometrical formulæ.—Mr. A. Y. Fraser read a communication from Mr. George A. Gibson on integration by parts and successive reduction.

PARIS

Academy of Sciences, June 7.—M. Jurien de la Gravière, President, in the chair.—Remarks on the works of M. Jean Claude Bouquet, by M. Halphen. To this notice is appended a list of the scientific writings of the illustrious mathematician, who was born at Morteau, Franche-Comté, on September 7, 1819, and died on September 9, 1885.—A new method of determining the refractions of light at all altitudes by means of the known value of one alone (continued), by M. Lœwy. The formulas are here given by which various refractions may be found after one has been determined by the method already explained.—On the part played by Lavoisier in determining the unit of weight in the metrical system, by M. C. Wolf. The imperfect data contained in Delambre's "Base du Système Métrique" are here supplemented from fresh documents tracing the action of Lavoisier in determining the various standards of weight in the metrical system adopted by the French Government at the close of the last century.—Heat of combustion and formation of the sugars, hydrates of carbon, and allied polyatomic atoms, by MM. Berthelot and Vieille. By their new method the authors have at last succeeded in effecting complete combustion of the sugars by free oxygen, thereby correcting the determinations already obtained by Rechenberg with the chlorate of potash for mannite, dulcite, lactose, saccharose, cellulose, and some other substances.—Fresh observations on the ammonia present in the ground, by MM. Berthelot and André. In reply to M. Schlœsing's last note the authors deal with the interesting problems suggested by that chemist's remarks on the laws regulating the interchange of ammonia between the atmosphere and the earth.—On the atomic weight and the spectrum of germanium, by M. Lecocq de Boisbaudran. Under the induction-spark a fine specimen of this element received from M. Winkler yields a beautiful spectrum with remarkably bright blue and violet rays, with atomic weight 72·27. Germanium would therefore appear to lie, not between bismuth and antimony, as at first supposed, but between silicium and tin, like the ekasilicium of Mendelejeff's classification. Winkler had fixed its atomic weight provisionally at 72·75.—Note on the age of the Pikermi, Mount Leberon, and Maragha fauna, by M. Albert Gaudry. The author's observations induce him to refer this geological epoch rather to the Middle than to the Upper Tertiary.—Researches on gelatine, by M. P. Schutzenberger.—Influence of the anæsthetic vapours on the living tissues, by M. R. Dubois. The paper gives a description of the action exercised by the vapours of chloroform, ether, sulphuret of carbon, and alcohol on the protoplasm of the animal and vegetable tissues. The action is regarded not so much as one of coagulation, as of substitution analogous to that obtained by Graham when studying the effects of ether, alcohol, &c., on the mineral colloidal hydrates.—Observations of the comet *c* (1886) made at the Observatory of Lyons with the Brunner six-inch equatorial, by M. Gonnessiat.—Note on the herpolhodie, by M. Hess.—Extension of the general law of solidification to thymol and napthaline, by M. F. M. Raoult. The figures 0·61 and 0·64, here determined for these two substances, approach as nearly as possible to 0·62 given by the author's general law of solidification announced some years ago.—On a visual illusion and the apparent oscillation of the stars, by M. H. de Parville. The phenomenon of the apparent motion of slightly illumined bodies in the midst of darkness is here associated with that of the apparent motion of the stars known to the Germans by the name of *Sternschwanken*.—Action of the hydrogenated acids on vanadic acid, by M. A. Ditte.—Action of the oxide of lead on the hydrochlorate of ammonia, by M. F. Isambert. This reaction, which absorbs heat, is shown to be entirely analogous to a phenomenon of dissociation, and controlled by the ordinary laws of dissociation.—Note on the molybdate of cerium, by M. Alph. Cossa. The form of this substance prepared by different processes by the author and M. Didier, confirms the strict analogy of molecular structure between certain combinations of the metals of cerite and the corresponding combinations of calcium and lead.—Note on a new alloy of aluminium, by M. Bourbouze. This useful alloy, consisting of 10 parts tin and 100 aluminium, is white, and has rather a higher density (2·85) than the pure metal.—On the presence of cholesterine in some new fatty substances of vegetable origin, by MM. Ed. Heckel and Fr. Schlagdenhauffen.—On the presence of cholesterine in the carrot; researches on this direct principle, by M. A. Arnaud. The cholesterine yielded by the carrot contains: carbon, 83·90; hydrogen, 12·20; oxygen, 3·90. It is insoluble in water, but very soluble in boiling alcohol, in the sulphuret of carbon, chloroform, and oils. It thus differs little from animal cholesterine, and is absolutely identical with the substance derived by Hesse from the Calabar bean.—Note on piliganine, the alkaloid of Lycopodiacea from Brazil, by M. Adriau. The piligan plant, which yields this principle, is a lycopod closely allied to the European *L. Selago*, and probably the variety known in Brazil as *L. Saussurus*. The extract is a strong poison, soluble in water, in alcohol, and chloroform.—Researches on the vegetable development of the sugar beetroot, by M. Aimé Girard.—On the crystalline form of the pyrophosphates and hypophosphates of soda, by M. H. Dufet.—On anthophyllite, an orthorhombic amphibole with two prismatic cleavages, *m* (110) ($mm = 125°$ about), and a third, *h'* (010), by M. A. Lacroix.—On the development of the elements of the gray cortical substance of the cerebral circumvolutions, by M. W. Vignal.—On a chronometer with magnetic coupling, by M. A. d'Arsonval. This is an apparatus constructed at the suggestion of M. Brown-Séquard for the purpose of determining the velocity of sensitive impressions transmitted through the spinal marrow in a normal or pathologic state.—Note on sacculine, by M. Y. Delage. The author replies to the objections recently urged by M. Giard against some of the results announced by him on the evolution of sacculine.—On the internal air of insects compared with that of plants, by M. J. Peyrou.—On the stratigraphic structure of the Sierra Nevada and Sierra de Ronda, South Spain, by MM. Ch. Barrois and Alb. Offret.—On the geology of the Central Tunisian region between Kef and Kairwan, by M. G. Rolland.—On the genus *Bornia*, F. Rœmer, one of the most charac-

teristic fossil plants of the Kulm and Upper Devonian formations, by M. B. Renault.

BERLIN

Physiological Society, May 14.—Dr. Kossel reported on experiments instituted by Dr. Kaske in the chemical division of the Physiological Institute, under his superintendence, on the chemical composition of the brain of the embryos of horned cattle. The occurrence of definite chemical substances, for example, elastine, keratine, cerebrine, in altogether special tissues, made it appear desirable to establish whether, during the process of development, the chemical composition or the morphological structure was the primary. Seeing that the brain of the embryo was very lymphatic, the composition and quantity of the lymph, which saturated all tissues of the embryo, were first ascertained and subtracted from the collective mass. The values found in two brains were compared with the results of the chemical investigation of brains carried out a considerable time ago in the laboratory of Prof. Hoppe-Seyler. The investigation referred to had shown that the gray substance of the brain of full-grown cattle differed essentially from the white substance. The gray substance, in the first place, contained but very little cerebrine, probably none at all; the white substance, on the other hand, contained more than 9 per cent. of the dry material. The gray substance was further distinguished from the white by its less amount of cholesterine and its greater quantity of albumen and extractives. In the defect in cerebrine, in the small amount of cholesterine, and in the copious supply of albumen and extractives, the brains of the embryos of horned cattle held exactly the same position as did the gray substance of grown-up brains. It was only in the quantity of lecithine and of salts that the embryonal brains demonstrated any difference from the gray substance. The embryonal brain was, therefore, very essentially distinguished from the white substance—a phenomenon in harmony with the fact that in the embryonal brain medullated nerve-fibres were not met with.—Prof. Christiani handed in his book published last year, "Zur Physiologie des Gehirns," and added some statements in corroboration of the view there set forth regarding the power of seeing on the part of rabbits after complete extirpation of Munck's sphere of vision. Prof. Gudden and Prof. Luciani had also, he said, found animals which after such operations had yet the power of sight. He sharply defined the difference between Prof. Munk and himself by saying that the former maintained an animal *must* be totally blind after excision of the sphere of vision on both sides, whereas his own observations allowed him to take up the position only that an animal after such an operation *might* get totally blind.—Dr. Virchow communicated the results of the investigations carried out by Herr Canfield, in the Anatomical Museum, into the accommodation apparatus of a bird's eye. In order to get at a knowledge of the physiological process of accommodation in the highly developed bird's eye, the anatomical substratum required to be gained. The investigation brought to light, in point of fact, a very long series of differences in the arrangement and development of the different formations of the apparatus situate between cornea, sclera, lens, and iris, among the different species of birds, great horned owl, owl, starling, dove, goose, and others. These differences the speaker illustrated by drawings, but no physiological explanation of them had yet been arrived at.—Dr. Gossels had made experiments regarding the secretion of nitrates through the urine in men and birds (duck and fowl), the nutriment administered having been in every case the same, nitrates being in some instances given, and also in some instances not given. By these experiments it was demonstrated that, in the case of animals secreting uric acid, a large part of the nitrates that had been partaken was again excreted, but that a still larger part disappeared in the body. As to what became of these latter nitrates, the speaker was not disposed to set up any hypothesis.—Prof. Zuntz, referring to the latter point, observed that several years ago it had been noticed in his laboratory that, after partaking of nitrate of ammonia, animals exhaled free nitrogen. A part at least of the nitrates, therefore, in accordance with this observation, was decomposed in the body and reduced to free nitrogen.

STOCKHOLM

Geological Society, March 4.—Hr. C. W. Crongvirt gave an account of the formation of iron ochre in some little lakes in the province of Helsingland. The lakes drew their water from the surrounding iron-containing streams. The yellow ochre seemed to collect on the clay, and the brown on the sand-bottom of the lakes. A factory has been started for its utilisation.—Prof. W. C. Brögger gave an account of the Olenell zone of North America, maintaining that this zone, with its peculiar fossils, which in several instances seemed to be the original types for varieties subsequently appearing, did in America—as well as had been long known to be the case in Scandinavia—occupy a very low place in the geological strata below the true Paradoxide slate.—Hr. F. Tegræus gave an account of his studies of the glacial formations on the Island of Gothland, in the Baltic. He stated that blocks and drifts proved that the glaciers had first moved in a south-westerly and afterward in an easterly direction. He had never found true ridges on the island, but certainly shore-terraces and terminal moraines.—Hr. E. Svedmark exhibited a specimen of argyrodite sent by Prof. Norkenskjöld, which contains the newly discovered element germanium.

BOOKS AND PAMPHLETS RECEIVED

"Habit in Education," by F. A. Caspari (Heath, Boston).—"Observaciones Magnéticas y Meteorológicas del Real Colegio de Belen," Julio-Setiembre, 1885 (Habana).—"Transactions of the Royal Irish Academy," vol. xxviii., "Science," part 22, "Alphabetical Catalogue of Earthquakes Recorded as having occurred in Europe and Adjacent Countries," by J. P. O'Reilly (Academy, Dublin).—"Plane and Spherical Trigonometry," by H. B. Goodwin (Longmans).—"Studies from the Biological Laboratory," vol. iii. No. 6 (Johns Hopkins University).—"First Lessons in Geometry," by B. H. Rau (Addison, Madras).—"History of the Royal College of Surgeons in Ireland," by Sir C. A. Cameron, (Fannin, Dublin).—"Journal of the Royal Microscopical Society," June (Williams and Norgate).—"Bees and Bee-keeping," part 10, by F. R. Cheshire (U. Gill).—"British Cage-Birds," part 10, by R. S. Wallace (U. Gill).—"Fancy Pigeons," part 10, by J. C. Lyell (U. Gill).—"A New Chapter in the Story of Nature," by C. B. Radcliffe (Macmillan).—"An Introduction to General Pathology," by J. B. Sutton (Churchill).—"The Elementary Principles of Electric Lighting," by A. A. C. Swinton (Lockwood).—"Journal of the Society of Telegraph-Engineers and Electricians," No. 61, vol. xv. (Spon).—"British Journal of Petrography," June, by J. J. H. Teall (Watson, Birmingham).—"The Aryan Maori," by E. Tregear (Didsbury, Wellington, N.Z.).—"Catarrh of the Upper Air-Tract," by Dr. S. Sexton (Vail, New York).—"The Terraces of Rotomahana," by F. Cowan (Brett, Auckland).—"A Visit in Verse to Halemaumau," by F. Cowan (Honolulu).—"Australia, a Charcoal Sketch," by F. Cowan (Greensburg, Pa.).—"Labour Differences and their Settlement," by J. D. Weeks (New York).—"Torpedoes for National Defence," by W. H. Jaques (Putnam, New York).—"City of Coventry Free Public Library, Report of Committee, 1885."—"Ichthyol und Resorcin," by Dr. P. G. Unna (Voss, Hamburg).

CONTENTS

THURSDAY, JUNE 24, 1886

MR. MINCHIN'S TREATISE ON STATICS

A Treatise on Statics, with Applications to Physics. By G. M. Minchin, M.A. Vol. II. 3rd ed. (pp. 512 + vi.). Clarendon Press Series. (Clarendon Press, Oxon, 1886.)

THIS new edition of this work has been separated into two volumes. The first volume (351 pp.), dealing with "Equilibrium of Coplanar Forces," aims at the standard of Undergraduate Honours; it is noticeable for the frequent use of graphic methods and for a long discussion on funicular polygons (now forming so important a help in graphic applications to engineering); this was published in 1884. The second and longer volume is a masterpiece of constructive skill in the adaptation of modern methods; it is particularly noticeable for the introduction of the theory of screws and of astatic equilibrium, also for an extensive selection of excellent examples, and for the free use of hyperbolic and elliptic functions in solutions: the reading required is thus considerable; it is, in fact, intended for those who seek Honours. The work is so much improved in this edition that it merits an extended notice. The second volume is divided into only seven chapters, each of which is an essay on its special subject. The numbering of chapters and articles is continuous with Vol. I., whilst the pagination is distinct.

Chapter XIII. (the leading chapter, 64 pp.) deals with Non-Coplanar Forces, and contains the usual propositions (16 pp.) about compounding and resolving forces and couples, about resultants, equilibrium, and central axis: then follow (48 pp.) the theory of screws, cylindroids, complexes, and degrees of freedom; the constructions given for the cylindroid are neat: in one the surface is traced by the blades of a pair of scissors, which open horizontally at a uniform rate, whilst the rivet falls vertically; this gives a vivid idea of the surface.

Chapter XIV. (34 pp.) treats of Astatic Equilibrium, which is defined to be a balance amongst forces of fixed magnitudes and directions at definite points of a body subject to displacement. This is treated by quaternions, the Cartesian method being found cumbrous. It is shown that a system of forces can always be astatically balanced by a set of *three* forces in any given directions (and even by three equal rectangular forces) applied at three points lying in a plane fixed in the body, and also by two forces if these points lie in a line, or by one force if they coincide (the general proof of all this is easy by elementary methods). This subject has some practical application in electrical measurements, for which an astatic magnet-pair is much used, and in seismometry, for which it has been sought to make pendulums astatic for small displacements (see Milne's new work on "Earthquakes," p. 26).

Chapter XV. (91 pp.) treats of Virtual Work. The term "work-coefficient" has here been with great advantage introduced to replace the lengthy and *incorrect* term "generalised component of force." Lagrange's method is treated of at length: its advantage is shown to consist in reducing all problems to the case wherein the displacements are independent, by introducing internal forces to represent the constraints. One disadvantage is its undue length, most marked in simple cases. Another is a decided risk of error in estimating the work of the internal forces; instances of error due to this in Lagrange's researches are shown, *e.g.* the cases (1) of an inextensible surface wherein Lagrange assumes (incorrectly) that $\delta dS = 0$ fully expresses the inextensibility; and (2) of an extensible surface wherein he assumes (incorrectly) the work of internal deformation to be simply proportional to dS; and (3) of an elastic wire wherein Lagrange overlooks the distortion. A brief summary of Jellett's researches on inextensible surfaces is given, and it is shown that such a surface is quite determinate (and therefore not deformable) if any bounding edge of it be fixed, except it be anticlastic or developable, which latter admit of deformation when certain edges only on them are fixed. The surface-tensions of liquid-films are investigated (12 pp.), and the experimental way of producing several such forms is given, and their stability discussed.

Chapter XVI. (45 pp.). On Strings and Springs.—The properties of strings in general, also on rough and smooth surfaces, are discussed, with some cases of the extensible string; next those of plane elastic rods and plane springs; lastly, those of a twisted wire (20 pp.): this last is important in electrometers. The interesting *kinetic* analogies are shown (1) of a plane elastic rod with the simple pendulum, and (2) of a bent and twisted uniform wire with a heavy mass moving about a fixed point, viz. that the differential equations in the analogous problems are similar.

Chapter XVII. (123 pp.), on Attraction, is divided into four sections.

Sec. I. (29 pp.). On Attraction in General.—It is explained that the law of gravitation implies that the attracting particles must be very small compared with their distance. Notice is most usefully drawn to this limitation several times in the sequel, *e.g.* it is shown that the Cartesian expressions *seem* to give indeterminate attraction for very close points; also that for attractions more rapid than $1/r^2$ the attraction on an internal point is really infinite.

Sec. II. (40 pp.). On Potential.—In the definition the usual idea of motion from infinite distance has been dropped, and the definition runs as the work done in bringing a tiny mass from a position of zero attraction, &c. (not from infinity): this is much better. The continuity of the gravity-potential and of its first derivatives, the discontinuity of its second derivatives, the absence of maxima or minima thereof in empty space, and the instability of equilibrium under gravitation to several masses are shown. The application of the method of inversion is given; and, amongst many examples, Thomson's solution of the attraction of a spherical shell whose density $\propto$ (distance)^3.

Sec. III. (13 pp.). On Ellipsoids. After the usual investigation of their attraction, it is shown that the surfaces of prolate and oblate spheroids are not equipotential: various problems interesting in the figure of the earth are given.

Sec. IV. (42 pp.). Spherical Harmonics.—Green's equa-

tion is deduced and its consequences investigated, especially in helping to find potential. Spherical Harmonics occupy the next thirty-two pages. The very convenient and appropriate name "Laplacian" is here assigned to the important "Laplace's coefficients": by analogy the name "Legendrian" might well be applied to Legendre's coefficients; short terms of this kind are useful, and commemorate the inventors. The usual developments are given; the applications to symmetric bodies are interesting, *e.g.* a potential function (*i.e.* one such that $\nabla^2 v = 0$), which is the potential of a symmetric body for all points on its axis, is *the* potential of the body.

Chapter XVIII. (103 pp.), on Small Strains and Stresses, is divided into three sections.

Sec. I. (32 pp.). Small Strains.—This treats of the *small* strains (changes of shape or size) of a body without reference to their causes. It is shown that straight lines, planes, and parallels remain such, whilst spheres become ellipsoids, &c., and there is always one line of no rotation at every point. It is also shown that every strain may be resolved into a pure strain and a rotation, and that the strain proper may be produced by three elongations, or by one elongation and a contraction all round an axis (this is called traction). Torsion is shown to be equivalent to shear, and shear to be equivalent to an extension and contraction, &c.

Sec. II. (22 pp.). Stress.—This treats of internal stress apart from concomitant strain The usual composition and resolution are investigated, the work of an actual strain and the virtual work of virtual strain are found, and the latter is shown to be an exact differential.

Sec. III. (49 pp.). Stress and Strain.—The relations between the moduli of compression (k) and distortion (μ), the contraction-coefficient (η), and Young's modulus (k) are first traced for isotropic bodies, and the strain- and stress-potentials found for the same, and it is shown that every force-system produces definite strain. The work in pure compression and pure twisting is investigated, and it is shown that twisting couples applied at ends of a cylinder produce pure torsion only in a circular cylinder, so that in other cases the plane sections are deformed. The theory of the slightly bent plane beam is investigated as far as the theorem of three moments. In heterotropic bodies it is shown the conservation of energy reduces the number of independent elasticity-coefficients from thirty-six to twenty-one. St.-Venant's reduction to fifteen for cases where the mutual action of two particles is independent of other particles is discussed, and is shown to lead to the value $\eta = \frac{1}{4}$ for the lateral contraction-coefficient of an isotropic body. Maxwell's researches on the propagation of gravitation are reproduced, and are described as showing that gravitation could be produced by a certain stress over a closed surface propagated through an all-pervading medium (ether) transferring strain like a solid, but further research shows that this ether is not quasi-solid.

Chapter XIX. (45 pp.). Electrostatics.—After the usual elementary propositions it is shown that a "line of force" meeting an electrified conductor obliquely is refracted, and that the charge-distribution over an isolated body is determinate: this leads to interesting problems in soap-bubbles. It is shown from Green's equation that a hollow conductor screens its contents from outer electric disturbance; this has a practical application in protection of delicate instruments inside a metallic cage. Lastly, the theory of electric images is discussed, and examples given.

From the full analysis given it will be seen that the work is a most important one: it is, in fact, one of the best treatises of the day.

ALLAN CUNNINGHAM, Major, R.E.

THE CRUISE OF THE "BACCHANTE"

The Cruise of Her Majesty's Ship "Bacchante," 1879-82. Compiled from the Private Journals, Letters, and Note-books of Prince Albert Victor and Prince George of Wales, with Additions by John N. Dalton. Two Vols. (London: Macmillan and Co., 1886.)

To us the chief interest of these two bulky volumes lies in the fact that they are the record of what we may call the technical education of our future King and his brother. It was a right and proper thing for the Prince of Wales to do to see that his sons should become personally acquainted with the leading sections of that great Empire with the conduct of which they will in the future have so much to do. Indeed, in these times, when our colonies are coming so conspicuously to the front, when their affairs are regarded as of Imperial importance, it might be a good thing to insist that our Colonial Secretaries should follow the princes' example, and that no one should be considered qualified for the post of Minister for the Colonies who had not studied their affairs on the spot. Technical education is considered essential nowadays to any one occupying a responsible position in even the humblest of callings; but we fear that statesmanship is still beyond the pale of science.

In the volumes before us Canon Dalton has the lion's share. The princes' contributions have been edited by him from their diaries, note-books, and letters; while he himself contributes long sections in which he brings together much useful information, and discussions on the affairs of the various colonies visited. Of course the writings attributed to the princes are no doubt much indebted to the superintendence of their tutor; at the same time the boyish hands can be traced throughout. The whole work is creditable both to the princes and to Canon Dalton. They certainly worked hard both at their books and at their duties as middies; for in all respects when on board ship they were treated precisely as their mates. They evidently took a genuine interest in their duties on board; took a pride in mastering all the details of navigation and the working of a war-ship like the *Bacchante*: were as eager to pass their examinations as if their future careers depended on the result. Much of their share of the work consists of details as to the day's cruise, their own work as officers, the exercises proper to such a ship, and the incidents of the gun-room. Mixed up with this are the results of their own observations in the countries visited, information gathered during their visits or from books, their experiences when sojourning in the colonies, in Japan and other countries, with occasional reflections suggested by all this. Canon Dalton's contributions are more solid and serious. He enters into

long details on the history and present condition of the colonies, referring at length to the various questions that are uppermost in each, giving as a rule fairly the views of the various parties, though by no means abstaining from showing his own leanings. Certainly the work contains a vast amount of useful statistical, historical, industrial, and commercial information on our colonies, and will be found of service to any one desirous of getting up the subject. Of course it is not to be expected that a work like this will contain much that is novel or of scientific value. In Japan the princes indeed saw a great deal which is not likely to come in the way of the ordinary visitor; while a large portion of the second volume is devoted to Egypt and the Holy Land, which they explored under the guidance of such specialists as Capt. Conder and Sir Charles Wilson, and therefore are able to record much of real and almost unique importance in the geography and antiquities of those interesting countries.

What can Canon Dalton mean by permitting the insertion of the following entry, without note or comment? The apparition is stated to have been seen on the passage from Melbourne to Sydney:—

"July 11.—At 4 a.m. the *Flying Dutchman* crossed our bows. A strange red light as of a phantom ship all aglow, in the midst of which light the masts, spars, and sails of a brig 200 yards distant stood out in strong relief as she came up on the port bow. The look-out man on the forecastle reported her close on the port bow, where also the officer of the watch from the bridge clearly saw her, as did also the quarterdeck midshipman, who was sent forward at once to the forecastle; but on arriving there no vestige nor any sign whatever of any material ship was to be seen either near or right away to the horizon, the night being clear and the sea calm. Thirteen persons altogether saw her, but whether it was *Van Diemen* or the *Flying Dutchman* or who else must remain unknown. [Here are a few German verses on the phantom ship.] The *Tourmaline* and *Cleopatra*, who were sailing on our starboard bow, flashed to ask whether we had seen the strange red light. At 10.44 a.m. the ordinary seaman who had this morning reported the *Flying Dutchman* fell from the foretopmast crosstrees on to the topgallant forecastle and was smashed to atoms. At 4.15 p.m. after quarters we hove to with the headyards aback, and he was buried in the sea. He was a smart royal yardman, and one of the most promising young hands in the ship, and every one feels quite sad at his loss."

Then follows a statement about the admiral having been "struck down," as if it had some connection with the apparition.

The cruise of the princes, which lasted from September 1879 to August 1882, was divided into two well-marked sections. The first, extending to May 1880, included visits to Gibraltar and the Mediterranean, Madeira, the Canaries, West Indies, and Bermudas. After a long visit to Vigo, the second part of the cruise was begun in August 1880. By Ferrol, Madeira, and the Cape Verde Islands the River Plate was made, where some time was spent ashore. After touching at the Falkland Islands, a run was made to the Cape, where several weeks were spent, during which the princes visited several parts of Cape Colony, and showed special interest in the Observatory under Dr. Gill. In the spring of 1881 a long, stormy, and dangerous run was made across the southern Indian Ocean to Cape Leeuwin in West Australia, where the *Bacchante* was compelled to remain some time on account of damage to her rudder. This gave the princes an opportunity of becoming familiar with the peculiar geographical conditions of West Australia, and seeing the actual conditions of colonial life. Then followed long visits to South Australia, Victoria, New South Wales, and Queensland. Some time was spent in the Fiji Islands, of which the princes saw a good deal. Thence a straight run was made for Japan, where the princes had a very busy time indeed in visiting the many sights of that interesting country. Touching at Shanghai, Canton, and the Straits Settlements, the *Bacchante* reached Ceylon, where the princes met Prof. Haeckel, and showed a good deal of interest in him and his work. Then up the Red Sea to Egypt, where and in Palestine three months were spent, months of pretty hard work for the princes. Touching at Greece, Crete, Ceylon, Sicily, and Gibraltar, the *Bacchante* passed out of the Mediterranean and reached home on August 5, 1882, after a cruise during its whole commission of 54,679 miles. There are numerous attractive illustrations in the book, one small map of the world, showing the route, and numerous sectional charts drawn by the princes themselves.

OUR BOOK SHELF

Dogs in Health and Disease, as Typified by the Greyhound. By J. S. Hurndall. Pp. vii. + 81. (London: E. Gould and Son, 1886.)

Dogs: their Management and Treatment in Disease. By G. Ashmont. Pp. v. + 212. (London: Sampson Low, 1885.)

THE first of these two manuals is intended to assist owners of dogs in diagnosing the complaint from which the animal is suffering, and to suggest remedies which may be applied until professional advice can be secured. The book advocates the "homœopathic" system of treatment, and the first twenty-five pages are devoted to a general exposition of this system "in simple unconventional language."

The second book is much fuller in detail, and is evidently suitable as a hand-book for the veterinarian; the mode of treatment differs very considerably from that recommended in Mr. Hurndall's manual, but we must leave to those practically acquainted with the subject the decision as to the relative merits of the two systems. The section relating to hydrophobia is naturally of interest at present; this disease is more fully treated than any other, though the author points out its extreme rarity; nevertheless it is admitted that the danger to persons bitten by a really mad dog is considerable—one-third to four-fifths of the cases, according to whether the wound has or has not been cauterised, are said to be fatal. On the other hand, Mr. Hurndall (p. 52) quotes eighty cases of persons bitten by mad animals, of which not a single one terminated fatally.

The section relating to parasites is somewhat meagre, though the author may be right in saying that the study of these animals more nearly concerns the zoologist than the veterinarian. These principles are perhaps carried a little too far when *Ascaris marginata* is spoken of as a "lumbricoid" which "resembles the common earthworm." The book is carefully written, and free from obvious misprints, but the large amount of matter compressed into a small volume has rendered necessary the use of rather inconveniently small type. F. E. B.

Our Island-Continent: a Naturalist's Holiday in Australia. By Dr. J. E. Taylor, F.L.S. With Map. (London: S.P.C.K., 1886.)

DR. TAYLOR took a trip to Australia for his health, during which he visited South Australia, Victoria, and New South Wales. He has of course scarcely anything new to tell us, though his little book is pleasant reading, and many features of the island-continent are brought out that would only strike a naturalist. Why does Dr. Taylor not state the year of his visit?

The Handy Guide to Norway. By Thomas B. Willson, M.A. (London: Stanford, 1886.)

THIS is a business-like and compact guide which can easily be put into the tourist's pocket, though its price is rather surprising. This is probably due to the fact that it contains many sectional maps, an exceedingly useful feature to the intelligent traveller. The appendix on the Flora and Lepidoptera of Norway, by Dr. R. C. R. Jordan, will prove serviceable to the tourist interested in natural history.

Mountain Ascents in Westmoreland and Cumberland. By John Barrow, F.R.S. (London: Sampson Low and Co., 1886.)

MR. Barrow is an experienced Alpinist, but has a genuine appreciation of the gentler heights of his native land. He has ascended nearly every peak of any consequence in the Lake region, and this volume describes simply and clearly how he did it. The book will be useful as a guide to any who wish to follow Mr. Barrow's example; while the notes on the botany of the district render it of some scientific interest.

An Account of a West Indian Sanatorium, and a Guide to Barbados. By the Rev. J. H. Sutton Moxly. (London: Sampson Low and Co., 1886.)

THIS is a special plea for Barbados as a health resort, and Mr. Moxly adduces many facts in support of his position. The climate is superb, and the great drawback is want of drainage, giving rise to epidemics of typhoid fever. The book is well worth perusal by those in search of a winter-summer. The book will be useful as a guide, though we regret to note the absence of any map.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Fishermen's Foul Water

PERMIT me to call attention to the fact that the small gelatinous masses that annually, about this time, cause the sea-water to become what fishermen call "foul," are now in great abundance on this coast. Their recurrence this year being somewhat later than usual is doubtless owing to the low temperature of May.

On viewing a sample of the water in a glass vessel, the spherical and pyriform masses giving a brownish tinge are readily seen; and a pocket lens makes evident the presence of large specimens of the diatom *Eucampia britannica* that are seen as perfect spirals, some of which have four or five complete turns, and also some filamentous rods. Microscopic examination of the sediment deposited in the course of a few hours enables one to see *Rhizosolenia, Asterionella,* and several other diatoms whose names are not known to me.

Respecting the gelatinous bodies, I may remark that they are studded with granules that appear to be nucleated.

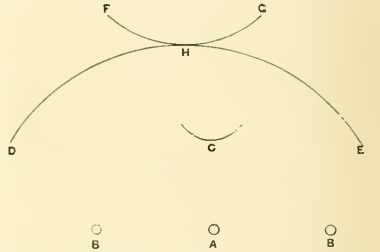
In one of these masses that I have had under observation to-day there has been a gradual segregration of the embedded germs, and this evening these exhibit individual movements which I think indicate the existence of cilia, although with a ¼-inch objective and C eye-piece (the highest power at hand) I cannot distinguish the cilia themselves.

It will be interesting to know how far round our shores at the present time these organisms extend, and I hope, by thus again directing attention to them, that some one more competent than myself will be led to investigate their life-history.

Sheerness W. H. SHRUBSOLE

Solar Halo

AT 6 p.m. this evening I observed an unusual halo and mock suns, a diagram of which is annexed.



A, the sun; B, B, mock suns; C, portion of halo, convex side towards sun; D E, portion of halo, concave side towards sun; F G, portion of halo touching D E at H. Distance—A to B, 22°; A to C, 22°; A to H, 44°.

The sky after having been overcast for the largest portion of the day was at this time fairly clear; very little lower cloud, but a good deal of cirrus. The measurements were observed with a sextant, and were exactly as given, reading the even degree in both cases. T. H. TIZARD

H.M.S. *Triton*, Great Yarmouth, June 21

Ampère's Rule

IN reviewing Mr. L. Cumming's "Electricity Treated Experimentally," "J. T. B." (NATURE, May 27, p. 74) humorously points out how easily students may get confused in trying to make use of "Ampère's rule." There is another rule, published by Pfaundler if I am not mistaken, which is in so far much simpler, as it does not compel the imagination to fancy movements and actions of the human body which it in reality never could perform:—

"Follow the direction of the current in moving the *right* hand along the wire, but holding it so that the stretched fingers are parallel to the wire and the palm is turned towards the north pole of the magnet. The outstretched thumb then gives the direction in which the north pole will be deflected."

Dresden-Striesen, June 12 G. DAEHNE

THE A.O.U. CODE AND CHECK-LIST OF AMERICAN BIRDS

SOME few years since the British Ornithologists' Union appointed a Committee to draw up and publish an authoritative list of the birds of the British Islands. This Committee consisted of the following English ornithologists:—P. L. Sclater (Chairman), Osbert Salvin, F. Du Cane Godman, Henry Seebohm, Howard Saunders, H. E. Dresser, R. Bowdler Sharpe, and H. T. Wharton (Secretary). The Committee held seventy-one meetings, and ultimately a list of British birds was published under the title "A List of British Birds, compiled by a Committee of the British Ornithologists' Union."

Notwithstanding the adverse criticism which has been passed on this little book by a few ornithologists, there can be no doubt that it has supplied a want, and that the high scientific position of many of the members of the Committee invested the publication with a degree of

authority worthy of the object of the work in question. Undoubtedly the "List" will bear improvement, and the second edition will perhaps contain not a few modifications, the constitution of the original Committee having necessarily embraced men holding different opinions on the subject of nomenclature and classification. Still, taking the results of their labours as a whole, it seems to us that the work was as well done as could have been expected, and was a well-meant attempt to introduce uniformity into the nomenclature of British birds.

But since the publication of the B.O.U. "List" a complication has arisen through the action of Mr. Henry Seebohm, whose work on British ornithology we have more than once alluded to in this journal. Mr. Seebohm makes short work of nomenclatural difficulties as far as British birds are concerned. Where any doubt arises respecting the oldest published name of a bird, or in cases when obscurity surrounds the meaning of an ancient description, Mr. Seebohm cuts the Gordian knot by taking the general consensus of opinion amongst ornithologists of repute, *i.e.* "*auctorum plurimorum*," and where he finds an actual majority of them have adopted a certain name, then he considers the latter to be the best known, and uses it accordingly. There is a decided advantage about this method. It simplifies matters amazingly, because in such cases as those of the Chiff-chaff, Garden-warbler, and many other familiar birds, the best-known names, *Phylloscopus rufus* and *Sylvia hortensis*, are restored to them, instead of *P. collybita* and *S. salicaria*, which Prof. Newton had shown them to be with more strictness entitled to. It is probable that had a few more years elapsed before Mr. Seebohm published his work, he would have had to modify his nomenclature even while adopting his *auctorum plurimorum* principle. For this reason. There is no ornithologist in this country more looked up to and respected than Prof. Newton, and with good cause. All his work is of the very best, and when he publishes anything, every ornithologist, whether approving his conclusions or not, knows that they are the result of the most careful and deliberate work, on which no time or labour has been spared, and be it an encyclopædia article or a complete work, the student may be sure that he will find the subject worked up in a way that will leave little for him to add. This is our experience of Prof. Newton's work, and it is work which will stand the test of time, for, as long as ornithology is studied, Prof. Newton's publications will always be found to carry the history of his subject down to the time in which he wrote, forming a starting-point for future study. Such being the position of the editor of the fourth edition of Yarrell's "British Birds," it is not surprising to find that his new edition of this standard work was received by ornithologists with respect, and that his nomenclature was at once adopted by the majority of the younger students in this country. Mr. Seebohm's synonymy of British birds shows this over and over again. Then the influence of Mr. Dresser's "Birds of Europe" cannot be under-rated, for he is content to sit at the feet of Prof. Newton, and unhesitatingly adopts his conclusions. Nor could he have done better, for the original work in the "Birds of Europe" is of the poorest quality, and it is as a successful and indefatigable compiler that Mr. Dresser is recognised, viewed in which light there can be no doubt that his work will be considered the work of reference on European ornithology for a long time to come. Thus we have the nomenclature of Prof. Newton, by its adoption in Mr. Dresser's standard work, doubly enforced on the ornithologists of Great Britain, and so followed by them that, as his synonymy shows, Mr. Seebohm would have found that in a few more years it would have been *auctorum plurimorum*, according to his own principles of nomenclature. Prof. Newton adheres faithfully to the rules of the British Association, and one knows, therefore, the principles by which he is guided. Mr. Seebohm differs on many questions from the Association Code, and we have our own ideas as to certain points of nomenclature, our protest being chiefly against men of the Bonapartian school, who take Linnean specific names and make them generic, adding a new specific name of their own. To our mind, Linnean names should be held sacred by zoologists, even if it involves the adoption of the subsequent genus, so that we must admit *Pica pica* or *Crex crex*. The question has been argued over before, and the usual verdict is against the adoption of this mode of nomenclature; but we have not found it unworkable in practice, and indeed it is often convenient, marking out the typical species of the genus. Sufficient has been said in the foregoing remarks to show that there is considerable variability of opinion even amongst British ornithologists with regard to the adaptability of the Association Code to the requirements of modern science.

But, between the method in vogue in England and that employed by American ornithologists, there has long been great divergence, increasing with years. We have all been looking for some authoritative, *and final*, work on the birds of North America, and it is with pleasure that we have lately received the "A.O.U. Code and Check-List of North American Birds," published by the American Ornithologists' Union. The Committee appointed for the purpose of drawing up this "List" was an excellent one, consisting of Prof. Elliott Coues, Messrs. J. A. Allen, R. Ridgway, W. Brewster, and H. W. Henshaw, while they also received the co-operation of Dr. L. Stejneger, who has made some notable researches into the synonymy of birds during recent years.

The "A.O.U. List of North American Birds" forms a bulky volume of nearly 400 pages. It is divided into five parts: an Introduction (pp. 1-17), in which a history of the subject is given, showing the various efforts of Committees and individual naturalists to systematise the treatment of scientific nomenclature. This is very completely done by the A.O.U. Committee, and with admirable fairness to the labours of their predecessors. Then comes the second section, propounding the "Principles, Canons, and Recommendations" (pp. 18-69), the "Check-List of North American Birds," according to the canons of nomenclature of the American Ornithologists' Union (pp. 71-347),[1] a "Hypothetical List" of recorded North American birds whose status is doubtful (pp. 349-357),[1] and, lastly, a most useful list of the "Fossil Birds of North America" (pp. 359-367).

The introduction calls for no remarks, but it is the "Principles and Canons" of the A.O.U. which will interest the British ornithologist.

In looking through the check-list and comparing the nomenclature used for some of the Palæarctic and Nearctic birds, the ordinary student will be somewhat startled. *Colymbus* is no longer to be retained for the Divers, but for the Grebes, and the Divers become *Urinator*, so that our ordinary Red-throated Diver, familiar as *Colymbus septentrionalis* is now *Urinator lumme*, of the American "List." From this it will be seen that the latter does not simplify existing nomenclature to begin with, and it is the great love of change, which has been so characteristic of recent ornithological work in America, which makes us sceptical as to whether even the authority of the A.O.U. "List" will be sufficient to prevent further modifications in this direction. We remember well how in our younger days we fell into the nomenclatural snare, and how we carried out, as we thought wisely, changes of well-known names in favour of one which had priority of a few years; and in one instance we remember rejecting a well-known name for another because the latter occurred a few pages earlier in the same book. These errors of judgment we have lived to repent, because we find with increasing experience that

[1] By some mistake the pages in the Table of Contents referring to these two sections are not correctly given (p. viii.).

the study of ornithology presents many phenomena of far deeper interest than the mere search after the oldest name, resulting, as it does too often, in the unearthing of some utterly unknown title, to the confusion of the student. The same principles of nomenclature which we tried to follow in earlier days are those of the A.O.U. now, which result in *Urinator lumme* as the name of the Red-throated Diver. And it is not as if there will be any finality about this nomenclature, for we have seen the treatment of too many monographs to make us believe this. When an ornithologist takes up a group of birds and monographs it, he spends months or even years of study on this particular group, obtains a grasp of his subject, and does his level best to give finality to his work. Does he succeed? Seldom, if ever. We hold it as an absolute canon that the nomenclature of monographs should be followed, unless a definite reason is given why a name should be altered. But, instead of this being done, we find, over and over again, that the author of a small paper or of a faunal list will, by altering generic names and so re-shuffling the specific names, give a totally different aspect to birds which have only just before been carefully monographed with a hope of finality in their nomenclature. So will it probably be with the A.O.U. "List," when some ornithologist in America will rise up and (as we expect to see before long) declare the trinomial system unworkable or the nomenclature of the "List" too complicated, and will re-shuffle the names, and attain temporary renown.

We think, however, that, now that the two leading Ornithological Societies of England and America have spoken with authority on the subject of the nomenclature of the birds of their respective countries, the British Ornithologists' Union should endeavour, if possible, to confer with the sister Society in America, and see if a common ground of agreement cannot be arrived at. If these two bodies came to a settlement, the whole matter could be laid before an Ornithological Congress, and there would be some hope of unanimity for the future. The points of divergence in practice between English and American ornithologists are less than might be supposed. The two principal ones are the adoption by the A.O.U. of the 10th edition of Linnæus's "Systema Naturæ" instead of the 12th edition, and the employment of trinomial nomenclature. So many English ornithologists are now using the latter mode that there ought to be no difficulty in conceding the latter point if any ornithologist like the method. Formulated as it is in the A.O.U. "Code," there is no difficulty in understanding what is meant by the trinomial titles, and the American Committee have given a clear definition of their object in Canon XI., though the difficulties which have been pointed out on this side of the water are still not disposed of. "In a word, intergradation is the touchstone of trinomialism. It is also obvious that, the larger the series of specimens handled, the more likely is intergradation between forms supposed to be distinct to be established, if it exists." So says the canon above quoted; but, we would ask, if two forms absolutely intergrade, why are they not of the same species? and why will not a binomial title be sufficient? and again, what name is to be given to the specimens collected at the point of contact? Or again, if a larger series of specimens proves that two species do not intergrade, as they were at first supposed to do, then they will each once more bear a separate specific name. Further, are trinomials to be used for insular forms, as is done by Mr. Allen for *Loxigilla noctis sclateri* from Santa Lucia, as there is no chance of intergradation between it and *L. noctis* from the neighbouring islands? Trinomial nomenclature has, however, taken such a place in American ornithology, and is adopted by so many naturalists in the Old World, that the principle must be conceded to all who like to avail themselves of it. The question with regard to the tenth edition of Linnæus's "Systema" might also be got over, but the A.O.U. will have greater difficulty in convincing European naturalists that it is advantageous to the progress of ornithological science to alter established nomenclature by introducing *Chelidon* as the generic name for the Chimney-swallow instead of the feather-legged Martins, which are to be henceforth *Hirundo*. This radical change is to be adopted in homage to Forster's "List of British Birds," a mere list of names without a character for a single genus. Although similar lists have sometimes been accepted for specific names, their recognition in the case of genera is rare, although in many instances long-established usage has rendered some of them familiar.

The few objections which we have made above must not be supposed to lessen our respect for the general tenour of the work now issued by our American *confrères*, whose labours deserve our most careful consideration, while it cannot be doubted that the publication of this "Code and Check-List" will have great influence on the future of zoological nomenclature.

R. BOWDLER SHARPE

PROFESSOR NEWCOMB'S DETERMINATION OF THE VELOCITY OF LIGHT[1]

THE method selected for the important experiments described in the present memoir,[2] is that known as Foucault's. The idea fundamental to it is that of the determination of the interval occupied by light in flashing from a revolving to a fixed mirror and back, by the amount of deviation produced in its return path through the change meantime effected in the position of the revolving mirror. The angle of deviation of the ray is double the angle of displacement of the reflector; to this angle corresponds (since the mirror rotates at a known rate) a definite fraction of a second, which is the time of luminous transmission across twice the measured distance between the mirrors.

But this theoretically simple means of ascertaining the velocity of light is complicated, in practice, with innumerable difficulties. A choice demanding the utmost nicety of judgment must be made between various conflicting conditions; sacrifice in one direction is the price of advantage in another; a balance has to be struck, giving the largest sum-total of facilities, with the fewest and least intractable drawbacks. The plan finally decided upon by Prof. Newcomb was the result of much anxious deliberation: we hope to render it, in its main outlines, intelligible to our readers.

A fundamental condition of the problem is to get an image of the light-source absolutely coincident with the light-source itself, *so long as the movable mirror is at rest*. And this, whatever be the position the mirror is at rest in, provided only that it be such as to permit the rays sent out by it to return, after due triple reflection, to the eye. This requisite is secured by locating the centre of curvature of the distant concave mirror in the axis of the revolving plane one. All rays emitted from this point towards the former will return along the same paths; differences of direction due to differing positions of the movable mirror will be eliminated by the return reflection; and there ensues a "stationary image" of the light-source, occupying, when visible at all, an invariable situation.

So far, all the operators by Foucault's method have been unanimous; but in the placing of the lens indispensable for the management and concentration of the light employed, a material distinction obtained between the

[1] "Measures of the Velocity of Light made under direction of the Secretary of the Navy during the years 1880–82," by Simon Newcomb, Professor, U.S. Navy. Astronomical Papers prepared for the use of the American Ephemeris and Nautical Almanac, vol. ii. parts iii. and iv. (Washington: Bureau of Navigation, 1885.)

[2] For the historical notice serving as an introduction to it, see NATURE, May 13, p. 29.

plan of experiment chosen by Prof. Newcomb, and that pursued by Prof. Michelson in his similar investigation at the Naval Academy in 1879 (see NATURE, vol. xxi. pp. 94, 120). Fig. 1 represents in principle the arrangement adopted by the former, which was also that used by Foucault. In it the lens, L, is placed between the light-source, S, and the revolving mirror, A. Fig. 2 shows the disposition preferred by Michelson, in which the lens is interposed between the revolving and fixed mirrors. In both equally, S and M are, and for the purpose in view necessarily must be, in conjugate foci of the lens.

A disadvantage of the first form is that the measurement of any considerable deviations will be attended by uncertainties caused by the oblique passage through the lens of the return beams. It was, however, obviated in the experiments under consideration, by the use of *two* lenses—one for the outgoing, the other for the incoming rays. The second method (Michelson's) promises increased brilliancy of the image; which may, nevertheless, be regarded as outweighed by atmospheric and other impediments to its distinctness, as well as by the illumination of the field of view produced by the passage through it of some part of the lens with every revolution of the mirror. The method exemplified in Fig. 1 was then chosen by Prof. Newcomb as affording more or less calculable conditions; while No. 2 involved all the uncertainties of definition habitually besetting astronomical observations.

Fig. 1.

Fig. 2.

Let us now endeavour to realise the nature of the experimenter's immediate task. The precise measurement of an angle actually constitutes it. From the mirror A, so long as it remains at rest, an image is reflected in a certain direction; but no sooner is A set rapidly rotating, than the same image is reflected in a slightly different direction. The amount of this difference—in other words, the angle of deviation—is the object to be ascertained.

Obviously, the first desideratum is to render the inevitable error of measurement comparatively small, by making the quantity to be measured large. Two roads are open towards this end. A high velocity can be given to the mirror A; or a great distance can be interposed between A and M. By the first means, the angle rotated through in a given time will be augmented; by the second, the time available for the displacement of the reflector will be prolonged by the lengthening of the journey imposed upon the rays to be reflected. The difficulties hampering increased speed are purely mechanical, though none the less formidable; those in the way of a lengthened path are optical.

The preservation of light enough to keep the image bright and distinct is of paramount necessity for the avoidance of ruinous uncertainties in its measurement. Now, in Foucault's experiments, the object affording the image was the line of a reticule. It was dark upon a bright ground; a platinum-wire relieved against a sheaf of sunbeams. But no perfectly defined image of such an object could be formed at any considerable distance; and we find, accordingly, that the utmost length by which he ventured to separate his mirrors was twenty metres. His entire apparatus was, in fact, contained in a single room. Hence, notwithstanding a speed given to his mirror of from 600 to 800 revolutions per second, the actual linear deflection of the return ray amounted to no more than seven-tenths of a millimetre. Chiefly by employing as his light-source an illuminated slit, the lucent image of which on a dark ground bore the enormous loss of light ensuing from the transportation of the fixed mirror to a distance of close upon 2000 metres, Michelson was enabled to augment this deflection some two-bundred-fold. The resulting velocity for light of 299,910 kilometres per second was proportionately trustworthy, the error of the angular measurement upon which it immediately depended being estimated to be one hundred times less than in Foucault's determination. Prof. Newcomb's improvements carried him still further towards absolute accuracy.

The details of construction of his "phototachometer" were decided on in the summer of 1879, and the instrument was completed by the Messrs. Clark in May 1880. It consisted essentially of four parts—a sending and a receiving telescope, a revolving and a fixed mirror. Sunlight, thrown from a heliostat through an adjustable vertical slit at the eye-end of the sender, passed down the tube, which was bent at right-angles to get it out of the way of the observing telescope, and after reflection by a plane mirror at the elbow, passed out through the objective towards the revolving mirror. This was formed by a

rectangular prism of polished steel, 85 millimetres in height, and with a cross-section of 37·5 square millimetres. The vertical faces constituting the reflecting area were nickel-plated, and proved of a remarkably durable though not very high polish. Motion in opposite directions at will was communicated by two air-turbines, acting one at the top, the other at the bottom of the mirror, and serving, by a simple contrivance, each for the regulation of the contrary velocity imparted by the other. A wheel-work arrangement, by which an electric current was broken once for every twenty-eight revolutions of the mirror, gave the means of obtaining a chronographic record of its rate of going. Two fixed mirrors, mounted side by side on cast-iron stands, were employed to return the light sent to them by the revolving mirror. Each was about 40 centimetres in diameter, and had a radius of curvature of some 3000 metres. The object-glass of the receiving telescope was (in the first instance) placed immediately under that of the sender, the former thus directly facing the lower, the latter the upper section of the movable mirror. The two *tubes*, however, owing to the "broken" form given, as already mentioned, to that of the sender, made with each other an angle of 90°. Horizontal movement round a vertical axis coincident with that of the rotating mirror, was possessed by the observing telescope, to which was attached a pair of microscopes for reading off the divisions on a horizontal divided arc fixed to a stiff frame at its further end. The amount of this horizontal motion of the telescope measured the deviation of the thrice-reflected sunbeam, and, by an immediate deduction, its velocity.

The site chosen for the erection of the apparatus was Fort Myer, on the south side of the Potomac, overlooking the city of Washington. The stationary mirrors, to and from which the carefully guarded rays performed their trips, were placed, to begin with, in the grounds of the Naval Observatory, at a distance of 2551 metres from Fort Myer; but were in 1881 removed to a point at the base of the Washington Monument, at a distance increased to 3721 metres. Some tentative experiments were undertaken on June 22, 1880; after a few days' trial, however, it was found that the wheel-work for counting the revolutions of the mirror was destroyed by the rapidity of the impressed movements. New wheels wore out almost before a set of readings could be obtained with them; until at length the Messrs. Clark, finding that no metal would stand the inflicted wear and tear, substituted *raw hide* as the material for the first wheel, a device which proved wholly successful. With the instrument thus modified work was begun on August 9, and continued without interruption until September 20. The transportation of the fixed mirrors to the Monument station in the spring of 1881 postponed the commencement of operations to August 8; and their effective prosecution was then impeded by the discovery of a source of systematic error in a "torsional vibration" of the rotating mirror. That is to say, the steel prism employed to reflect the light, no longer, when its speed attained a certain point, revolved as an absolutely rigid whole, but *tended towards* the possession of different velocities in its different parts. Hence a slight twisting of its mass producing vibrations round the axis of rotation, the effect of which was visible in the breaking up of the image of the slit into four separate images, one due to each of the faces of the prism. The persistence of this baffling symptom compelled a modification of the instrument, by which the sending and receiving telescopes could be respectively depressed and raised so as to alternate their positions, and the portions of the mirror they were directed towards. The mean of any two complete sets of observations made with the telescopes thus interchanged would be free, as Prof. Newcomb shows, from the effects of any probable form of torsional vibration.

No such effects, however, were apparent in the observations of 1882. This last series extended from July 24 to September 5, and were so nearly free from accidental differences that the probable error of a complete determination was scarcely more, under good conditions, than the ten-thousandth part of the whole. Upon these, accordingly, the chief reliance was placed in the final discussion of results.

The announcement that Messrs. Forbes and Young had detected a difference of 2 per cent. in the rates of transmission of red and blue light prompted, at Fort Myer, a most careful watch for traces of colour in the reflected image of the slit. But although, from a discrepancy of even one-twentieth that amount, a spectrum 15″ in breadth must have ensued, the iridescent edges which would infallibly have betrayed its presence were not seen.

An important novelty in Prof. Newcomb's method was his use of opposite rotations and their accompanying opposite deviations. In his instrument the mirror, as already stated, could be made to revolve at pleasure, either from right to left or from left to right. Instead, then, of measuring, as had always previously been done, the deflection produced in the return ray by the change from rest to an ascertained rate of rotation, the object of his determinations was the total deflection due to extremes

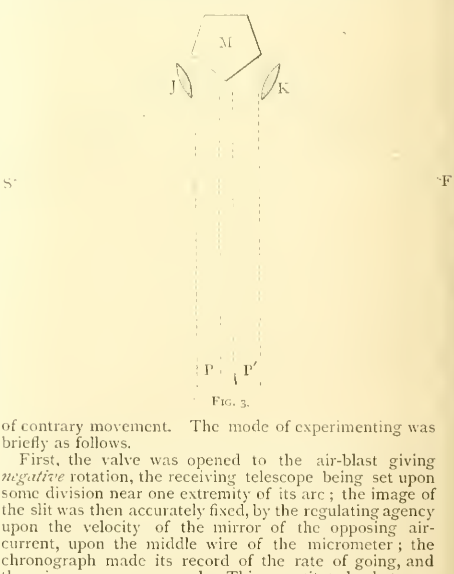


Fig. 3.

of contrary movement. The mode of experimenting was briefly as follows.

First, the valve was opened to the air-blast giving *negative* rotation, the receiving telescope being set upon some division near one extremity of its arc; the image of the slit was then accurately fixed, by the regulating agency upon the velocity of the mirror of the opposing air-current, upon the middle wire of the micrometer; the chronograph made its record of the rate of going, and the microscopes were read. This constituted what was called a "run," and occupied two minutes or upwards. The telescope was next unclamped, and directed near the opposite end of the divided arc. *Positive* motion was given by opening the other valve, and the process of fixing the image and reading off repeated. A comparison of the two sets sufficed to determine the time spent by the light in passing to and from the mirrors on the other shore of the Potomac.

This method of contrary deviations is most strongly recommended by Prof. Newcomb to future investigators. It combines the two advantages of doubling the angle to be measured, and of abolishing possible errors in the determination of the zero-point. In the present series, velocities, alternately in opposite directions, rarely ex-

ceeding 230 revolutions per second,[1] gave a total change of direction of nearly 8°. And this largeness of the measured angle materially contributed to enhance the accuracy of the results. Highly effective, also, for the same end were the elaborate precautions for darkening the telescopic field of view, and thus rendering the image of the illuminated slit more distinct. As their upshot, daylight was reduced to about one-thousandth its normal intensity. What was left only just sufficed to show the spider-lines without artificial light. The necessity for such precautions may be estimated from our author's statement that a concave mirror, of which the diameter should be one decimetre for each kilometre of distance, would receive only 1/60,000 part of the light reflected from the revolving mirror; while of that 60,000th part only a small fraction could be practically turned to account, owing to the many sources of loss in reflection and transmission. Since, however, *two* fixed mirrors, each four decimetres across, placed at a distance of less than four kilometres, were employed in the operations at Fort Myer, the proportion of light there returned was rather more than double the above estimate. Prof. Newcomb appears to have been, on the whole, eminently successful in his optical arrangements. The imperfect definition which was the besetting difficulty of Michelson's experiments gave him little trouble.

The recent American determinations of the velocity of light, justly considered as of far superior precision to any others yet executed, give the following results:—

Michelson, at Naval Academy, in 1879	299,910 km.
Michelson, at Cleveland, 1882	299,853 „
Newcomb, at Washington, 1882, using only results supposed to be nearly free from constant errors	299,860 „
Newcomb, including all determinations	299,810 „

To these are added for comparison:—

Foucault, at Paris, in 1862	298,000 „
Cornu, at Paris, in 1874	298,500 „
Cornu, at Paris, in 1878	300,400 „
The same, discussed by Listing	299,990 „
Young and Forbes, 1880-81	301,382 „

Prof. Newcomb's finally-concluded result is that light travels *in vacuo* at the rate of $299,860 \pm 30$ kilometres per second. And the probable error of thirty kilometres, small as it is, has been liberally estimated. A determination so satisfactory of this important element goes far towards solving the problem of the sun's distance. Combined with Nyrén's constant of aberration, 20″·492, it gives, for the solar parallax, the value of 8″·794. The corresponding distance of 149·61 million kilometres, or 92,965,020 miles, agrees quite closely with Dr. Gill's result from the opposition of Mars in 1877, and exceeds by only 165,020 miles the mean deduced by Mr. D. P. Todd from earlier determinations of light-velocity. No information as to the dimensions of the solar system which we are ever likely to get from a transit of Venus can approach in reliability the present conclusion.

Prof. Newcomb is so far from believing that the *ne plus ultra* of accuracy has been reached in his own remarkable experiments, that he appends to the detailed description of their method some valuable suggestions for its improvement. He had hoped, indeed, he tells us, to reach a concluded value exact to between five and ten kilometres, which, after repeated verification, might be available as a test of the invariability of standards of length. The further prosecution of the inquiry, however, he now leaves to any physicist who may be invited to the task by the promise of his advice and co-operation.

Fundamentally, he holds that the system pursued at Fort Myer in 1880-82 is preferable to any other yet tried. No known expedient for ascertaining the rate of transmission of light is comparable to that of its deflection, after a measured journey, by a moving mirror. The apparatus by which this plan was realised admits, however, in his opinion, of some amelioration in detail. The disadvantageous necessity, for instance, of appropriating a separate section of the reflecting surface to the outward- and homeward-bound rays could be removed by the substitution of a pentagonal for a quadrangular prism, as shown in Fig. 3, where M is a section of the revolving mirror, J the object-glass of the sender, receiving light from the slit S, and throwing it in the direction P towards the distant reflector. On its return along the path P′, the ray is reflected from an adjoining face of the revolving mirror into the receiving telescope, K.

[1] Michelson's revolving mirror executed 256 turns in a second.

The closing words of the paper under review attest the unappeased aspiration towards accuracy characteristic of the successful investigator.

"A still further perfection of the method," its author writes, "which would lead to a result of which the precision would be limited only by our means of linear measurement is, I conceive, within the power of art. It consists in placing the fixed mirror at so great a distance that the pentagonal revolving mirror would move through an arc of nearly 36° while the ray is going and returning. If a speed of 500 turns per second could be attained, the required distance would be thirty kilometres. Then, in opposite directions of rotation, the return ray would be reflected at phases of the mirror differing by the angle between two consecutive faces. The result would be that the receiving telescope would need to have but a small motion, and all the observer would have to measure would be the small angle by which the difference of positions of the mirror when the flash was received in opposite directions of rotation, differed from 72°. In the Rocky Mountains or the Sierra Nevada no difficulty would be found in finding stations at which a return ray could be received from a distance of thirty, forty, or even fifty kilometres, with little more dispersion and loss than at a distance of four kilometres through the air of less favoured regions. It is true that the surface of the distant reflector would have to be increased in proportion to the distance, but it would not be necessary to make a single reflector of great size. A row of ten reflectors, each six or eight decimetres in diameter, might be sufficient to insure the visibility of the return ray."

A. M. CLERKE

NOTES

AT a meeting of the Royal Society of Edinburgh on June 7, medals were presented as follows:—To Mr. John Aitken (Darroch), the Keith Prize for 1883–85, for his paper on the formation of small clear spaces in dusty air, and for previous papers on atmospheric phenomena; to Edward Sang, LL.D., the Makdougall-Brisbane Prize for 1882–84, for his communication on the need for decimal subdivisions in astronomy and navigation and on tables requisite therefor, and generally for his recalculation of logarithms both of numbers and of trigonometrical ratios; to Mr. B. N. Peach the Neill Prize for 1883–86, for his contributions to the geology and palæontology o Scotland.

THE organising committee of Section A has arranged that a special discussion shall be held, at the Birmingham meeting of the British Association, jointly with Section D, on the physical and physiological theories of colour-vision. The discussion will be opened by Lord Rayleigh, and Dr. Michael Foster will also take part in it. Persons who wish to contribute papers bearing on the subject of discussion are requested to send their names to the Recorders of Sections A or D, at 22, Albemarle Street, W., not later than August 1.

THE death is announced, in his seventieth year, of Mr. Llewellyn Jowett, the well-known archæologist.

At the Conference at the Colonial and Indian Exhibition on Wednesday, June 23, a paper was read by Mr. W. Lant Carpenter, on "The Position of Science in Colonial Education." The colonies to which Mr. Carpenter had directed his attention were:—Canada generally; South Africa, the Cape of Good Hope and Natal; Western and South Australia, Victoria, New South Wales, Queensland, New Zealand, and Tasmania, the last of which, unfortunately, is not represented at the present Exhibition. An account of the present condition of scientific education in each of these colonies was given. As a general conclusion, Mr. Carpenter thought that the claims of science to a place in State-aided primary education were more fully recognised than in the old country, and this, not merely because it was the only foundation upon which a system of technological education could be securely built, but for its value in drawing out the minds of the pupils. As regards the branches by which the time-honoured routine of subjects may be most beneficially varied, precedence was almost universally accorded to drawing, and to the objective presentation of the elements of science. In secondary grammar and high schools, however, science scarcely occupied a position equal to that in corresponding English schools, but there were many signs of improvement in this respect. In the Colleges and Universities of the older colonies the classical and academic influence was still very strong, while in the newer ones the claims of scientific education to be put on an equal footing with literary were recognised. Great has had been the progress of public opinion in England during the last few years on the importance of science as an element in education, the author was disposed to consider it greater in the colonies in the same period. Certainly the development of that opinion to its present point had been much more rapid in the colonies than at home. There were many voluntary colonial Associations for the promotion of science, and the author concluded his paper by throwing out the suggestion that, if there were grave and practical difficulties in the way of an Imperial federation of the Australian colonies, the establishment of an Australian Association for the Advancement of Science, somewhat on the lines of the British and American Associations for similar purposes, might not be beyond the reach of practical men of science, and he was strongly of opinion that such a federation would tend to strengthen "the position of science in colonial education."

Arrangements have been made for the examination in the Indian Court of the Colonial and Indian Exhibition of certain commercial products, which are believed to be insufficiently known or to be suitable for new purposes. Among the substances which will be examined are fibres, silk and silk substitutes, drugs, tobacco, gums and resins, minerals, oils, oil-seeds and perfumery, dyes, mordants and pigments, timbers, tanning materials and leather, and food-stuffs. Any visitors to the Exhibition, who are interested in the subject, will be permitted to attend these examinations of products, which will take place in the Commercial Room, attached to the Economic Court, where all further information may be obtained. Should the results of this examination render such a course desirable, Conferences of a formal character will probably be held at a later date.

The International Society of Electricians has decided upon building laboratories for the use of physicists in Paris. They will be established in the grounds of the old Collège Rollin, granted by the city of Paris, in the vicinity of the School of Practical Physics recently erected by the Municipal Council. The funds will be supplied by public subscription, a contribution from the Society, and a sum of 360,000 francs, which is the surplus of the last Electrical Exhibition organised by M. Cochery.

We take the following from *Science*:—It will be remembered that in the month of May a gentleman in Brooklyn died from hydrophobia. His medical attendants, competent physicians, had no doubt about their diagnosis, and his symptoms were characteristic of that disease. Confirmatory of this opinion, the autopsy revealed no lesion to which could be attributed the symptoms from which he suffered—a condition which is also characteristic of hydrophobia. Portions of the brain and the spinal cord were carefully wrapped in cloth wet with a solution of bichloride of mercury and sent to Dr. Sternberg. Small portions of these were thoroughly mixed with sterilised bouillon; and this broth was then, by means of a hypodermic syringe, injected under the dura mater covering the brain of a rabbit, a small button of bone having been first removed by a trephine. The wound was then closed by sutures. Three rabbits were thus operated upon. One died at the end of twenty-four hours as the result of the operation; hydrophobia, of course, having nothing to do with it. Another is now, after eighteen days, apparently well. The third one, on the sixteenth day, commenced to show signs of being ill: he was disinclined to move, and in a few hours evidences of paralysis appeared, at first in the hind-legs, and subsequently in all the extremities. On June 5, the eighteenth day after the operation, he died. The wound had healed, and there were no evidences of inflammation. The brain showed no softening at the point where the inoculation was made, no pus, nor any evidences of inflammation either of the brain substance or of its membranes. The cord also appeared normal. Portions of the medulla of this rabbit were immediately mixed with sterilised bouillon, and two rabbits were inoculated in the same manner as has been described. This case is of great interest as being, so far as we know, the first animal in this country to become affected with hydrophobia from inoculation with material taken from a person who died from that disease. If Dr. Sternberg is as successful with these rabbits as with the first, there is no reason why the series cannot be continued, and thus the protective virus of Pasteur be obtained in this country, and a trip to Paris by the victims of dog-bites made unnecessary. As we go to press we learn that the second rabbit, mentioned above as remaining unaffected for eighteen days, shows unmistakable signs of hydrophobia.

Dr. Thornton, the new Director of the Madras Museum, has organised a series of investigations for the purpose of studying systematically the marine and terrestrial fauna on the west coast of the Presidency. They will be continued from time to time as favourable opportunities arise.

At 8.40 a.m. on May 17 a remarkable phenomenon was witnessed at Dönnæs, in the north of Norway, some twenty five miles south of the Polar Circle. A small bright horizontally-lying circle was suddenly seen with its centre right in zenith, the periphery passing through the centre of the sun. In the circle were besides four mock suns, in east, west, north, and south, so that they would almost have formed the corners in an irregular square. There was also another circle perpendicular on the other, and with the sun as centre, but it was much fainter. The little circle and the two mock suns nearest the sun were rainbow-coloured, and the great circle and the two mock suns furthest off intense white. After half an hour the phenomenon faded for a while, but soon again became as intense as before. It disappeared after having been in view for an hour and a half. The weather was fine and sunny, but hazy. Afterwards it became cloudy with rain.

The large zoological collection known as the Museum Godeffroy has just been purchased by Mr. Damon (Weymouth). The ethnological portion was sold a short time since to the Leipzig Museum, as already announced in Nature.

In addition to the specially meteorological results contained in the report of the Hong Kong Observatory for the past year,

which we noticed last week, Dr. Doberck, the Government Astronomer there, refers to the great value of the systematic meteorological observations with verified instruments which have lately been set on foot at many of the stations and lighthouses of the Chinese Customs, and which will serve as an important aid in the investigation of typhoons. He pays a well-deserved tribute to Japan's "extensive meteorological service, conducted on approved principles," and to the useful weather maps issued by the Tokio Observatory, while he deplores the absence of a similar comprehensive service in the Philippines and the non-publication of such data as are observed there—an omission which increases the labour of following typhoons in their passage across or near to those islands. The intention of the French authorities to establish a meteorological observatory at Haiphong, on the coast of Tonquin, seems to have been dropped, at least for a time, since the death of the distinguished meteorologist, Dr. Borius. The Hong Kong Observatory during the year was supplied with a gazing-telescope, as was recommended by Col. Palmer in the original project, a Lee equatorial from Greenwich having been erected. In 1882, when the plan of the Hong Kong Observatory was first drawn up, the local Government was willing to pay for one thoroughly equipped, but the Colonial Office at home cut out the most important part of the provision for magnetic research, and this unfortunate spirit of parsimony in expenditure connected with scientific research seems now to have extended to Hong Kong. For Dr. Doberck complains that the addition to the work of his Observatory is not accompanied by a corresponding increase of funds and staff, that his telegraphic facilities are insufficient to give full effect to the proper purposes of the establishment, that the slopes of the observatory hill have been left unturfed since they were stripped in 1883, and that no effective measures have been taken to improve the unhealthiness of the site, which is on the Kowloon peninsula opposite the town of Victoria. It sounds incredible that the gun which was supposed to be set apart for the purpose of announcing the approach of a typhoon has also been used to announce the arrival of the mail-steamers—a course which is as senseless as it is cruel, for it confuses the unfortunate boat- and junk-men who swarm in the Hong Kong waters, and who either throw up their work and flee into a place of refuge when only a mail-steamer is arriving, to their great loss of time and money, or they take no precautions at all when a typhoon is really at hand. In the latter case, if any lives were lost, an English coroner's jury would probably indict the official responsible for this gross negligence for manslaughter, as they would the chemist who carelessly gives strychnine in place of Epsom salts. Dr. Doberck proposes that, if the gun be used for post-office purposes, it should cease altogether as a typhoon warning.

AT the annual meeting of the Chemical Society of Tokio, held on April 10, and reported in the *Japan Mail*, a very satisfactory report was read. The Society is composed of Japanese and foreign men of science, the total number of members last year being eighty-six, and being constantly on the increase. The number of papers read amounted to nineteen. The journal of the Society is published four times a year, and it is hoped to make it a monthly journal soon, "especially as the number of papers read is not few, nor their nature inferior to those which appear in foreign journals." The Society undertook to translate chemical terms into Japanese about four years ago, and it now possesses (though not yet published) a dictionary of commoner chemical terms in Japanese, English, French, and German. It has also undertaken to establish a system of chemical nomenclature in the Japanese language, of which the nomenclature of the elements and of inorganic compounds is already nearly finished. It is hoped that a sound and complete system of nomenclature will be published in the course of the coming year. An address was delivered by Mr. Watanabe, the head of the new University of Japan, and, on other grounds, an important official, who impressed on the members of the Society the necessity of making chemistry popular, on account of its intimate connection with arts and manufactures. He hoped, too, that more and more original work in science would be done in Japan, for on such work depended ultimately all improvement in manufacturing processes.

AT the same meeting a paper was read by Dr. Kellner on the deportment of urea towards soils, with special reference to the mode of manuring the soil employed in China and Japan. The experiments on this subject which have been carried out at the Komaba Agricultural College show that the application of fresh excreta is injurious to crops, and that, in this state, a great deal of the most valuable nitrogenous compounds of the manure is lost by rain, which carries the urea into the deep subsoil beyond the reach of the roots of the plants. Japanese farmers had long ago come to a similar conclusion for themselves, for they only employed this manure when in a highly decomposed state, when the urea had been converted by putrefaction into ammonium carbonate.

THE report of the Rugby School Natural History Society for the past year is a very satisfactory one, for it shows great activity on the part of the members and of the Society collectively in every direction. With a single exception the papers are contributed by working members or associates; the collection of British quadrupeds commenced last year is almost complete, and a new vivarium has been added to the Society's resources. The papers deal with many subjects from China to heraldry, but local ornithology appears to have received special attention; for the Society's first prize essay was won by Mr. Austen with a paper on the water-birds of Rugby; the second by Mr. Mander, on some of the large birds round Rugby. Mr. Solly also contributes an interesting paper on microscopic fungi, with illustrations. But it is in the sectional reports that the activity of the Society is made most manifest. Here we find a meteorological report, based on continuous observation throughout the year; a vivarium report; a report from the entomological section, containing a list of the Lepidoptera observed at or near Rugby during the year; similarly the report of the botanical section contains a long list of observations, in which are some plants hitherto unknown in the flora of Rugby; the zoological report, it may be added, is a specially long one; and the book concludes with the report of the Temple Observatory, where so much good astronomical work is being done. Of the many excellent natural history societies which pass under our notice from time to time, few can show more or better work than the Rugby School Society.

ONE interesting matter referred to in the report just noticed was the presentation of an address of congratulation to Mr. M. H. Bloxam, a very energetic member of the Society, on reaching his eightieth year. In his reply Mr. Bloxam claimed to be, in a peculiar degree, a link between the Rugby of the present and that of the past. He transacted business with a Rugbeian who entered the school in the reign of George II., 127 years ago. Mr. Bloxam entered Rugby School about 72 years ago, and left it 64 years ago; and while he was at the school a retired master died who was born in 1718, early in the reign of George I., 167 years ago. The Rev Henry Holyoak was master of the school in the boyhood of that retired old master, and Mr. Holyoak was alive in the lifetime of a nephew of Lawrence Sheriff, the founder of Rugby School. Now Lawrence Sheriff died 318 years ago. Thus three lives, one of them being Mr. Bloxam's, carry us back to the foundation of Rugby School.

We are requested to state that the annual Students' *Conversazione* will take place at the Finsbury Technical College on Friday evening, July 2, commencing at 7 o'clock. A good exhibition of apparatus, models, and specimens has been arranged to illustrate the various branches of applied science and art comprised under the College scheme of technical education.

A Swedish geologist, Dr. H. Sjögren, is about to proceed to the naphtha regions on the Caspian Sea, in order to prosecute geological studies.

We have received from Messrs. Griffin and Co. the third annual issue of the "Year-Book of the Scientific and Learned Societies." It gives a brief chronicle of the work done during the year by the various Societies, together with the necessary information as to official changes.

The Saghalien Ainos do not exhibit the same uncouthness as those of Yezo; there is a greater absence of beards and of hairy bodies generally. The hue of the skin very closely resembles that of the Caucasian; the foreheads are high but narrow, and their general bearing and facial expression denote an intelligence much superior to that of the Yezoines. As for the theory of an ethnical connection between the Ainos and the Japanese, Mr. Penhallow says that an examination of the pure types would not permit such a belief to be entertained. There is a mixture of the two in places, but the half-breed is as easily recognisable there as elsewhere in the world. The Japanese, he concludes, are unquestionably Mongoloid, while the facts show the Ainos to be physically distinct, while the best authorities agree in the great resemblance which they bear to Europeans, the prevailing view being that they are distinctly Aryan.

The additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus* ♀) from India, presented by Mrs. George Willing; two Tcheli Monkeys (*Macacus tcheliensis* ♂ ♀) from Jung-ling, near Pekin, presented by Dr. S. W. Bushell, C.M.Z.S.; a Wild Swine (*Sus scrofa* ♀) from Tangier, presented by Mr. John Brooks; four Sparrow Hawks (*Accipiter nisus*), British, presented by Mr. J. Rowland Ward, F.Z.S.; an Egyptian Goose (*Chenalopex ægyptiaca*), a Robben-Island Snake (*Coronella phocarum*), a Hoary Snake (*Coronella cana*), an Infernal Snake (*Boodon infernalis*), a Rhomb-marked Snake (*Psammophylax rhombeatus*), a Horned Viper (*Vipera cornuta*), eight Geometric Tortoises (*Testudo geometrica*), a Leopard Tortoise (*Testudo pardalis*), three Areolated Tortoises (*Homopus areolatus*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; a Crowned Horned Lizard (*Phrynosoma coronatum*) from California, presented by Mr. S. Upton Robins; a Common Viper (*Vipera berus*), British, presented by Mr. W. H. B. Pain; a Tuatera Lizard (*Sphenodon punctatus*) from New Zealand, presented by Capt. R. Sutherland; a Tarantula Spider (*Mygale*, sp. inc.) from Bahamas, presented by Mrs. E. Blake; a Peruvian Thicknee (*Œdicnemus superciliaris*) from Peru, two White-backed Piping Crows (*Gymnorhina leuconota*) from Australia, deposited; a Balearic Crane (*Balearica pavonina*) from West Africa, purchased; a Japanese Deer (*Cervus sika*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

The Absorption Spectrum of Oxygen.—About three years ago M. Egoroff was able to show that the great groups A and B in the solar spectrum were due to the absorption of oxygen. More recently the α band was also found to be due to the same gas. M. Janssen, studying the absorption of oxygen has now discovered that under certain conditions the gas yields another spectrum, composed no longer of lines easily separated, but of shaded bands which can only be resolved with great difficulty. This system of bands appears for moderate pressures much later than the spectrum of lines, but it shows itself very quickly with increase of the density: the two systems are so different that it is possible to obtain either the first without the second or *vice versâ*. M. Janssen was at first unable to explain how it was that these bands were not visible in the solar spectrum when they were easily obtained by passing light through thicknesses of oxygen far less than the sun's light has to traverse before reaching us. But further experiments showed that these bands did not develop in proportion to the thickness of the stratum of oxygen producing them, multiplied by its density, but in proportion to the thickness multiplied by the square of the density. The density of our atmosphere being small as compared with some of the pressures at which M. Janssen worked, the non-appearance of these bands amongst the telluric lines of the solar spectrum is readily explained.

Potsdam Observatory.—The fifth volume of the *Publications* of the Astrophysical Observatory of Potsdam is occupied with a very careful determination, by Drs. Müller and Kempf, of the wave-lengths of 300 of the principal lines in the solar spectrum. Four gratings were used in this inquiry—one with about 2500 lines to the inch, the second with 6250 lines, and the third and fourth with about 10,000 lines to the inch. Eleven normal lines were first measured with all four gratings and in the spectra of three or four orders with each grating, every observation being carefully corrected for temperature, &c. The computation of the wave-lengths of the 300 lines follows, and the details of the reduction of the observations of the eleven normal lines, and a catalogue of the wave lengths of 2614 lines as given in the Potsdam Atlas of the spectrum, and as now corrected, concludes the work. The following are the wave-lengths of the selected normal lines, expressed in millionths of a millimetre:—C, 656·314, 640·035, 612·247; D_2, 589·625, 562·475, 545·580; b_2, 517·284, 495·770, 470·321, 441·534, and 407·186. It would seem from these determinations that Angström's wave-lengths require small but sensible corrections.

The Binary Star γ Coronæ Australis.—With reference to our note on this double star (Nature, vol. xxxiii. p. 425), in which we pointed out the large difference in the position-angles computed, for the present year, from the orbit of Mr. Gore and from that of Mr. Downing, we may draw attention to a communication by Mr. H. C. Wilson, of the Cincinnati Observatory, printed in the *Observatory*, No. 111, pp. 234–235. Mr. Wilson gives the mean results of observations of the binary in 1881 and 1883 as follows:—

1881·72	45°·53	1″·38
1883·62	37·75	1·62

The angles computed from Mr. Gore's elements for these two epochs are respectively 47°·29 and 36°·49, which may be regarded as agreeing fairly well with the observations. It appears, therefore, that of the two orbits referred to above, Mr. Gore's is by far the most satisfactory.

Observations of the Companion of Sirius.—Prof. Young has communicated to the *Sidereal Messenger* (No. 46, p. 182) a series of measures of the companion of Sirius made at Princeton, for the most part with the 23-inch refractor, with powers of 460 and 300. Prof. Young remarks that during the present year the companion has been a difficult object, except when the seeing was good, and there have been fewer good nights than usual. The mean annual results are:—

Position-Angle			Distance		
Epoch	Measure	No. of nights	Epoch	Measure	No. of nights
1883·105 ...	39°·0 ...	1 ...	1883·105 ...	9″·41 ...	1
1884·273 ...	36·30 ...	5 ...	1884·270 ...	8·70 ...	4
1885·112 ...	34·06 ...	7 ...	1885·089 ...	8·09 ...	8
1886·047 ...	29·77 ...	4 ...	1886·049 ...	7·59 ...	3

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 JUNE 27—JULY 3

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on June 27

Sun rises, 3h. 47m.; souths, 12h. 2m. 44·1s.; sets, 20h. 19m.; decl. on meridian, 23° 20′ N.: Sidereal Time at Sunset, 14h. 42m.

Moon (three days after Last Quarter) rises, 1h. 6m.; souths, 7h. 59m.; sets, 15h. 4m.; decl. on meridian, 9° 31′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury ...	5 1	13 17	21 33	23 25 N.
Venus ...	1 42	9 18	16 54	17 22 N.
Mars ...	11 20	17 30	23 40	1 11 N.
Jupiter...	11 17	17 31	23 45	2 3 N.
Saturn ..	4 16	12 26	20 36	22 34 N.

June	h.	
28 ...	10	.. Mars in conjunction with and 0° 59′ south of Jupiter.
29 ...	0	... Venus in conjunction with and 2° 57′ north of the Moon.
July		
2 ...	16	... Sun at greatest distance from the Earth.

Variable Stars

Star	R.A. h. m.	Decl. ° ′		h. m.
U Cephei	0 52·2	81 16 N.	June 29,	0 54 *m*
S Leonis	11 5·0	6 5 N.	July 3,	*M*
δ Libræ	14 54·9	8 4 S.	,, 3,	23 6 *m*
U Ophiuchi...	17 10·8	1 20 N.	,, 1,	3 46 *m*
			,, 1,	23 53 *m*
X Sagittarii...	17 40·4	27 47 S.	,, 3,	2 0 *M*
R Scuti	18 41·6	5 50 S.	June 27,	*m*
R Lyræ	18 51·9	43 48 N.	,, 28,	*m*
η Aquilæ	19 46·7	0 43 N.	July 1,	0 0 *m*
R Vulpeculæ	20 59·3	23 22 N.	,, 1,	*M*
δ Cephei	22 24·9	57 50 N.	June 29,	0 0 *M*

M signifies maximum; *m* minimum.

Meteor Showers

The principal radiants of the season are:—Near β Ursæ Majoris, R.A. 164°, Decl. 57° N.; near ζ Ursæ Majoris, R.A. 210°, Decl. 55° N.; near ξ Serpentis, R.A. 263°, Decl. 15° S.; from Vulpecula, R.A. 302°, Decl. 27° N.; near ζ Pegasi, R.A. 338°, Decl. 13° N.

Stars with Remarkable Spectra

Name of Star	R.A. 1886·0 h. m. s.	Decl. 1886·0 ° ′	Type of spectrum
R Aquilæ...	19 0 32	8 3·4 N.	III.
R Sagittarii	19 9 59	19 30·4 S.	III.
229 Schjellerup	19 25 33	76 20·1 N.	IV.
228 Schjellerup	19 27 46	16 37·2 S.	IV.
R Cygni ...	19 33 45	49 56·6 N.	III.
D.M. + 32°·3522	19 36 34	32 21·1 N.	IV.
χ Cygni ...	19 46 11	32 37·5 N.	III.
D.M. + 43°·3425	19 53 31	43 57·3 N.	IV.
D.M. + 35°·4001	20 5 46	35 49·5 N.	Bright lines
D.M. + 35°·4002	20 6 6	35 36·8 N.	IV.
D.M. + 35°·4013	20 7 24	35 50·6 N.	Bright lines
D.M. + 36°·3956	20 10 4	36 17·8 N.	Bright lines
D.M. + 15°·4172	20 23 54	15 53·7 N.	III.
D.M. + 17°·4370	20 32 52	17 52·0 N.	III.
V Cygni ...	20 37 37	47 44·1 N.	IV.
D.M. + 17°·4401	20 40 15	17 40·6 N.	III.

SECOND ANNUAL REPORT OF THE COUNCIL OF THE MARINE BIOLOGICAL ASSOCIATION OF THE UNITED KINGDOM[1]

I. THE Council has met during the past year nine times, viz. on October 5, October 27, December 14, March 19, March 25, April 5, April 9, May 19, and June 7. The chief business which has occupied the Council during the past year has been the preparation of the plan of the Laboratory building now in course of erection on the Citadel Hill at Plymouth, and the arranging for the execution of this plan by building and engineering firms. Further, the Council has given much time and attention to negotiations with the Lords of Her Majesty's Treasury with regard to a grant in aid of the objects of the Association.

The most important facts which the Council has to communicate to the Association as the result of the year's work are:—

(1) The undertaking on the part of the Lords of the Treasury to submit to Parliament a grant of 5000*l.*, to be paid in two yearly instalments, and 500*l.* a year for five years, in aid of the objects of the Association; and

(2) The formal approval by the Council of a contract by Mr. Berry, of Plymouth, to erect the buildings and construct the reservoir of the Plymouth Laboratory at the price of 5902*l.* 16*s.*, and also of a contract by Messrs. Leete, Edwards, and Norman, of London, to construct and fit the aquariums and pumping apparatus for the Plymouth Laboratory at the price of 3000*l.*

The excavation of the site on the Citadel Hill at Plymouth is now actually in progress, and the Laboratory will be in all probability ready for occupation by this time next year.

In June 1885, the Council reported a capital sum of nearly 8000*l.* as definitely promised to the Association, of which 4787*l.* was in the hands of the Treasurer.

The Council now has to report a capital sum of 10,000*l.* available for expenditure on the building and fitting of the Plymouth Laboratory, and in addition an annual income from investments and annual subscriptions of 1100*l.* a year. Of the disposable capital sum about 5000*l.* is in the hands of the Treasurer, whilst the sum of 5000*l.* is to be paid in two instalments, one in 1886, and one in 1887, by Her Majesty's Treasury.

In June 1885, the Association numbered 277 members, of whom 163 were annual subscribers, the rest having compounded. It now numbers 305 members, of whom 169 are annual subscribers.

Amongst important donations to the Association made during the past year, the Council desire especially to mention the sum of 500*l.* received from Mr. John Bayly, of Plymouth, who was already a Founder, and is now qualified as a Life Governor of the Association. On hearing that the Council felt it to be necessary to omit certain features in the plan of the Plymouth Laboratory as approved by them, on account of the expense involved, Mr. John Bayly came forward with this munificent donation, and thus enabled the Council to carry out their original design.

II. In reference to the grant from the Treasury, the Council submit, for the information of the members of the Association, the final letter received from the Lords of the Treasury and the answer returned by the Council to that communication.

Treasury Chambers, December 9, 1885

SIR,—I have laid before the Lords Commissioners of Her Majesty's Treasury your letters of the 2nd and 13th ultimo, on the question of the proposed assistance to be given by the Government to the Marine Biological Association of the United Kingdom. Their Lordships have considered the matter very carefully, and they now desire me to inform you that they are prepared to propose to Parliament a grant of 5000*l.* towards the cost of the Laboratory which the Association intends to construct at Plymouth, such grant to be paid in two instalments of 2500*l.* each, one in 1886 87, and the other in 1887–88, and also an annual grant of 500*l.* for five years, beginning in the year 1887–88, towards the current expenses of the Laboratory, on the following conditions:—

(1) That the Council of the Association agrees to have its accounts formally audited each year, and to furnish a statement of income and expenditure to the Treasury.

(2) That the Council undertakes to issue at regular intervals (probably half-yearly) a detailed report of the work done in the Plymouth Laboratory, and to furnish the Treasury with such report.

(3) That the Council pledges itself definitely to aim at procuring practical results with regard to the breeding and management of food-fishes.

(4) That the Council undertakes to place space in the Plymouth Laboratory at the disposal of any competent investigator deputed by a recognised authority to carry out any investigation into fish questions for which the Laboratory can give facilities.

I am to add that my Lords will make the necessary provision for these grants in the Estimates for the coming year, but until Parliament shall have sanctioned the grant, it will not be in their power to make any payments to the Association in fulfilment of the above promise of assistance.

In conclusion I am to suggest, with reference to your letter of the 8th instant, that in future any communications between the Association and the Scotch Fishery Board should be conducted through the Secretary for Scotland.

I have the honour to be, Sir, your obedient servant,

M. W. RIDLEY

[1] Presented to the Annual General Meeting of the Association on June 8, 1886, Prof. Huxley, President of the Association, in the chair.

SIR,—I have laid before the Council of the Marine Biological Association your letter of the 7th inst., and I am instructed by the Council to say that they accept the four conditions mentioned in your letter as those upon which the Lords Commissioners of Her Majesty's Treasury are prepared to propose to Parliament a grant of 5000*l*., and an annual grant of 500*l*. a year for five years, in aid of the building and of the current expenses of the Laboratory about to be erected by the Association on Plymouth Sound.

The Council of the Association desire me to express, through you, to my Lords, the gratification which they experience in receiving this important assistance and mark of confidence from my Lords, and to offer at the same time their thanks to my Lords for the favourable consideration which has been accorded to the request of the Council.

In regard to the term "a recognised authority" used in the fourth condition proposed by my Lords and accepted by the Council, I am directed to say that the Council assumes that a "recognised" authority means a "STATE" recognised authority, such as the Scotch Fishery Board or Her Majesty's Inspectors of Fisheries.

On behalf of the Council of the Marine Biological Association I am accordingly empowered to state—

(1) That the Council agrees to have its accounts formally audited each year, and to furnish a statement of income and expenditure to the Treasury.

(2) That the Council undertakes to issue at regular intervals (probably half-yearly) a detailed report of the work done in the Plymouth Laboratory, and to furnish the Treasury with such report.

(3) That the Council pledges itself definitely to aim at procuring practical results with regard to the breeding and management of food-fishes.

(4) That the Council undertakes to place space in the Plymouth Laboratory at the disposal of any competent investigator deputed by a recognised authority to carry out any investigation into fish questions for which the Laboratory can give facilities.

I have the honour to be, Sir, your obedient servant,
E. RAY LANKESTER, Hon. Sec. M.B.A.
To Sir M. W. Ridley, Bart.

III. The attention of the Council having been drawn to the statement made in the House of Commons by the President of the Board of Trade to the effect that it was the intention of the Government to constitute a Fisheries Department under that Board, the following letter was addressed on March 19 to the President of the Board of Trade :—

April 7, 1886

SIR,—I am desired by the Council of the Marine Biological Association to inform you that they have observed from an official statement in Parliament that it is the intention of Her Majesty's Government to constitute a Fishery Department as a branch of the Board of Trade. With a view to the meeting of the Council of the Association on March 19 last, I took the liberty to inquire whether you would be disposed to receive a deputation from the Council in order that it might have an opportunity of placing before you some account of the origin of the Association and of its proposed operations with regard to the fisheries of the British seas. The Council having learned that you thought the reception of such a deputation inadvisable, have instructed me to communicate to you, in accordance with your invitation, a brief account of the position of the Association and of its relations to the Government.

The Council have taken steps to erect on the Citadel Hill, at Plymouth, on a site granted by the War Office, a Laboratory, which will be equipped with all the appliances in the shape of tanks and working rooms suited for the study and observation of fishes and other marine organisms. It will afford convenient accommodation for from sixteen to twenty scientific investigators qualified to engage in biological research.

In view of the national utility of such a Laboratory in connection with the fishing industry, the Lords Commissioners of Her Majesty's Treasury have agreed to submit to Parliament a vote of 5000*l*., in two annual instalments, towards the erection of the Laboratory, and to supplement this when the Laboratory has been completed by an annual subsidy of 500*l*.

I am directed to annex for your information a copy of the letter from my Lords embodying these undertakings. You will observe that they have laid stress on the duty of the Association to investigate everything relating to the economy of food fishes, and that they further desired that the Marine Biological Association should work in harmony with the Scotch Fishery Board, which was, at that time, the only formally constituted body connected with the subject in Great Britain.

The Council apprehend, therefore, that the Marine Biological Association, receiving as it does liberal support and recognition of the importance of its aims from the Government, should place at the disposal of the new Fishery Department any resources it may possess for the prosecution of such scientific investigations as the Department may wish to initiate.

The Council further desire me to say that they will be glad to enter into such relations with the Department as may seem to you best calculated to effect the object.

In making this offer the Council believe that they are carrying out the spirit of the conditions imposed upon them by my Lords in the letter of which a copy is inclosed.

I am, Sir, your most obedient servant,
E. RAY LANKESTER, Hon. Sec. M.B.A.
The Right Honourable A. J. Mundella, M.P.,
Board of Trade

To this letter the following answer was received :—

Board of Trade, Whitehall Gardens, S.W.,
April 8, 1886

SIR,—I am directed by Mr. Mundella to acknowledge the receipt of your letter of April 7, and to request that you will be so good as to convey to the Council of the Marine Biological Association his best thanks for their kind offer which they have made to place at the disposal of the new Fishery Department the resources which they possess for the prosecution of such scientific investigations as the Department may wish to initiate. Mr. Mundella wishes me to add that the matter is now receiving his most careful consideration.

I am, Sir, your obedient servant,
Prof. E. Ray Lankester T. W. P. BLOMEFIELD

IV. In reference to the building and aquarium fittings, which have been approved, and are now in course of construction for the Laboratory at Plymouth, the Council appointed, as stated in the last Annual Report, a Committee consisting of the Treasurer, the Secretary, Dr. John Evans, Prof. Moseley, and Mr. Spence Bate. These gentlemen, assisted by Mr. Walter Heape, Assistant Secretary, and by Mr. Inglis, civil engineer, of Plymouth, met on several occasions in order to consider the details of the Laboratory building and its fittings. Information and advice was obtained from the directors of the existing laboratories and aquariums in Europe and in the United States of America, as well as from engineering firms acquainted with the special kind of work required. The limitation of the funds at the disposal of the Council had to be strictly borne in mind by the Committee, and finally, after much deliberation, a plan of buildings and fittings was submitted by the Committee and approved by the Council. The approval of the War Office had to be obtained in regard to the design of the building which is to be erected on the site granted by that Department of State, and the Council has had the advantage in this connection of the advice and assistance of Mr. E. Bell, the architect to the War Office. The design, for which a contract has now been accepted by the Council, has the approval of the authorities of the War Office and of the Town Council of Plymouth.

V. Shortly after the annual meeting in June 1885, Mr. St. Leger Bunnett, of New Stone Buildings, 60, Chancery Lane, was appointed Assistant Secretary with the special purpose of aiding the Secretary in obtaining subscriptions and donations.

In January of the present year Mr. Walter Heape vacated his post of Assistant Secretary, and was appointed Resident Superintendent of the Plymouth Laboratory at a salary of 200*l*. a year. Mr. Heape will be provided with a suite of apartments in the Laboratory building. Since his appointment Mr. Heape has visited the Zoological Laboratory at Naples for the purpose of acquiring information which may assist him in the management of the Plymouth Laboratory. He will at once proceed to Plymouth and take up his residence there, in order to commence an investigation of the natural history of Plymouth Sound and to enter into relations with the fishermen of the district, so as to prepare the way for the operations of the Laboratory when completed. Mr. Heape will also watch the erection of the Laboratory building and report from time to time to the Council of the Association at Plymouth.

VI. The Council propose to make two alterations in the

by-laws of the Association which will require the approval of the present general meeting and of a subsequent special general meeting, which will be duly summoned. The first proposal is to enact by by-law of the Association that the Prime Warden of the Fishmongers' Company shall always be *ex officio* a member of the Council of the Association. The Fishmongers' Company have shown their interest in the enterprise of the Association by contributing 2000*l.* to its funds. In reply to an inquiry from the Council, the Court of Assistants of the Fishmongers' Company have cordially accepted the proposition that the Prime Warden of the Company should hold the official relation to the Association above proposed. The Council therefore propose to alter By-law 2 of the Association by the insertion between the words "officers" and "and fourteen other members" of the words "the Prime Warden of the Fishmongers' Company for the time being."

The second proposal has relation to the admission of the Universities of Great Britain and Ireland to a share in the government of the Association. As was stated in the last Annual Report, members of the University of Cambridge have subscribed a sum of 500*l.* for the purpose of qualifying the University as a Governor of the Association. During the past year a similar fund has been raised by members of the University of Oxford. At the annual general meeting in June 1885, in view of these proceedings, the following addition to By-law 17 was carried: "Any University of the United Kingdom, on the payment of 500*l.* to the Association by members of the University, shall, if the Council of the Association consent thereto, acquire as a consequence the perpetual right of nominating one member of the Council of the Association."

The Council now propose to erase the words just cited, and to substitute the following:—

"Any University of the United Kingdom, on the payment of 500*l.* to the Association in the name of the University and for the purpose of acquiring the right herein specified, shall, if the Council of the Association assent thereto, become a Governor of the Association, and acquire the perpetual right of nominating annually one member of the Council of the Association to serve for one year (from the annual meeting in one year to that in the following year); and any resident member of the University subscribing 100*l.* or more to such fund of 500*l.*, shall, in virtue of such subscription, become a 'Founder' of the Association."

VII. The Council have again to record a severe loss to the Association in the list of its Vice-Presidents owing to the death of Dr. W. B. Carpenter, C.B., F.R.S. Dr. Carpenter was a warm supporter of the Association, and contributed largely by his advocacy of its objects to the success which has now been attained.

VIII. The Council do not propose any change in the list of Officers, Vice-Presidents, and Council for the ensuing year. They desire to notify that the following gentlemen have qualified by subscription of 500*l.* each as Life-Members (Governors) of the Council, viz. Mr. Robert Bayly, of Plymouth, 1885; Mr. Bazley White (Clothworkers' Company), 1885; Mr. E. L. Beckwith (Fishmongers' Company), 1885; and Mr. John Bayly, of Plymouth, 1886.

IX. During the ensuing year the building at Plymouth will be in course of erection. It is anticipated that the chief duty of the Council during this period will be to organise a scheme of investigation to be carried out at Plymouth when the Laboratory is in working order.

It will be especially the business of the Council to determine the conditions under which the Laboratory shall be accessible to the naturalists of the United Kingdom and other countries for the purpose of aiding in those inquiries into the life-history of marine animals and plants, and particularly of food-fishes, which it is the purpose of the Association to foster.

X. The plan of the Laboratory building includes a library. The Council take the present opportunity of asking for donations of works relating to fisheries and to marine zoology and botany for the library. They will also be glad to receive subscriptions towards a special library fund, in reference to which and all similar matters, the Hon. Secretary, Prof. Lankester, can be consulted.

XI. In conclusion, the Council desire again to express the great obligation which the Association is under towards the Council of the Linnean Society for the continued permission accorded by that body to the Association to meet in the rooms of the Society.

MEMORANDUM RELATING TO THE MODE IN WHICH SCIENTIFIC KNOWLEDGE CAN BE MADE USEFUL TO ENGLISH FISHERIES

THE following Memorandum has been presented to the President of the Board of Trade and officially acknowledged by him:—

Without committing ourselves to all the statements and opinions contained in the subjoined Memorandum, we, the undersigned, wish to state that we concur generally with the views as to the proposed constitution of the new Fishery Department therein expressed—

Argyll, K.G., F.R.S.; Walsingham; Stalbridge; E. Marjoribanks, M.P., Member of the late Royal Commission on Trawling; John Lubbock, Bart., M.P., F.R.S.; James Paget, Bart., F.R.S.; Henry W. Acland, K.C.B., F.R.S.; J. Fayrer, K.C.S.I., F.R.S., Honorary Physician to the Queen, Physician to the Secretary of State for India in Council; C. Spence Bate, F.R.S., Member of Council of the Marine Biological Association; I. Bayley Balfour, F.R.S., Sherardian Professor of Botany in the University of Oxford; Ed. Lonsdale Beckwith, late Prime Warden of the Fishmongers' Company, Member of Council of the Marine Biological Association; F. Jeffrey Bell, F.Z.S., Professor of Zoology in King's College, London, Member of Council of the Marine Biological Association; Henry B. Brady, F.R.S.; W. S. Caine, M.P., Member of the late Royal Commission on Trawling; P. H. Carpenter, F.R.S.; W. H. Dallinger, F R.S., President of the Royal Microscopical Society; F. Darwin, F.R.S.; W. T. Thiselton Dyer, C.M.G., F.R.S., Director of the Royal Gardens, Kew, Member of Council of the Marine Biological Association; W. H. Flower, F.R.S., Superintendent of the British Museum, Natural History, President of the Zoological Society, Vice-President of the Marine Biological Association; Hans Gadow, Strickland Curator and Lecturer on Animal Morphology in the University of Cambridge; Arthur Gamgee, F.R.S., Fullerian Professor of Physiology in the Royal Institution of Great Britain; W. H. Gaskell, F.R.S.; A. Günther, F.R.S., Keeper of the Zoological Department of the British Museum, Member of Council of the Marine Biological Association; S. F. Harmer, Fellow of King's College, Cambridge; W. A. Herdman, Professor of Zoology in University College, Liverpool, Member of Council of the Marine Biological Association; G. M. Humphry, F.R.S., Professor of Surgery in the University of Cambridge, late Professor of Anatomy, Fellow of King's College; J. N. Langley, F.R S., Fellow of Trinity College, Cambridge; E. Ray Lankester, F.R S., Jodrell Professor of Zoology in University College, London, Fellow of Exeter College, Oxford, Hon. Sec. of the Marine Biological Association; A. Milnes Marshall, F.R.S., Professor of Zoology in Owens College, Manchester, Member of Council of the Marine Biological Association; W. C. McIntosh, F.R.S., Professor of Natural History in the University of St. Andrews, Vice-President of the Marine Biological Association; H. N. Moseley, F.R.S., Linacre Professor of Human and Comparative Anatomy in the University of Oxford, Chairman of Council of the Marine Biological Association; Geo. J. Romanes, F.R.S., Member of Council of the Marine Biological Association; J. Burdon Sanderson, F.R.S., Waynflete Professor of Physiology in the University of Oxford; E. A. Schäfer, F.R.S., Professor of Physiology in University College, London; P. L. Sclater, F.R.S., Secretary of the Zoological Society, Member of Council of the Marine Biological Association; Adam Sedgwick, F.R.S., Fellow of Trinity College, Cambridge, Member of Council of the Marine Biological Association; C. Stewart, F.L.S., Conservator of the Museum of the Royal College of Surgeons, Member of Council of the Marine Biological Association; D'Arcy W. Thompson, Professor of Zoology in University College, Dundee; Sydney H. Vines, F.R.S.; W. F. R. Weldon, Fellow of St. John's College, Cambridge; Frank Crisp, Vice-President of the Linnean Society, Hon. Treasurer of the Marine Biological Association; Peter Eade, President, on behalf of the Norfolk and Norwich Naturalists' Society; J. Gurney, Mayor of Norwich, R. E. Burroughes, H. W. Stafford, John B. Pearce, Harry Bullard, S. Gurney Buxton, and John Barwell, Conservators under the Norfolk and Suffolk Fisheries Act, 1877, for the City of Norwich; C. Louis Buxton, T. C. Blofeld, and E. Frost, Mayor of Thetford, Conservators for Norfolk; B. F. Grimsey, Mayor of Ipswich, and Lieut.-Col. H. M. Leathes, Conservators for Suffolk; F. B. Archer, Conservator for Lynn; C. J. Greene, Hon. Sec. of the

Yare Preservation Society; Lieut.-Col. F. H. Custance; Michael Beverley, M.D.; H. W. Bidwell; G. F. Buxton; H. W. Fielden, late Naturalist to Sir G. Nares's Arctic Expedition; Thos. Southwell.

I.—*Preface*

(1) The necessity for an administration of our marine and fresh-water fisheries based upon thorough or scientific knowledge of all that relates to them has become obvious of late years. The Trawling Commission of 1884–85 has reported to this effect in so far as the subject of their inquiries is concerned. Other nations have adopted such a method of dealing with their fisheries, with good results and the promise of better.

(2) The inquiries and operations necessary cannot be conducted as the result of private commercial enterprise. They must be national in character.

(3) Whilst the general trade returns of the fishing industry, on the one hand, and the practical enforcing of regulations as to the protection of fishing-grounds and the restriction of fishing operations within certain seasons and localities are matters with which an ordinary staff of officials can effectually deal, yet the chief purposes of the operation of a satisfactory Fisheries Department are of such a nature that only expert naturalists can usefully advise upon them and carry them out. It is therefore important that the organisation of a State Fisheries Department should either be primarily under the control of a scientific authority who should direct the practical agencies as to trade returns and police, or that there should be distinct and parallel branches of the Department—the one concerned in scientific questions, the other in collecting trade returns and in directing the fisheries police.

(4) It does not appear that there is any ground for supposing that individuals of scientific training are *ipso facto* unfitted for administrative duties, and there would be obvious advantages in placing the operations of a Fisheries Department under one head. Indeed, it may be maintained that an education in scientific matters, and capacity for scientific work, is likely to produce a more practical and enterprising director of such a Department than could elsewhere be found. It has not been found desirable to place the administration of the important botanical institution at Kew in the hands of a non-scientific director, and there is no obvious reason for avoiding the employment of a scientific staff in the case of a Fisheries Department.

II.—*Nature of the Work to be done*

(1) Generally to ascertain what restrictions or modifications in the proceedings of fishermen are desirable, so as to insure the largest and most satisfactory returns prospectively as well as immediately from the fishing-grounds of the English coast and from English rivers and lakes.

(2) Especially to ascertain whether existing fishing-grounds can be improved by the artificial breeding of food-fishes and shell-fish, and to determine the methods of carrying on such breeding, and to put these methods into practice.

(3) To find new fishing-grounds.

(4) To introduce new fish—either actually new to the locality or new to the consumer.

(5) To introduce (if practicable) methods of rearing and fattening marine fish in stock-ponds.

(6) To look after the cultivation and supply of bait.

(7) To introduce new baits, new methods of fishing, improved nets, improved boats, new methods of transport and of curing.

The work can be divided into two sections. A. Investigation; B. Practical Administration.

A. *Investigation.*—The inquiries which are necessary in order to effect the purposes indicated above are as follows:—

(1) A thorough physical and biological exploration of the British coasts within a certain distance of the shore-line, especially and primarily in the neighbourhood of fishing-grounds. The investigation must include a determination of temperature and currents at various depths, the nature of the bottom, the composition of the sea-water, and the influence of rivers and conformation of coast upon these features. At the same time the entire range of the fauna and flora must be investigated in relation to small areas so as to connect the varying living inhabitants of different areas with the varying physical conditions of those areas and with the varying association of the living inhabitants *inter se*. Only in this way can the relation of food-fishes to the physical conditions of the sea, and to their living associates be ascertained and data furnished for ultimately determining the causes of the local distribution of different kinds of food-fishes and of the periodic migrations of some kinds of them.

(2) A thoroughly detailed and accurate knowledge of the food, habits, and movements of each of the important kinds of food-fishes (of which about five-and-twenty, together with six shell-fish important either as food or bait, may be reckoned). The relation of each of these kinds of fish to its fishing-ground must be separately ascertained; its time and mode of reproduction, the mode of fertilisation of its eggs, the growth of the embryo, the food and habits of the fry, the enemies of the young and of the adult, the relation of both young and adult to temperature, to influx of fresh water, to sewage contamination, to disturbing agencies such as trawling, and ordinary traffic.

(3) An inquiry as to whether over a long period of years there has been an increase or a decrease in the abundance of each kind of food-fish on the chief fishing-grounds as a matter of fact, together with an inquiry as to the actual take of each kind of fish in successive years, and further an inquiry as to any accompanying variation in (*a*) the number of fishing-boats; (*b*) the methods of fishing; (*c*) the climatic conditions or other such possibly influential conditions as previous inquiry may have suggested.

(4) An inquiry for the purpose of ascertaining experimentally whether the decrease in the yield of fishing-grounds, in regard to each several species of food-fish can be remedied: (*a*) by artificial breeding of the fish; (*b*) by protecting the young; (*c*) by increasing its natural food; (*d*) by destruction of its enemies; (*e*) by restrictive legislation as to time or place of fishing and as to size of fish which may be taken and character of fishing apparatus which may be used.

(5) An inquiry to ascertain whether, if periodic, natural causes are at work in determining the fluctuations of the yield of fishing-grounds, their effect can be foretold, and whether this effect can in any case be counteracted; similarly to ascertain in the case of migratory shoal-fish whether any simple and trustworthy means can be brought into operation for the purpose of foretelling the places and times of their migrations so as to enable both fishermen and fish-dealers to be ready for their arrival.

(6) An inquiry into the diseases of fish, especially in relation to salmon and other fresh-water fish.

B. *Practical Administration.*—The chief heads under which this presents itself as distinct from the antecedent search for reliable data are:—

(1) The management of an efficient "intelligence department," giving weekly statistics of the fishing industry, the appearance and disappearance of certain fish at particular spots, the number of fishing-boats employed, the methods of fishing employed, the meteorological conditions.

(2) The advising and enforcing of restrictions by the Legislature as to time, place, and method of capture of fish.

(3) The artificial breeding and rearing of fish to stock impoverished fishing-grounds.

(4) The leasing and management of the foreshore and sea-bottom in particular spots, for the purposes of oyster-culture and mussel-culture, and of marsh-lands near the sea for the formation of tanks and fish-ponds.

(5) The opening up of new fishing-grounds and of new fish industries (curing and treatment of fish for commercial purposes).

(6) The introduction of new species of food-fish and shell-fish.

III.—*General Organisation and Staff necessary to carry on the Inquiries and to put the Results attained into Practice*

It is a matter of fundamental importance to determine, first of all, whether it is desirable that these matters should be dealt with by a permanent staff, or, on the other hand, by the occasional employment of a scientific man—not habitually occupied in these inquiries—to attempt the solution of any particular problem which an unskilled official may present to him.

Clearly there must be economy in employing permanently certain naturalists who will familiarise themselves with this

special class of questions and become experts in all that relates to fishery problems.

Further, is it desirable that the matters which are to be inquired into should be determined by an official unskilled in natural history? Or, on the other hand, that the selection of inquiries likely to lead to a satisfactory result should be made by a man of science, specially conversant with the nature of the things to be dealt with?

The organisation required consists, so far as persons are concerned, of:—

(1) A chief scientific authority.

(2) A staff of working naturalist-inspectors.

(3) A staff of clerks.

And, so far as material is concerned, of:—

(4) A London office, with collection of fishes, apparatus used in fishing, maps, survey-records, statistical returns, and library.

(5) A surveying-ship, under the orders of the Department, to be manned and maintained by the Admiralty.

(6) A chief laboratory fitted for carrying on investigations such as those named in Section II., and also two smaller movable laboratories, together with steam yacht fitted for dredging and sounding.

(7) Hatching-stations and fish-ponds.

With regard to the foregoing headings, it is a matter for consideration whether "the chief scientific authority" should be an individual or a committee of five. The position assigned to this post should be equal to that of the Director of the Geological Survey or the Director of the Royal Gardens, Kew, or, if the "authority" takes the form of a committee, it should be placed on the same footing as the Meteorological Council. The person or persons so appointed should be responsible for all the operations of the Department, and of such scientific training and capacity as to be likely to devise the most useful lines of inquiry and administration.

The "naturalist-inspectors" should be six in number, but operations might be commenced with a smaller staff. They should be thoroughly competent observers, and under the direction of the chief scientific authority they would be variously employed, either on the surveying-ship, at the chief laboratory, or in local laboratories, hatching-stations, or in the London office and museum.

The naturalists thus employed would become specialists in all matters relating to the life-history of fishes and their food; they would acquire a skill and knowledge far beyond that which it is possible to find amongst existing naturalists, who occasionally are requested to make hurried reports on such matters as salmon disease or the supposed injury of the herring-fisheries by trawlers.

One of the naturalist-inspectors should be a chemist and physicist, in order to report on the composition of the water and the nature of the bottom in the areas investigated.

"Clerks" would be required in the London office to tabulate statistics and carry on correspondence. These gentlemen need not necessarily have any scientific knowledge. It would probably be necessary to have a correspondent or agent of the Department in every large fishing centre. Probably the coast-guard officials might be taken into this service.

With regard to material equipment it appears to be necessary that a Scientific Fisheries Department should have at its London office a Museum of fishing apparatus for reference and instruction, and also complete collections illustrative of the fishes, their food, enemies, and other surroundings. In the same building would be exhibited maps showing the distribution and migrations of food-fishes, the coast temperature and its variations, the varying character of the sea-bottom, sea-water, &c.

The surveying-ship or ships would be provided by the Admiralty.

A central laboratory is in course of erection upon Plymouth Sound by the Marine Biological Association. Her Majesty's Government has promised to contribute 5000*l.* and 500*l.* a year to this institution, on condition that its resources are available for the purpose here indicated. Certain of the "naturalist-inspectors" (probably three at any one time) would be stationed at the Plymouth laboratory in order to carry on special studies of the development and food of particular species of fish.

The smaller movable laboratories, steam-yacht, and other appliances would not be costly.

ON NEW APPLICATIONS OF THE MECHANICAL PROPERTIES OF CORK TO THE ARTS[1]

IT would seem difficult to discover any new properties in a substance so familiar as cork, and yet it possesses qualities which distinguish it from all other solid or liquid bodies, namely, its power of altering its volume in a very marked degree in consequence of change of pressure. All liquids and solids are capable of cubical compression, or extension, but to a very small extent; thus water is reduced in volume by only 1/2000 part by the pressure of one atmosphere. Liquid carbonic acid yields to pressure much more than any other fluid, but still the rate is very small. Solid substances, with the exception of cork, offer equally obstinate resistance to change of bulk; even india-rubber, which most people would suppose capable of very considerable change of volume, we shall find is really very rigid.

I have here an apparatus for applying pressure by means of a lever. I place a piece of solid india-rubber under the plate and you see that I can compress it considerably by a very light pressure of my finger. I slip this same piece of india-rubber into a brass tube, which it fits closely, and now you see that I am unable to compress it by any force which I can bring to bear. I even hammer the lever with a mallet, and the blow falls as it would on a stone. The reason of this phenomenon is, that in the first place, with the india-rubber free, it spread out laterally while being compressed longitudinally, and consequently the volume was hardly altered at all; in the second case, the strong brass tube prevented all lateral extension, and because india-rubber is incapable of appreciable cubical compression, its length only could not be sensibly altered by pressure.

Extension, in like manner, does not alter the volume of india-rubber. In this glass tube is a piece of solid round rubber which nearly fills the bore. The lower end of the rubber is fixed in the bottom of the tube, and the upper end is connected by a fine cord to a small windlass, by turning which I can stretch the rubber. I fill the tube to the brim with water, and throw an image of it on to the screen. If stretching the rubber either increases or diminishes its volume, the water in the tube will either overflow or shrink in it. I now stretch the rubber to about 3 inches, or one-third of its original length, but you cannot see any appreciable movement in the water-level, hence the volume of the rubber has not changed.

Metals when subjected to pressures which exceed their elastic limits, so that they are permanently deformed, as in forging or wire-drawing, remain practically unchanged in volume per unit of weight.

I have here a pair of common scales. To the under sides of the pans I can hang the various specimens that I wish to examine; underneath these are small beakers of water which I can raise or lower by means of a rack and pinion. Substances immersed in water lose in weight by the weight of their own volume of water; hence if two substances of equal volume balance each other in air, they will also balance when immersed in water, but if their volumes are not the same, then the substance having the smaller volume will sink, because the weight of water it displaces is less than that displaced by the substance with the larger volume. To the scale on your left hand is suspended a short cylinder of ordinary iron, and to the right-hand scale a cylinder of ordinary copper. They balance exactly. I now raise the beakers and immerse the two cylinders in water; you see the copper cylinder sinks at once, and I know by that that copper has a smaller volume per pound than iron, or, as we should commonly say, it is heavier than iron. I now detach the copper cylinder, and in its place hang on this iron one, which is made of the same bar as its fellow cylinder, but forced, while red hot, into a mould by a pressure of sixty tons per square inch and allowed to cool under that pressure. The two cylinders balance, as you see. Has the volume of the iron in the compressed cylinder been altered by the rough treatment it has received? I raise the beakers, immerse the cylinders, the balance is not destroyed; hence we conclude that although the form has been changed the volume has remained the same. I substitute for the hot compressed cylinder one pressed into a mould while cold, and held there for some time, with a load of sixty tons per square inch; the balance is not destroyed by immersion, hence the volume has not been altered. I can repeat the experiments with these copper cylinders and the

[1] A Paper read at the Royal Institution of Great Britain on April 9, 1886, by William Anderson, M.Inst.C.E., M.R.I.

result will be found the same. Extension also is incapable of appreciably altering the density of metals. I attach to the scales two specimens of iron taken from a bar which had been torn asunder by a steady pull. One specimen is cut from the portion where it had not been strained, and the other from the very point where it had been gradually drawn out and fractured. The specimens balance, I immerse them, you see the balance is not destroyed; hence the volume of the iron has not been changed appreciably by extension.

But cork behaves in a very different manner. I place this cylinder of cork into just such a brass tube as served to restrain the india-rubber and apply pressure to it in the same way; you see I can readily compress the cork, and when I release it it expands back to its original volume: the action is a little sluggish on account of the friction of the cork against the sides of the tube. In this case, therefore, a very great change in the volume of the material has been easily effected.

But although solids evidently do not change sensibly in bulk, after having been released from pressures high enough to distort them permanently, yet, while actually under pressure, the volumes may have been considerably altered. As far as I am aware, this point has not been determined experimentally for metals, but it is very easy to show that india-rubber does not change.

I have here some of this substance, which is so very slightly lighter than water, that, as you see, it only just floats in cold water but sinks in hot. If I could put it under considerable pressure while afloat in cold water, then, if its volume became sensibly less, it ought to sink. In the same way, if I load a piece of cork and a piece of wood so that they barely float, if their volumes alter they ought to sink.

In this strong upright glass tube I have, at the top, a piece of india-rubber, immediately below it a piece of wood, and below that a cork; the wood and the cork are loaded with metal sinkers to reduce their buoyancy. The tube is full of water and is connected to a force-pump by means of which I can impose a pressure of over 1000 lbs. per square inch. The image of the tube is now thrown on the screen and the pressure is being applied. You see at once the cork is beginning to shrink in all directions, and now its volume is so reduced that it is incapable of floating, and sinks down to the bottom of the tube. The india-rubber is absolutely unaffected, the wood does contract a little, but not sufficiently to be visible to you or to cause it to sink. I open a stop-cock and relieve the pressure; you see that the cork instantly expands, its buoyancy is restored, and it floats again. By alternately applying and taking off the pressure I can produce the familiar effect so well known in the toy called "the bottle imps." It is this singular property which gives to cork its value as a means of closing the mouths of bottles. Its elasticity has not only a very considerable range, but it is very persistent. Thus in the better kind of corks used in bottling champagne and other effervescing wines you are all familiar with the extent to which the corks expand the instant they escape from the bottles. I have measured this expansion, and find it to amount to an increase of volume of 75 per cent., even even after the corks have been kept in a state of compression in the bottles for ten years. If the cork be steeped in hot water, the volume continues to increase till it attains nearly three times that which it occupied in the neck of the bottle.

When cork is subjected to pressure, either in one direction, as in this lever press, or from every direction, as when immersed in water under pressure, a certain amount of permanent deformation or "permanent set" takes place very quickly. This property is common to all solid elastic substances when strained beyond their elastic limits, but with cork the limits are comparatively low. You have, no doubt, noticed in chemists' and other shops that, when a cork is too large to fit a bottle, the shopkeeper gives the cork a few sharp bites, or, if he be more refined, he uses a pair of specially-contrived pincers; in either case he squeezes the cork beyond its elastic limits, and so makes it permanently smaller. Besides the permanent set, there is a certain amount of what I venture to call sluggish elasticity, that is, cork on being released from pressure, springs back a certain amount at once, but the complete recovery takes an appreciable time.

While I have been speaking, a piece of fresh cork, loaded so as barely to float, has been inserted into the vertical glass pressure-tube. I apply a slight pressure, you see the cork sinks. I release the pressure, and it rises briskly enough. I now apply a much higher pressure for a moment or two, I release it, and the cork will either not rise at all, or will do so very slowly; its volume has been permanently altered; it has taken a permanent set.

In considering the properties of most substances, our search for the cause of these properties is baffled by our imperfect powers and the feeble instruments we possess for investigating molecular structure. With cork, happily, this is not the case; an examination of its structure is easy, and perfectly explains the cause of its peculiar and valuable properties.

All plants are built up of minute cells of various forms and dimensions. Their walls or sides are composed chiefly of a substance called cellulose, frequently associated with lignine, or woody matter, and with cork, which last is a nitrogenous substance found in many portions of plants, but is especially developed in the outer bark of exogenous trees, that is, trees belonging to an order, by far the most common in these latitudes, the stems of which grow by the addition of layers of fresh cellulose tissue outside the woody part and inside the bark. Between the bark and the wood is interposed a thin fibrous layer, which, in some trees, such as the lime, is very much developed, and supplies the bass matting with which all are familiar. The corky part of the bark, which is outside, is composed of closed cells exclusively, so built together that no connection of a tubular nature runs up and down the tree, although horizontal passages radiating towards the woody part of the tree are numerous. In the woody part of the tree, on the contrary, and in the inner bark, vertical passages or tubes exist, while a connection is kept up with the pith of the tree by means of medullary rays. In one species of tree, known as the cork oak, the corky part of the bark is very strongly developed. I project on the screen the magnified image of a horizontal section of the bark of the cork oak; you see nine or ten bands running parallel to each other: these are the layers of cellulose matter that have been deposited in successive years. I turn the specimen, and you now see the vertical section with the radiating passages clearly marked.

The difference between the arrangement of the cells or tissue forming the woody part of the tree and the bark is easily shown. I have here three metal sockets, supported over a shallow wooden tray. Into them are fitted, first, a cork cut out of the bark in a vertical direction, next, a cork cut in a radial direction, and, lastly, a piece of common yellow pine. By means of my force-pump, I apply a couple of atmospheres of hydraulic pressure. I project an image of the apparatus on the screen, and you see the water has made its way through the wood and through the cork cut in the radial direction, while the cork cut in the vertical direction is impervious.

The cork tree, a species of evergreen oak, is indigenous in Portugal and along both shores of the Mediterranean. The diagram on the wall has been painted from a sketch obligingly sent to me by Mr. C. A. Friend, the resident engineer of the Seville Waterworks, to whom I am also indebted for this branch of a cork tree, these acorns, this axe used in getting the cork, and for a description of the habits of the tree, its cultivation, and the mode of gathering the harvest.

The cork oak attains a height of 30 to 40 feet; it is not cultivated in any way, but grows like trees in a park. The first crop is not gathered till the tree is thirty years old, the next nine or ten years later; both these crops yield inferior cork, but at the third crop, gathered when the tree is fifty years old, the bark has attained full maturity, and after that will yield the highest quality of cork every nine or ten years. In the autumn of the year, when the bark is in a fit state, that is, for small trees, from three quarters of an inch to one inch thick, and for larger ones up to one inch and a half, a horizontal cut is made, by means of a light axe like the one I hold in my hand, through the bark a few inches above the ground; succeeding cuts are made at distances of about a yard, up to the branches, and even along some of the large ones, then two or more vertical cuts, according to the size of the tree, and the bark is ripped off by inserting the wedge-shaped end of the axe-handle. In making the cuts great care is taken to avoid wounding the inner bark, upon the integrity of which the health of the tree depends; but where this precaution is taken, the gathering of the cork does not in any way injure the tree.

After stripping, the cork is immersed for about an hour in hot water, it is dressed with a kind of spokeshave, then laid out flat and weighted in order to take out the curvature; it is then stacked in the open air, without protection of any kind, for cork does not appear to be susceptible of receiving injury from the weather.

The minute structure of the bark is very remarkable. First, I project on the screen a microscopic section of the wood of the cork tree. It is taken in a horizontal plane, and I ask you to notice the diversity of the structure, and especially the presence of large tubes or pipes. I next exhibit a section taken in the same plane of the corky portion of the bark. You see the whole substance is made up of minute many-sided cells about 1/750 of an inch in diameter, and about twice as long, the long way of the cells being disposed radially to the trunk. The walls of the cells are extremely thin, and yet they are wonderfully impervious to liquids. Looked at by reflected light, if the specimen be turned, bands of silvery light alternate with bands of comparative darkness, showing that the cells are built on end to end in regular order. The vertical section next exhibited shows a cross section of the cells looking like a minute honeycomb. In some specimens large numbers of crystals are found. These could not be distinguished from the detached elementary spindle-shaped cells, of which woody fibre is made up, were it not for the powerful means of analysis we have in polarised light. I need hardly explain to an audience in this Institution that light passed through a Nicol prism becomes polarised, that is to say, the vibrations of the luminiferous ether are all reduced to vibrations in one plane, and, consequently, if a second prism be interposed and placed at right angles to the first, the light will be unable to get through; but if we introduce between the crossed Nicols a substance capable of turning the plane of vibration again, then a certain portion of the light will pass. I have now projected on the screen the feeble light emerging from the crossed Nicols. I introduce the microscopic preparation of cork cells between them, and you see the crystals glowing with many-coloured lights on a dark ground.

Minute though these crystals are, they are very numerous and hard, and it is partly to them that is due the extraordinary rapidity with which cork blunts the cutting instruments used in shaping it. Cork-cutters always have beside them a sharpening-stone, on which they are obliged to restore the edges of their knives after a very few cuts.

The cells of the cork are filled with gaseous matter, which is very easily extracted, and which has been analysed for me by Mr. G. H. Ogston, and proved to be common air. I have here a glass tube in which are some pieces of cork which have been cut into slices so as to facilitate the escape of the air. I connect the tube with an exhausted receiver and project the image on the screen; you see rising from the cork bubbles of air as numerous, but much more minute than the bubbles which rise from sparkling wines; much more minute, because the bubbles you see are expanded to seven or eight times their volume at atmospheric pressure on account of the vacuum existing in the tube. The air will continue to come off for an hour or more, and from measurements made by Mr. Ogston I find that the air occluded in the cork amounts to about 53 per cent. of its volume. The facility with which the air escapes, compared with the impermeability of cork to liquids is very remarkable.

I throw on the screen the image of a section cut from a cork which was kept under a vacuum of about 26 inches for five days and nights; aniline dye was then injected, and yet you see that the colour has not more than permeated the outermost fringe of cells—those, in fact, which had been broken open by the operation of cutting the cork. By keeping cork for a very long time in an almost perfect vacuum, and then injecting dye, a slight darkening of the general colour of a section of the cork may be noticed, but it is very slight indeed. How, then, does the air escape so readily when the cork is placed *in vacuo?*

The answer is, that gases possess the property of diffusion; that is, of passing through porous media of inconceivable fineness. When two gases, such as hydrogen and air, are separated by a porous medium, they immediately begin to pass into each other, and the lighter gas passes through more quickly than the heavier.

I have here a glass tube, the upper end of which is closed by a thin slice of cork, the lower end dips into a basin of water. Some hours ago the tube was filled with hydrogen, which you know is about $14\frac{1}{2}$ times lighter than air; consequently, according to the law of diffusion, it will get out of the tube through the cork quicker than the air can get in by the same means, and the result must be that a partial vacuum will be formed in the tube, and a column of water will be drawn up. You see that such has been the case, and we have thus proved that the cells of cork are eminently pervious to gases. The pores in the cell-walls appear, however, to be too minute to permit the passage of liquids.

I closed the end of a glass tube 11 mm. diameter, with a disk of cork 1·75 mm. thick, cut at right angles to the axis of the tree; I placed a solution of blue litmus inside the tube, and suspended it in a weak solution of sulphuric acid. Had diffusion taken place, both liquids would have assumed a red colour, but after sixteen hours no change whatever could be detected. A like inertness was exhibited when the tube was filled with a solution of copper sulphate and suspended in a weak solution of ammonia; a deep blue colour would have appeared had any intermixture taken place, and the same tube is before you immersed in ammonia and filled with red litmus solution. It has been in this condition since February 28, but no diffusion has taken place. A disk of wood 6 mm. thick under the same circumstances showed, after a couple of hours, by the liquids turning blue, that diffusion was going on actively. It is this property of allowing gases to permeate while completely barring liquids that enables cork to be kept in compression under water or in contact with various liquids without the air-cells becoming water-logged, and that makes cork so admirable an article for waterproof wear, such as boot-soles and hats, for, unlike india-rubber, it allows ventilation to go on while it keeps out the wet. The cell-walls are so strong, notwithstanding their extreme thinness, that they appear, when empty, to be able to resist the atmospheric pressure, for the volume of the cork does not sensibly diminish, even when all the air has been extracted. Viewed under very high power, cross-stays or struts of fibrous matter may be distinguished traversing the cells: these, no doubt, add to the strength and resistance of the structure.

From what you have seen you will have no difficulty in arriving at the conclusion that cork consists, practically, of an aggregation of minute air-vessels, having very thin, very water-tight, and very strong walls, and hence, if compressed, we may expect the resistance to compression to rise in a manner more like the resistance of gases than the resistance of an elastic solid such as a spring. In a spring the pressure increases in proportion to the distance to which the spring is compressed, but with gases the pressure increases in a much more rapid manner; that is, inversely as the volume which the gas is made to occupy. But from the permeability of cork to air, it is evident that, if subjected to pressure in one direction only, it will gradually part with its occluded air by effusion, that is by its passage through the porous walls of the cells in which it is contained. This fact can be readily demonstrated by the lever press which I have used, for, if the brass cylinder containing the cork be filled with soap and water and pressure be then applied, minute bubbles will be found to collect on the surface, and their formation will go on for many hours.

On the other hand, if cork be subjected to pressure from all sides, such as operates when it is immersed in water under pressure, then the cells are supported in all directions, the air in them is reduced in volume, and there is no tendency to escape in one direction more than another. An india-rubber bag, such as this, distended by air, bursts, as you see, if pressed between two surfaces, but if an india-rubber cell be placed in a glass tube and subjected to hydraulic pressure, it is merely shrivelled up; the strain on its walls is actually reduced.

To take advantage of the peculiar properties of cork in mechanical applications, it is necessary to determine accurately the law of its resistance to compression, and for this purpose I instituted a series of experiments of this kind. Into a strong iron vessel of $5\frac{1}{2}$ gallons capacity I introduced a quantity of cork, and filled the interstices full of water, carefully getting out all the air. I then proceeded to pump in water, until definite pressures up to 1000 pounds per square inch had been reached, and, at every 100 pounds, the weight of water pumped in was determined. In this way, after many repetitions, I obtained the decrease of volume due to any given increase of pressure. The observations have been plotted into the form of a curve, which you see on the diagram on the wall. The base-line represents a cylinder containing one cubic foot of cork divided by the vertical lines into ten parts; the black horizontal lines according to the scale on the left hand represent the pressures in pounds per square inch which were necessary to compress the cork to the corresponding volume. Thus to reduce the volume to one-half, required a pressure of 250 pounds per square inch. At 1000 pounds per square inch the volume was reduced to 44 per cent.; the yielding then became very little, showing that the solid parts of the cells had nearly come together, and this corroborates Mr. Ogston's determination that the gaseous part of cork constitutes 53 per cent. of its bulk. The engineer, in dealing with a compressible substance, requires to know not only the pressure which a given

change of volume produces, but also the work which has to be expended in producing the change of volume. The work is calculated by multiplying the decrease of volume by the mean pressure per unit of area which produced it. The ordinates of the dotted curve on the diagram with the corresponding scale of foot-pounds on the right-hand side are drawn equal to the work done in compressing a cubic foot of cork to the several volumes marked on the base-line. I have not been able to find an equation to the pressure curve; it seems to be quite irregular, and hence the only way of calculating the effects of any given change of volume is to measure the ordinates of the curve constructed by actual experiment. As may be supposed the pressures indicated by experiment are not nearly so regular and steady as corresponding experiments on a gas would be, and the actual form of the curves will depend on the quality of the cork experimented on.

The last point of importance in this inquiry relates to the permanence of elasticity in cork.

So far as preservation of elasticity during years of compression is concerned, we have the evidence of wine corks to show that a considerable range of elasticity is retained for a very long time. With respect to cork subjected to repeated compression and extension, I have very little evidence to offer beyond this, that cork which had been compressed and released in water many thousand times had not changed its molecular structure in the least, and had continued perfectly serviceable. Cork which has been kept under a pressure of three atmospheres for many weeks appears to have shrunk to from 80 to 85 per cent. of its original volume.

I will conclude this lecture by bringing under your notice two novel applications of cork to the arts.

Before the lecture-table stands a water-raising apparatus called a hydraulic ram. The structure of the machine is shown by a diagram on the wall. The ram consists of an inclined pipe, which leads the water from a reservoir into a chamber which terminates in a valve opening inwards. Branching up from the chamber is a passage leading to a valve, opening outwards and communicating with a regulating vessel, which is usually filled with air, but which I prefer to fill with cork and water. Immediately beyond the inner valve is inserted a delivery pipe, which is laid to the spot to which the water has to be pumped, in this case to the fountain jet in the middle of this pan.

The action of the ram is as follows:—The outer valve, which opens inwards, is, in the first instance, held open, and a flow of water is allowed to take place through it down the pipe and chamber. The valve is then released, and is instantly shut by the current of water which is thus suddenly stopped, and, in consequence, delivers a blow similar to that produced by the fall of a hammer on an anvil, and just as the hammer jumps back from the anvil, so does the water recoil back to a small extent along the pipe.

During this action, first, a certain portion of water is forced by virtue of the blow through the inner valve, opening outwards, into the cork vessel, and so to the delivery pipe, and instantly afterwards the recoil causes a partial vacuum to form in the body of the ram, and permits the atmospheric pressure to open the outer valve and re-establish a rush of water as soon as the recoil has expended itself. In the little ram before you, this action, which it has taken so long to describe, is repeated 140 times in a minute.

The ram is now working. You hear the regular pulses of the valve, and you see a jet of water rising some 10 feet into the air. I throw the electric light on the water, and I ask you to notice the regularity of the flow. You can, indeed, detect the pulses of the ram in the fountain, but that is because I am only using a regulating vessel of the same capacity as that generally used for air, and you will recollect that 44 per cent. of the substance of cork is solid and inelastic. By closing a cock I can cut off the cork vessel from the ram; you see the regularity of the jet has disappeared, it now goes in leaps and bounds. This demonstrates that the elasticity of cork is competent to regulate the flow of water. When air is used for this purpose the air-vessel has to be filled, and, with most kinds of water, the supply has to be kept up while the ram is working, because water under pressure absorbs air. For this purpose a "sniff-valve" is a necessary part of all rams. It is a minute valve opening inwards, placed just below the inner valve; at each recoil a small bubble of air is drawn in and passed into the air-vessel. This "sniff-valve" is a fruitful source of trouble. Its minuteness renders it liable to get stopped up by dirt; it must not, of course, be submerged, and, if too large, it seriously affects the duty performed by the ram. The use of cork gets rid of all these difficulties, no sniff-valve is needed, the ram will work deeply submerged, and there is no fear of the cork vessel ever getting empty. The duty which even the little ram before you has done is 65 per cent., and larger ones have reached 80 per cent.

The second novel application of cork is for the purpose of storing a portion of the energy of the recoil of cannon, for the purpose of expending it afterwards in running them out.

The result of the explosion of gunpowder in a gun is to drive the shot out in one direction, and to cause the gun to recoil with equal energy the opposite way. To restrain the motion of the gun "compressors" of various kinds are used, and in this country, for modern guns, they are generally hydraulic, that is to say, the force of recoil is expended in causing the gun to mount an inclined plane, and, at the same time, in driving a piston into a cylinder full of water, the latter being allowed to squeeze past the piston through apertures, the areas of which are either fixed or capable of being automatically varied as the gun recedes; or else the water is driven out of the cylinder through loaded valves. As a rule, the gun is moved out again into its firing position by its weight causing it to run down the inclined plane, up which it had previously recoiled. For naval purposes, however, this plan is inconvenient, because the gun will not run out to windward if the vessel is heeling over, on account of the inclined plane becoming more horizontal, or even inclined in the reverse direction, and should the ship take a permanent list, from a compartment getting full of water, the inconvenience might be very considerable.

In land service guns, when mounted in barbette, the rising of the gun exposes it and the loading detachment more to the enemy's fire, and in both cases, when placed in ports or embrasures, the ports must be higher than if the gun recoiled horizontally, and will therefore offer a better mark to the enemy's fire, especially that of machine guns, while the sudden rise of the gun in recoiling imposes a severe downward pressure on the deck or on the platform.

To obviate these disadvantages I have contrived the gun-carriage a model of which is before you on the table, and a diagram of which on the wall illustrates the internal construction. The gun is mounted on a carriage composed of two hydraulic cylinders, united so as to form one piece. The carriage slides on a pair of hollow ways, and also on to a pair of fixed rams, the rear ends of which are attached to the piece forming the rear of the mounting. There are water passages down the axes of the rams, and these communicate through an automatic recoil-valve, opening from the cylinders, with the two hollow slides. There is a second communication between the cylinders and slides by means of a cock, which can be opened or shut at pleasure. The hollow slides are packed full of cork and water, the latter also completely filling the cylinders, rams, and various connecting passages.

By means of a small force-pump enough water can be injected to give the cork so much initial compression as will suffice to run the gun out when the slides are inclined under any angle which may be found convenient.

When the gun is fired, the cylinders are driven on to the rams, and the water in the cylinders is forced through the hollow rams into the cork and water vessels formed by the slides, and the cork is compressed still farther. When the recoil is over, the automatic recoil-valve closes, and the gun remains in its rearward position ready for loading.

As soon as loaded, the running-out cock is opened, the expansion of the cork drives the water from around it into the cylinders, and so forces the gun out.

If it be desired to let the gun run out automatically immediately after recoil, it is only necessary to leave the running-out cock open, and then the water forced among the cork by recoil returns instantly to the cylinders, and runs the gun out quicker than the eye can follow the motion.

I will now load the model and fire a shot into this strong steel cylinder, at the bottom of which is a thick layer of soft wood. I will close the running-out valve, so that the gun shall remain in the recoiled position. Sir Frederick Abel has kindly arranged some of his electric fuses specially to fit this minute ordnance, and I can fire the gun by means of a small electro-magnetic battery. The gun has now recoiled, and remains in its rear position. I load again, open the running-out cock, the gun runs out, and I fire without closing the cock. You see the gun has recoiled and run out instantly again.

The arrangement I have adopted may be made by using air

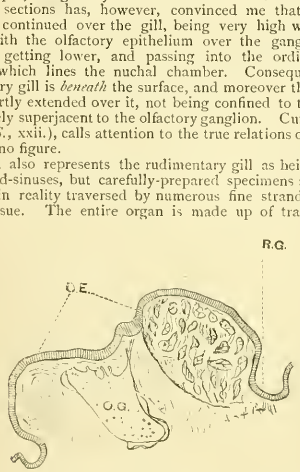
instead of cork, but air is a troublesome substance to deal with; it leaks out very easily, and without showing any signs of having done so, which might readily lead to serious consequences. A special pump is required to make up loss by leakage.

The merit of cork is its extreme simplicity and trustworthiness. By mixing a certain proportion of glycerine with the water it will not freeze in any ordinary cold weather.

NOTE ON THE RUDIMENTARY GILLS, ETC., OF THE COMMON LIMPET (PATELLA VULGATA)

SPENGEL, in his admirable paper "Die Geruchsorgane und das Nervensystem der Mollusken" (*Zeitschrift f. wiss. Zool.* xxxv.), figures a transverse section of one of the rudimentary gills and its surroundings. This appears to be incorrect in one or two particulars. In the first place the gill is figured as projecting freely at the surface. The examination of numerous sections has, however, convinced me that the epithelium is continued over the gill, being very high where continuous with the olfactory epithelium over the ganglion, but gradually getting lower, and passing into the ordinary epithelium, which lines the nuchal chamber. Consequently the rudimentary gill is *beneath* the surface, and moreover the sensory tract is partly extended over it, not being confined to the region immediately superjacent to the olfactory ganglion. Cunningham (*Q.J.M.S.*, xxii.), calls attention to the true relations of the gill, but gives no figure.

Spengel also represents the rudimentary gill as being full of large blood-sinuses, but carefully-prepared specimens show that these are in reality traversed by numerous fine strands of connective-tissue. The entire organ is made up of trabeculæ of



Transverse Section of Rudimentary Gill, &c., of *Patella vulgata* (× 90). R.G. Rudimentary gill; O.E. olfactory epithelium; O.G. olfactory ganglion.

connective-tissue, amongst which connective-tissue corpuscles abound. In some of the lacunæ masses of blood-corpuscles may be found.

Several small nerves run from the olfactory ganglion to the olfactory epithelium, and in some specimens nerve-fibres can almost be traced into the sense-cells. Gibson ("Anatomy of *Patella vulgata*," *Trans. R.S.E.*, xxxii.) has been unable to detect an olfactory ganglion. This is, however, very evident in microscopic sections.

I have used the term "rudimentary gills," for there seems little doubt that the structures in question are, as Spengel advocates, of this nature, but, lying as they do beneath the surface, they can hardly be functional. This position, too, suggests that these organs must have been rudimentary for a very long time. As Patella (Palacmæa) occurs in the fossil state as far back as the Middle Cambrian (Sedg.), the pallial gills may have been developed for a considerable period.

If, as Spengel believes, the molluscan olfactory organ enables the animals of that group to perceive the quality of the water passing over the gills, it is difficult to understand its well-developed state in Patella, where its position would appear to prevent such a use. Hence the olfactory organ in this form probably has some other function—possibly it may have something to do with the locality-sense, though this is very improbable (see note by author on "The Habits of the Limpet," NATURE, vol. xxxi. p. 200). The preceding observations were made at the Scottish Marine Station.

J. R. AINSWORTH DAVIS

University College, Aberystwyth

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—At the annual Scholarship election at St. John's College the following awards were made to students of Mathematics and Natural Science:—Hutchinson Studentship of 60*l.* a year for two years to A. C. Seward (First Class Nat. Sciences Tripos, Part II.), to enable him to follow up his researches in Fossil Botany; Hockin Prize for Physics with especial reference to Electricity, to Stroud (First Class Nat. Sciences Tripos, Part II.); Herschel Prize for Astronomy to Fletcher (Second Wrangler); Hughes Prizes for Mathematics to Fletcher, and for Natural Science to Rolleston (First Class Nat. Sciences Tripos, Part II.); Wright's Prizes for Mathematics to Baker and Orr, for Natural Science to Lake and Groom; Foundation Scholarships in Mathematics to Middlemast, Pressland, Tate, Bradford, Flux, and in Natural Science to Lake and W. Harris; extension of tenure of Scholarships to Kirby, Mossop, Bushe-Fox, and Baker in Mathematics, and to Shore and Turpin in Natural Science; Exhibitions in Mathematics to Hill, Fletcher, A. E. Foster, Norris, Varley, H. H. Harris, Orr, Greenidge, Flux, Card, Palmer, Millard, and in Natural Science to Lake, Groom, Rolleston, Seward, W. Harris; a Proper Sizarship in Natural Science to Cowell.

The following gentlemen have obtained first-class honours in the Natural Sciences Tripos, Part II., the subject for which they were specially classed being given after the name:—Carnegie, Chemistry, Caius; Edkins, Physiology, Caius; Hawkridge, Geology, Clare; Hudson, Physics, Pembroke; F. W. Oliver, Botany, Trinity; Rolleston, Human Anatomy with Physiology, St. John's; Seward, Geology, St. John's; Skinner, Chemistry, Christ's; Stroud, Physics, St. John's. Miss Freund, of Girton, was placed in the first class for Chemistry.

Messrs. Dixon, of Trinity College, and Fletcher, of St. John's, are respectively Senior and Second Wranglers. Both were educated at New Kingswood School, Bath, under Mr. T. G. Osborn. Miss Frost, of Newnham College, was placed between the 24th and 25th Wranglers.

In a recent discussion Prof. Stuart stated that 58 students attended the engineering courses and workshops in the Lent Term. Of these 32 were to be engineers; 7 were to engage in manufactures in which a knowledge of engineering was desirable; 3 were going into the army; 2 were to become teachers. As to their University position, 9 were M.A. or B.A., 21 were ready for the Mathematical Tripos, 2 for the Natural Sciences Tripos, 18 for the Special Examinations in Applied Science; 6 had only come to the University for a year's work in the workshops; 5 were not matriculated students.

DR. ORME MASSON, a graduate of Edinburgh University, and lately Elective Fellow in Chemistry, has been appointed to the Chair of Chemistry at Melbourne, Australia.

SCIENTIFIC SERIALS

Bulletins de la Société d'Anthropologie de Paris, tome ix., fasc. 1, 1886.—The present number gives the usual annual recapitulation of the rules of the Society, the lists of members, addresses by the outgoing and incoming presidents, financial and other reports, &c.—M. Moncelon laid before the Society a *résumé* of the principal results of his observations on the half-castes of New Caledonia during his residence in the colony. He drew attention to the evils resulting from the practice commonly followed by the native mothers of half-castes, of going back with their children to their native tribes, amongst whom these half-whites grow up in slavery as savages.—On certain Hova and Sakalava skulls, by M. Trucy. Both of these cranial groups are dolichocephalic, with an index of about 74, which is nearly the same as that of the Arabs of Algiers and the pariahs of Bengal. The Hovas and Sakalavas appear to be more intelligent than any other tribes of Madagascar, but while the Sakalava queen, the ally of France, submitted with her husband to be made the subject of careful anthropometrical observations, she enjoined upon the French officers to punish with death any one who opened or rifled a grave. It was consequently only by artifice and extreme circumspection that M. Trucy was able to obtain crania or other human bones. In the discussion which followed, regarding the mixed characters of the Hova crania, MM. Topinard, Dally, and others entered warmly into the question of typical and other distinctions of race.—On the development, in the adult, of supernumerary digits, by M. Fauvelle

This paper, which supplies some suggestive and not uninteresting matter, is based upon observations on the abnormal development in a full-grown axolotl of a fifth digit at the base of the fourth, and the gradual reparation, by multiplication of the parts, of various injuries to the other phalanges. Dr. Fauvelle considers at length the conditions on which the formation of supplementary parts in the adult may possibly depend, and whether we may not refer such abnormal manifestations of activity to a reversion in the cells of the connective-tissues to an embryonic condition, in which segmentation is possible. M. Avia, in confirming the views of Dr. Fauvelle as to the influence of heredity in the human species on the appearance of supernumerary organs, instanced the family of the Fodli, which for several centuries had exercised patriarchal supremacy over a tribe of the Arab Hyamites. In this family, whose members are not allowed to marry beyond the limits of their own kindred, polydactylism has become an established hereditary character, and is considered as an indispensable evidence of legitimacy, and right of succession. M. Avia has personally examined various Fodli, all of whom had twenty-four phalanges on their hands and feet.—On heredity, by Dr. Fauvelle. In this, as in the preceding paper, the author draws attention to the injury done to scientific inquiry by the constantly increasing recklessness with which physiological and anatomical conditions, whose causes we are ignorant of, are indiscriminately referred to so-called "atavism." It must be confessed, however, that the author himself in his exposition of the significance of the phenomena of heredity, as given in this paper, and in his more recent communication to the Society of his views regarding the real or assumed existence of atavism, exhibits the same want of accuracy and close definition which he condemns in others, and the vagueness of the opinions which he has enunciated with such dogmatic temerity excited a lively controversy, in which MM. Laborde and Sanson, and Mme. Cl. Royer, with other members, took part.—On primitive forms of numerations, by M. Letourneau. In this paper, and in the discussion which followed its reading, attention was drawn to the development, among some peoples, of a decimal system of numeration from the natural counting of the fingers, while according to Bachofen and others, the decimal method was preceded, in those earlier periods of civilisation in which the matriarchal principle was still in force, by the *octomal* system. Curious evidence of the prevalence of this practice of counting by 8 is afforded in Sanskrit, and in Greek and Latin, as well as in several modern European forms of speech, by the close affinity, if not identity, of the words signifying nine and new, as, *e.g.*, the French "neuf," thus showing that the numeral following eight was of more recent acceptation than the final term of the octomal form of numeration.

Bulletin de l'Académie des Sciences de St. Pétersbourg, vol. xxi. No. 1.—List of the members of the Academy on March 1, 1886.—Diagnoses of new plants from Asia, by C. J. Maximowicz, part 6 (Latin), containing a good many new species.—Report on new linguistic materials contained in the "Codex Comanicus," by Prof. Radloff (German).

SOCIETIES AND ACADEMIES

London

Royal Society, June 10.—"A Minute Analysis (experimental) of the various Movements produced by stimulating in the Monkey different Regions of the Cortical Centre for the Upper Limb, as defined by Prof. Ferrier." By Charles E. Beevor, M.D., M.R.C.P., and Prof. Victor Horsley, F.R.S., B.S., F.R.C.S.

The following investigation was undertaken as prefatory to a research into motor localisation of the spinal cord.

Anatomy.—(1) Attention is drawn to some minute details of the topographical anatomy of the upper limb centres as defined by Prof. Ferrier.

(2) Outlines of the shape and arrangement of the fissure of Rolando, the præcentral and intra-parietal sulci.

(3) Proof adduced in support of the authors' opinion that the small horizontal sulcus named X by Prof. Schäfer really corresponds to the superior frontal sulcus of man.

Previous Researches.—Ferrier's results are then given in full.

Method of Experimentation is explained in detail, as also the mode of subdivision of the part of the cortex investigated into centres of about 2 mm. square.

From the results of excitation are then deduced the two following axioms:—

Axiom I.—Viewing as a whole the motor area of the central cortex for the upper limb, as defined by Prof. Ferrier, we find that the regions for the action of the larger joints are situated at the upper part of the area, *i.e.* closer to the middle line, while those for the smaller and more differentiated movements lie peripherally at the lower part of the area.

Axiom II.—As a broad result, extension of the joints is the most characteristic movement of the upper part of Ferrier's arm centre; while flexion is equally characteristic of the movements obtained by stimulating the lower part. Finally, between these two regions there is a small portion where flexion and extension alternately predominate, a condition to which we have given the name of *confusion*. (Here both flexors and extensors are contracting at the same time, and consequently the joint is usually fixed in a neutral position, each group of muscles alternately drawing it in opposite directions.)

Priority of Movements is found to take place, and follows the "march" first indicated by Dr. Hughlings Jackson as existing in epileptic seizures.

This *march* is in accordance with Axiom I., since the shoulder commences the series of movements in the uppermost part of the area, the thumb at the lowest part, and the wrist in the intermediate part.

Summary.—(1) That X is the superior frontal sulcus of man.

(2) That the movements of the joints are progressively represented in the cortex from above down.

(3) Localisation of sequence of movements.

(4) Localisation of quality of movements.

(5) That there is no absolute line of demarcation between the different centres.

Mathematical Society, June 10.—J. W. L. Glaisher, F.R.S., President, in the chair.—At a special meeting the following resolution was unanimously carried:—"That the Council be empowered to take the necessary steps to obtain a charter of incorporation for the Society."—At the ordinary meeting Messrs. A. R. Forsyth, F.R.S., R. Lachlan, and the Rev. J. J. Milne were admitted into the Society.—The following communications were made:—Reciprocation in statics, by Prof. Genese.—On the theory of screws in elliptic space (third note), by A. Buchheim.—Some applications of Weierstrass's elliptic functions, by Prof. Greenhill.—Formula for the interchange of the independent and dependent variables with some applications to reciprocants, by C. Leudesdorf (second paper on reciprocants), by L. J. Rogers.—On the motion of a liquid ellipsoid under the influence of its own attraction, by A. B. Basset.—Electrical oscillations on cylindrical conductors, by Prof. J. J. Thomson, F.R.S.

Chemical Society, May 20.—Dr. Hugo Müller, F.R.S., President, in the chair.—The following papers were read:—Sources of error in the calorimetric study of salts, by Prof. W. A. Tilden, F.R.S.—On the action of aldehydes and ammonia on benzil, by Francis R. Japp, F.R.S., and W. Palmer Wynne, B.Sc.—On imabenzil, by the same.—On ammonia-derivatives of benzoin, by Francis R. Japp, F.R.S., and W. H. Wilson, Ph.D.—On compounds from benzil and benzoïn and alcohols, by Francis R. Japp, F.R.S., and Julius Raschen.—On the action of phosphoric sulphide on benzophenone, by the same.—The separation and estimation of zirconium by means of hydrogen peroxide, by G. H. Bailey, D.Sc., Ph.D.—An apparatus for the determination of the temperature of decomposition of salts, by the same.—The retention of lead salts by filter-paper, by L. Trant O'Shea.

June 3.—Dr. Hugo Muller, F.R.S., President, in the chair.—The following papers were read:—Notes on Sir W. Fairbairn's experiments on re-melting cast iron, by Thomas Turner, Assoc. R.S.M.—Some ammonium compounds and other derivatives of α-1′ hydroxyquinoline, by C. A. Kohn, B.Sc., Ph.D.—β-sulphophthalic acid, by Prof C. Graebe and A. Rée, Ph.D.—Compounds obtained by the aid of β-sulphophthalic acid, by the same.—Derivatives of taurine (part 2), by J. William James.

Anthropological Institute, June 8.—Mr. Francis Galton, F.R.S., President, in the chair.—The election of Mr. Joseph J. Mooney was announced.—Mr. C. H. Read read a paper on the ethnological exhibits in the Colonial and Indian Exhibition, in which he reviewed briefly the collections to be seen in the various courts, and described in detail some of the objects. The author dwelt especially upon the meagreness of the collection sent from the Dominion of Canada, where there is such a vast

field for ethnological inquiry.—Miss Buckland read a paper on American shell-work and its affinities, in which it was pointed out that the resemblance in shell ornaments found in mounds in various States of North America to those existing in the Solodmon and Admiralty Islands renders it highly probable that a commerce was carried on between the islands of the Pacific anthe American continent prior to the Spanish conquest.—A paper by Mr. C. W. Rosset, on the Maldive Islands, was read. The group contains upwards of 12,000 islands, which lie in clusters called atolls, of which there are more than twenty. The king's or sultan's island is situated in Malé Atoll, and here Mr. Rosset spent seventy days, as the sultan would not allow him to visit the other atolls. The natives live almost entirely upon fish and rice; and as the islands are not capable of producing grain of any kind, the rice has to be imported from India, the nearest point of which is about 350 miles distant. The author gave an interesting description of the customs of the natives, and exhibited a large collection of photographs, dresses, and other objects of ethnological interest.

Royal Meteorological Society, June 16.—Mr. W. Ellis, F.R.A.S., President, in the chair.—The Rev. J. R. Boyle and Mr. H. B. de la Poer Wall, M.A., were elected Fellows of the Society.—The following papers were read:—Note on a sudden squall, January 13, 1886, by Mr. R. H. Scott, F.R.S. This is an account of a remarkably sudden squall of about ten minutes' duration, which passed over the south of England on the morning of January 13. It was first recorded at Falmouth at 8.20 a.m., and passed over London at 10.40 a.m.—The floods of May 1886, by Mr. F. Gaster, F.R.Met.Soc., and Mr. W. Marriott, F.R.Met.Soc. The month of May 1886 will long be remembered for the heavy rains that occurred between the 11th and 13th, and the floods they produced over the greater part of the west and midland counties of England. In fact, at Worcester the flood was higher than any that have occurred there since 1770. On the 11th and 12th heavy rain fell over the east of England, there being over 3 inches during these two days at several places in counties Down, Dublin, and Wexford; the greatest reported being 3·52 inches at Kilkeel, co. Down. Over the other parts of the United Kingdom the rainfall on the 11th was under 1 inch. Rain, however, commenced falling about noon on Tuesday over the midland counties, and continued with increasing intensity till Friday morning; the duration at most places being about sixty hours. The heaviest rainfall occurred in Shropshire, where over 6 inches fell at several stations; while at Burwarton as much as 7·09 inches was recorded, the amounts for each day being 0·60 inches on the 11th, 3·10 inches on the 12th, and 3·39 inches on the 13th Very serious floods followed these heavy rains. At Shrewsbury the extreme height of the flood on the Severn was 16 feet, and at Worcester 17 feet 1 inch, above the average summer level. At Ross the flood on the Wye was 14 feet; at Nottingham the rise of the water in the Trent was 12¼ feet; at Rotherham the flood was 8 feet 5 inches; and in North-East Yorkshire the Derwent rose to nearly 11 feet above summer level. These floods caused great damage to property and loss of life; bridges were washed away; railway traffic suspended; and thousands of workmen thrown idle. In several places the waterworks were flooded, and the towns' water-supply was consequently cut off. Mr. Gaster drew attention to the complex character of pressure-distribution during the time referred to, and showed how the region of maximum rainfall followed certain of the shallow depressions which appeared over the British Islands. He drew attention to the peculiarities of this type of depression, showing how in many, if not in most, cases the rainfall was heaviest in their rear, and was brought by the easterly, not by the westerly, wind. He also referred to some previous instances of heavy floods, in which similar atmospheric conditions prevailed, and explained how it was that, as the disturbance passed off, snow fell instead of rain, this in its turn being followed by severe cold and in some places frost.—On atmospheric pressure and its effect on the tidal wave, by Capt. W. N. Greenwood, F.R.Met.Soc. The object of this paper is to show how a little knowledge of weather-forecasting, with some practical knowledge of local weather changes and a good barometer, will go far towards forming a right correction for application to the predicted height of the tide, and also to determine what that correction should be in its relation to the fluctuations of the barometer and the prevailing gradient.—Meteorological results at Levuka and Suva, 1875-85, with notes on the climate of Fiji, by Mr. J. D. W. Vaughan, F.R.Met.Soc. The climate of Fiji is remarkably healthy. Diseases such as fevers of an aggravated and malarious character, cholera, and liver complaints are unknown.

EDINBURGH

Royal Society, June 7.—The Hon. Lord Maclaren, Vice-President, in the chair.—Dr. H. R. Mill and Mr. J. T. Morison, of the Scottish Marine Station, read a paper on tidal variations of salinity and temperature in the estuary of the Forth. They divide a river-system into four parts: (1) the *river proper* with its tributaries and feeding-lakes, in the whole of which the water is fresh; (2) the *estuary*, in which the river-water meets that of the firth or sea, and in which there is rapid change of salinity with position and great tidal differences; (3) the *firth* or sea-inlet, in which there is a very uniform and gradual increase of salinity from estuary to sea; (4) the *sea proper* adjacent to the mouth of the firth. It was shown that the temperature of the river in spring and summer being higher than that of the firth, and in consequence surface-water being warmer as well as fresher than bottom-water, the curves representing vertical distribution of salinity and of temperature were identical. Hence the interaction of river and firth waters can be studied as completely by the thermometer as by the hydrometer. In the estuary of the Forth translational motion of the whole mass of water is found to characterise both flood and ebb tide, but about the times of high and low water considerable shearing motion takes place. So long as no shearing occurs, the water is of nearly uniform salinity from surface to bottom at any given time.—Mr. J. J. Barlow communicated a paper on a new method and reagents for detecting chlorides, bromides, and iodides in the presence of each other, and also in the presence of nitrates and chlorates.—Mr. J. A. Thomson gave a paper on the anatomy of *Suberites domuncula*, and also, in conjunction with Mr. P. Geddes, a paper on the history and theory of spermatogenesis.—Dr. J. Waddell gave an account of experiments by which he has determined the atomic weight of tungsten. The methods he used are superior to those previously employed.—Mr. A. H. Anglin discussed certain theorems mainly connected with alternants.

PARIS

Academy of Sciences, June 15.—M. Jurien de la Gravière, President, in the chair.—On the earthquake which occurred in Brazil on May 9, extract from a letter of H.M. dom Pedro d'Alcantara. This disturbance, the first on record, took place in the Petropolis district on May 9 at 3.20 p.m. The vibration, which was of a mild character, lasted scarcely four seconds, and was also felt along the coast as far as Rio de Janeiro, and inland 266 kilometres from that point. It was accompanied by exceptional cold weather, the glass falling to -5° C. in some parts of the province of Minas Geraes, and -3° in other places.—On the absorption spectra of oxygen, by M. J. Janssen. In continuation of his studies on the absorption spectra of the gases, the author deals here with those of oxygen, which reveals some features of great interest for molecular mechanics.—Remarks on the decomposition of the sal ammoniacs by the bases and metallic oxides, by M. Berthelot.—On the ammonia present in the ground (third note), by M. Th. Schlœsing, in reply to MM. Berthelot and André. The question is discussed whether the quantity of ammonia present in vegetable soil is, as a rule, comprised between 0 mg. and 20 mg., as determined by the author, M. Boussingault, and other analysts, or whether this quantity ranges from 78 mg. to 118 mg., as determined by MM. Berthelot and André. It is pointed out that the difference between the two views is a question of quantity; and as the quantity depends on the process of analysis by which it is determined, it ultimately resolves itself into a question of analytical processes.—Lavoisier and the Commission on Weights and Measures, by M. E. Grimaux. Some unpublished documents are printed, showing the action taken by the Commission on behalf of Lavoisier, at that time under arrest as a farmer-general. From one of these documents it appears that, in consequence of said action, the illustrious names of Laplace, Delambre, Borda, and others, were themselves removed from the Commission on the 3rd Nivose of the second year of the Republic (December 26, 1793).—Observations on Fabry's comet, by M. L. Cruls. The spectral analysis made at Rio de Janeiro during the month of May with a spectroscope of slight dispersive power showed distinctly the three bands characteristic of carbon compounds.—Comparative dimensions of the satellites of Jupiter, deduced from observations made during the year 1885, by Dom Lamey.

For the four satellites these observations yielded for the vertical diameters at mean distance the following angular dimensions:—

I.	$1''\cdot176 \pm 0''\cdot360$
II.	$1\cdot281 \pm 0\cdot392$
III.	$1\cdot725 \pm 0\cdot436$
IV.	$1\cdot286 \pm 0\cdot447$

—Note on the herpolhodie (second communication), by M. Hess.—On the measurement of the specific volume of the saturated vapours, and on the value of the mechanical equivalent of heat, by M. A. Perot. Reversing the well-known process of Messrs. Fairbairn and Tait for determining the volume of a known mass of saturated vapour at a given temperature, the author finds the number expressing the mechanical equivalent of heat to be about 424.—Note on a registering hygrometer, by M. Alb. Nodon. This instrument, which is constructed on a principle analogous to that of Breguet's metallic thermometer, is contrived to work for ten consecutive days. Its indications are unaffected by a temperature ranging from 10° to 35° C.—Law determining the electric conductivity of saline solutions of mean concentration, by M. E. Bouty.—Relation between the coefficient of self-induction and the magnetic action of an electro-magnet, by M. Ledeboer.—New magnetic maps of France, by M. Th. Moureaux. The observations, which have served as the groundwork of these charts, were mostly made during the years 1884 and 1885 under the direction of M. Mascart, at seventy-eight stations in every part of France, the results being all referred to January 1, 1885, by comparison with the curves of variation as determined with the magnetograph at the Observatory of the Parc Saint-Maur. From these observations the declination is shown to be least at Belfort (13° 59'·8), greatest at Conquet (19° 25'·1), varying in the north of France about 30' for a degree of longitude, and less in the south.—Summer isobars, winds, and cloudiness on the Atlantic, by M. L. Teisserenc de Bort. The maps embodying these data are based on the records of English and Dutch vessels, comprising 40,900 observations for each element, and on a report on the equatorial region published by the Meteorological Office.—Note on the earthquake in Brazil, by M. Cruls. Although traces of ancient volcanoes and more recent eruptive formations have been detected on the seaboard of Rio de Janeiro, the author considers that this seismic disturbance was not volcanic, but due to shrinking or some analogous movement of frequent occurrence in the crust of the earth.—Reply to some objections made to the memoir on microseismic observations, by M. T. Bertelli.—On the penta-sulphuret of phosphorus, by M. F. Isambert.—On the principle of equivalence in the phenomena of chemical equilibria, by M. H. Le Chatelier. The experimental law serving as the base of pure mechanics—two forces equal to a third are equal to each other, and reciprocally—is true also of chemical equilibria. But in order to eliminate the equivocal notion of *force*, the author substitutes for it another thus formulated: in every phenomenon of equilibrium two material systems equivalent in relation to a third will also remain equivalent in relation to any other system to which they may be opposed, and they are in mutual equilibrium when opposed to each other. This law is here verified in the case of vaporisation, dissociation, solubility, and under other conditions.—On monosodic orthophosphate and arseniate, by MM. A. Joly and H. Dufet.—On a combination of methylic alcohol and anhydrous baryta, by M. de Forcrand.—On the monochloracetate of butyl, by M. G. Gehring. In order to complete the series of monochloracetates, the author has prepared, and determined some of the physical properties of, this substance, adopting the same general method as that employed in the preparation of the monochloracetate of methyl.—On the development of the œsophagus, by M. P. de Meuron.—On the vascular system of *Dorocidaris papillata*, by M. H. Prouho.—On the crystals of gypsum in the pseudo-potters' clays of the Paris district, by M. Stan. Meunier.—Preliminary note on the geological structure of the Lure range, Lower Alps, by M. W. Kilian. This range, which runs for 50 kilometres from the neighbourhood of Vilhosc to Monbrun (Vaucluse), appears to be intermediate between the Alpine and Pyrenean systems. A summary is given of its geological constituents, ranging from the Middle and Upper Jurassic to the Tertiary conglomerates and marls.—On the male fertilisations of Arthropitus and Bornia, by M. Renault.—A contribution to the study of pre-foliation and pre-efflorescence in fossil plants, by M. L. Crié.—Remarks on a meteor observed at the Trocadéro on June 13, by M. L. Jaubert.

STOCKHOLM

Academy of Sciences, June 9.—On the Academy's Zoological Station in the province of Bohus, by Prof. Sven Lovén.—On the resistance of mixtures of acids against electrical conductibility, by Dr. S. Arrhenius.—A collection of ethnographical objects of Central American Indians, presented to the National Museum by the Swedish Consul-General in Guatemala, Mr. S. Ascoli, exhibited and explained by Prof. F. A. Smith.—On the new elementary body germanium, and some of its combinations, by Prof. L. Fr. Nilsson. The researches of Profs. Nilsson and Petterson, made at the request of Prof. Winkler, the discoverer of germanium, show that his suggestion that germanium might possibly be identical with Mendelejeff's ekasilicium is quite correct, and in accordance with the true facts.—Methods for the determination of elements of refraction in prisms having great refracting angles, by Mr. W. Ramsay.—On the mode of occurrence of the sand-worn stones in the Cambrian strata at Lugnås, in Sweden, by Prof. A. G. Nathorst.

BOOKS AND PAMPHLETS RECEIVED

"The First Report upon the Fauna of Liverpool Bay and the Neighbouring Seas," Edited by Prof. Herdman (Longmans).—"L'Inclinaison des Vents" R. P. Marc Dechevrens (Chang Hai).—"Die Alchemie in Alterer und Neuerer Zeit," Erster und Zweiter Theil, by H. Kopp (Winter, Heidelberg).—"An Elementary Treatise on Geometrical Optics," 2nd edition, revised, by W. S. Aldis (Deighton, Bell, and Co.).—"Records of the Saidapet Experimental Farm," by Chas. Benson (Keys, Madras).—"New Commercial Plants and Drugs," No. 9, by T. Christy (Christy).—"Bulletin of the Illinois State Laboratory of Natural History," Vol. II., Art. V. "Studies from the Contagious Diseases of Insects," by S. A Forbes (Franks, Peoria).—"Cornell University: Proceedings in Memory of Louis Agassiz and in Honour of Hiram Sibley, June 17, 1885."

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THURSDAY, JULY 1, 1886

KEPLER'S CORRESPONDENCE WITH HERWART VON HOHENBURG

Ungedruckte wissenschaftliche Correspondenz zwischen Johann Kepler und Herwart von Hohenburg, 1599. Ergänzung zu: Kepleri Opera Omnia, ed. Chr. Frisch. Nach den MSS. zu München und Pulkowa edirt von C. Anschütz. (Prag: Victor Dietz, 1886.)

HERWART VON HOHENBURG was a highly stimulating correspondent. His scientific curiosity was insatiable; his official duties as Bavarian Chancellor precluded personal research; and he accordingly deputed to the busy brain of Kepler the working out of problems which engaged his scanty leisure, while baffling his powers. The pressure of his demands was, indeed, so severe that Kepler at times bewailed himself in confidential quarters over the grinding labours they imposed upon him: but he could ill afford to quarrel with a patron who was as generous as he was inquisitive; and he thus continued to evolve for his benefit the stores of curious learning and adventurous theory of which some considerable specimens have lately been unearthed, and are now presented to the public.

The correspondence took its origin from the publication, in 1596, of the "Mysterium Cosmographicum," by which Herwart's admiring attention was drawn to the speculative young "mathematicus" of the Styrian States; and continued from October 1597 to December 1609. There was, however, a gap in its records. Three letters, known to have been written by Kepler to Herwart in the year 1599, were not forthcoming. Dr. Frisch, the late indefatigable editor of Kepler's "Opera Omnia," gave up the search as hopeless; and the detection of the latitant documents became possible only with the correction, in a new printed catalogue of the manuscript collections in the Munich State Library, of an error in the old printed one,—an example, were such needed, of the uses to historical research of the least inviting bibliothecarian drudgery. The opportunity for discovery was promptly turned to account by M. Carl Anschütz, the editor of the present *brochure*; who deserves the acknowledgments of every one interested in scrutinising the workings of a most singularly and brilliantly constituted mind, not alone for the zeal of his inquiries, but also for the elaborate care with which he has set forth their results, rendering intelligible by copious annotations what must otherwise have remained, to the vast majority of readers, hopelessly obscure.

The truth chiefly emphasised by a perusal of these remarkable letters is that of the inextricable entanglement of Kepler's mystical with his scientific views. Many men have speculated wildly while investigating acutely; Kepler alone, perhaps, investigated acutely *because* he speculated wildly. His visions of abstract beauty and order in a neatly fenced and finished universe warmed his fancy, and inspired and lightened labours which would otherwise have been insupportable. His discoveries were the fruit of his illusions, because his illusions were faithfully and unwearyingly confronted with the realities of nature. He was a dreamer; but he was not content to leave his dreams undisturbed by facts. Hence his superiority—

ἔξοχος Ἀργείων κεφαλήν τε καὶ εὐρέας ὤμους—

to the common run of Pythagorean enthusiasts, and hence his great name in scientific history.

The topics discussed in the present correspondence forcibly illustrate the compound nature of his mind, no less visionary in its instincts than positive in its methods. They include the theory of eclipses, the *rationale* of planetary influences, the harmonic relations of planetary velocities, the date of the birth and the horoscope of Augustus, the nature of terrestrial magnetism, and the position, actual and primitive, of the north magnetic pole. The first of the three letters is dated from Gratz, April 9 and 10, 1599. It opens with a pompous eulogium on Tycho Brahe. "Taceant omnes, et Tychoni Brahe Dani auscultent." Nor does it detract, we are told, from his merits to have taken a wrong theoretical turn. His hostility to the motion of the earth nowise impairs the excellence of his observations and instruments. Each astronomer is free to embrace, without discredit to his skill and erudition, whatever "religion of movement" seems best to himself. "Sed ad rem."

Tycho, deceived no doubt by reports of coronal splendours (he had never himself witnessed the phenomenon), had denied the possibility of a total solar eclipse, the moon suffering, he alleged, a diminution of one-fifth of its apparent diameter when projected on the sun. Kepler, while unconvinced of the fact, was at no loss for an explanation. A dense lunar atmosphere, powerfully reflective of the sun's rays, while partially permeable by them, was invoked by him to augment the seeming dimensions of the full moon, and throw a kind of subdued glory round the eclipsed sun. The perplexity started by Tycho was not, however, so easily allayed. It kept cropping up at intervals; and led eventually both to Kepler's optical researches, and to what we may call his discovery of the corona, as an actual fact to be reckoned with by science. The eclipse observed by Clavius at Rome in 1567 he showed to have been unquestionably total; the sun was fully covered by the moon; yet an unlooked-for radiance survived ("Op. Omnia," t. ii. p. 318). He accounted for it by the illumination of an "ethereal substance" in the solar neighbourhood, "not altogether nothing, but possessing some measure of density"; nor have we yet got much beyond the approximate ratification of his conjecture.

Later in life Kepler formally laid down his arms before the lunar theory, after spending enormous labour on the effort to bring it into conformity with his Laws. But here, in these long-missing letters, he unexpectedly emerges as the discoverer of the moon's annual equation. The fact seems to admit of no doubt; his words are explicit. The discrepancies between the observed and calculated times of eclipses compelled the correction. Had not Copernicus, he remarks, been occupied with greater things, he must have introduced the same "annual inequality" depending upon the eccentricity of the earth's orbit. "What he neglected," he adds, "I now do." The chief merit of this important advance has usually been ascribed to Tycho. He had doubtless glimpses of its necessity, but omitted to follow them up. The earliest

explicit declaration hitherto known in favour of introducing such a correction was contained in a letter from Kepler to Bernegger of June 30, 1625 ("Op. Omnia," t. vi. p. 618). It now appears, not only that the conclusion was an entirely original one, but that he had arrived at it twenty-six years previously. M. Anschütz promises some further elucidations of the point, which we await with interest.

One of the most curious chapters in Kepler's mental history is furnished by his attitude towards the astrological superstitions of his time. Herwart, as a good Catholic, had condemned them; his correspondent made out a case in reply. His contention, it is true, was not on behalf of the vulgar charlatanry of the science. This he admitted to be indefensible, save on the one poor plea of stringent necessity. Providence, as he wrote to Maestlin, which had denied to no animal the means of preserving its life, had assigned, for that end, astrology to the astronomer. He must draw horoscopes and publish prophesying calendars, or cease to exist. Thus only could he obtain means to pursue nobler studies. The people, while giving their money for the lies they loved, unconsciously promoted the truth they were indifferent to. It was an involuntary, but none the less efficacious, "endowment of research."

So Kepler filled his empty pockets, and satisfied his conscience by professing incredulity in his own vaticinations. They proved, nevertheless, and, as it were, in his own despite, highly successful. Not a few of them stumbled felicitously into fulfilment. Some art, or luck, drew them, now and again, into conformity with the future. And since, as their author himself remarked, the game is one in which the hits count, but the misses are forgotten ("Das Treffen behält man, das Fehlen aber vergisst man") his reputation as a seer rose high, and brought him in the best and only sure part of his income.

There was, however, a recondite species of planetary influence believed in by Kepler as part of the eternal order of things. By the belief, indeed, his whole career of investigation was profoundly influenced; for the effort to justify it led him into a track of thought which finally conducted him to the Third Law. One of the chief points of interest in the present correspondence is that it discloses the time and manner of his entrance upon that track. "Lift up your ears to listen: Eureka!" he wrote to Maestlin, August 29, 1599; and to Herwart, August 6, he solemnly announced his invention of a "theorema jucundum," in which was concentrated the whole secret of the music of the spheres. Already he gives the title ("de Harmonia Mundi"), and, to a certain extent, the plan, of the great work published twenty years later. It was conceived, as we now see with additional clearness, less under the influence of sober truth-seeking, than in the fervour of illusive speculation. Essentially, it was a piece of brilliant extravagance. That the harmonic law of periods and distances should have been found as a nugget amid such worthless, though shining debris, is one of the oddest facts in the history of science.

The theory of planetary harmonies was struck out by Kepler as an adjunct to his peculiar theory of planetary aspects. It might in fact be called its dynamical counterpart. Geometrical relations of movement were substituted in it for geometrical relations of position. The velocities of the six planets were, he averred, so connected that, were there an inter-planetary medium capable of conveying audible vibrations, a celestial chord of the sixth and fourth would perpetually resound through space. The intellectual perception of potential harmonies sufficed, however, for the delectation of the rational creatures appointed to enjoy them; while, similarly, the intellectual apprehension of "aspects" affected, primarily, the sentient "soul of the world," and, secondarily, through the varying moods thus impressed by the stars, the course of sublunary affairs. The third letter to Herwart is mainly filled with details of Kepler's persevering efforts to complete and fortify the visionary analogy between astrological aspects and musical intervals.

Yet even here, in this region of intangible speculation, his innate respect for facts did not desert him. What autobiographical details he left, we owe to his desire to compare his life as it was with what, astrologically, it ought to have been. And the first of the present letters contains a highly curious little bit of self-study, illustrative of the depressing effects of "Saturn in sextile with the Sun" at the hour of nativity. Here is Kepler described by himself, ætat. twenty-seven.

"A body of no ample proportions, lean and scraggy; a mind unaspiring, that is to say, burying itself in literary nooks and crannies, suspicious, timid, tending towards, and abiding in difficulties and knotty points; manners to correspond. Sour and sharp flavours, the gnawing of a bone, the devouring of dry bread, form my gustatory delights; my keenest ambulatory joy is to traverse steep and rugged paths, to mount hills, to pierce my way across dense thorn-brakes. Pleasure in life other than in study I neither have nor desire; proffered, I reject. My fortune matches my tastes to a hair. Where others might abandon hope, I find access to achievement and fame. Yet not over spacious; for my advance is continually checked, and my circumstances change without mending. All my efforts have hitherto met with strenuous resistance. It may be that social sympathy will ever be denied me while I irritate mankind by advocating the movement of the earth, while

> "tanti ponderis orbem
> Obnixa cervice cito per sidera lapsu
> Incito, terricolûm contra nitente senatu."

A. M. CLERKE

UPLAND AND MEADOW

Upland and Meadow, a Poactquissings Chronicle. By Charles C. Abbott, M.D. (London: Sampson Low, Marston, Searle, and Rivington, 1886.)

THIS is a very pleasantly written book by an author who may be justly regarded as a kind of American Gilbert White. We may as well inform our readers at once that the district of which the natural history is herein chronicled is situated by a little stream which empties itself into the River Delaware, and that the name, which will appear to English readers somewhat difficult of pronunciation, is of Indian origin. There are fourteen chapters in the work, and an index which is to be strongly commended for its completeness. It is really a most important feature in a book of this kind to have a good index, and in insisting upon this necessity we are intentionally paying a complimentary tribute to the

author, because there is a large amount of valuable observation which readers should have occasion to refer to after the first perusal of the work, but which would be lost without such an index, owing to the necessarily disjointed mode of treatment entailed by an adherence to seasonal records. We need only refer to the early editions of Kirby and Spence's "Introduction to Entomology" as an example of a work containing a large collection of facts and observations rendered almost useless for want of an index.

Dr. Abbott is evidently a close observer, and English naturalists will derive both pleasure and profit by a perusal of his chronicle. It is rather to be regretted that he has confined himself so much in the text to the local trivial names of the animals and plants of his district. It places English readers at a disadvantage, for example, to have to turn to the index each time a species is mentioned in order to find out what is referred to under such names as "grakles," "quaker-girls," "quahog," or "scuttle-bug." But this is, after all, a matter of small importance, because the scientific names will be found in the index, and the criticism is made only on behalf of that large circle of readers in the old country which the work ought to attract, and to which it appeals through its English publishers.

The author's strong point appears to be ornithology, but his sympathies are fairly distributed, and his observations are recorded in a pleasant, chatty style which is sure to be attractive to general readers :—

"To realise what a wealth of animal and vegetable life is ever at hand for him who chooses to study it, let a specialist visit you for a few days. Do not have more than one at a time, or you may be bewildered by their enthusiasm.

"I have had them come in turn—botanists, conchologists, microscopists, and even archæologists. What an array of names to strike terror to the breasts of the timid; yet they were all human, and talked plain English, and, better than all, were both instructive and amusing."

As a specimen of the author's style we give the following from Chapter II., entitled "Poaetquissings in Winter."

In order to carry on observations without frightening the denizens of the creek, the author was in the habit of lying down upon the ice, covered over with a blanket so as to be able to see into the frozen depths. The terrestrial life soon became accustomed to his presence, and at length became inquisitive. "This was amusingly illustrated in one instance by a weasel, in crossing the creek on the ice, stopping to investigate the peculiar something lying in its path. Peering under the blanket, it either heard my blood circulating or smelled it. At all events it gave my ankle a nip which brought me quickly to my feet, and sent the bloodthirsty wretch scudding over the ice with marvellous rapidity. How the crows laughed! I had noticed a flock of these birds when I went to the creek, and had been wondering if their incessant cawing was not a discussion of my curious movements. They were, possibly, disposed to think me a trap laid for them, but were astonished or amused at my sudden regaining of the perpendicular when the weasel offered to investigate the matter."

In the third chapter, "Twixt Cold and Heat," will be found a good collection of observations and experiments bearing on the subject of instinct, with special reference to the nesting of birds. Whether the author's views on this much-vexed question will command assent we cannot undertake to say, but whether we differ from his conclusions or not, his experiments are certainly worthy of serious consideration. Among these we have a series of experiments with a chromo-picture of a cat, with a mirror, and with coloured yarns, the latter having for their object the testing of the sense of colour. In the case of a Baltimore oriole in course of building its nest, a decided choice was exerted—red, yellow, purple, and green yarns having been refused, and gray only selected, till the nest was nearly finished, when a few of the purple strands were used. Other amusing experiments on the transference of eggs are described in this same chapter.

With reference to the subject of migration the author states in Chapter IV., on "Marsh Wrens," that "temperature and migration are largely coincident, but cannot be considered as cause and effect." He further adds that certain rules respecting the habits of American birds which had been regarded by previous observers as fixed and invariable, are quite variable if observations are only continued over a sufficiently long period of time. "The results of a single year will have but little bearing upon the regularity or want of it in a bird's movements. The observations of the same person in the same locality must extend over at least a decade before it is safe to arrive at any general conclusions." We commend this passage to the members of our county field clubs who are in want of material for observation.

Space will not permit us to make any lengthy extracts from the book, but we cannot refrain from calling the attention of the bird-destroyers of this country to the admirable "apology" for the grakle (*Quiscalus versicolor*) which the author makes in the fifth chapter. These birds were formerly regarded as enemies to agriculture, owing to their habit of feeding upon ripe grain, which led to their being dubbed by the unpopular name of "maize thieves." But, according to Dr. Abbott's observations, it is at least doubtful whether, on the whole, man does not profit more by the existence of these birds than is lost by the attack upon the grain. To get an idea of the amount of insect food consumed by a pair with five young, he observed the birds for two hours (10 to 11 a.m. and 2 to 3 p.m.), during which time thirteen trips were made by each bird, each returning with an insect every time. The young thus got a "square meal" at least every ten minutes. The feeding goes on for ten hours per diem till the young are twenty-five days old, when they leave the nest, so that during this period each young bird has been supplied with 1300 insects, or 6500 altogether. The eleven nests in the colony under observation were supplied, therefore, with 71,500 insects, and as seven pairs in the colony raised second broods, a further supply of about 45,500 insects was "requisitioned," thus bringing up the total number consumed by one colony of birds to the enormous total of 117,000, or, including the food of the parent birds, about 150,000 "forms of insect life destroyed, all of which would have proved more or less destructive to the growing crops." We hope that the lesson taught by this observation will not be lost upon those who fail to see in persecution by birds a sufficient

cause for the marvellously perfect cases of adaptive resemblance so common among insects.

Apropos of the extermination of plants, Dr. Abbott remarks (p. 41) with respect to the witch-hazel (*Hamamelis virginica*):—"Bent twigs of this plant are still used by the 'gifted' to find water, lost farming tools, and, by one enthusiast, Indian graves. The faithful still claim it as efficacious, and he who doubts is sneered at if he expresses his opinion. All that the rambler can ask is that the plant be not exterminated, and that the fools may be." We may perhaps echo this sentiment on this side of the Atlantic without offence to the members of "Primrose" or any other floral "Leagues." A protest against the extermination of rare plants by "dealers" was circulated by the Corresponding Societies Committee of the British Association last year.

We have given a sufficient idea of this work to commend it to the notice of English naturalists, and we may remark in conclusion that, although the animals and plants referred to are not familiar to the ramblers by our own streams, the sparkling anecdotal style will cause the volume to be enjoyed by all, whether trained observers or casual country wanderers, and the spirit in which the author goes forth into the fields and woods or saunters by his favourite "Poaetquissings" may be well imitated by the numerous field naturalists now being called into activity by the widely-spread establishment of local societies. "He who has this interest in the life about him can never be lonely, wander wheresoever he will, nor return from a contemplative ramble other than a wiser and happier man." R. M.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

On Refractometers

I OBSERVE that in your issue of June 17 (p. 157) there is an article by Mr. Gordon Thompson on "The Determination of the Index of Refraction of a Fluid by Means of the Microscope." The method there described was, I believe, first proposed by the Duke de Chaulnes in 1767; and in 1876 was suggested by Dr. Royston Pigott in connection with his refractometer. It was employed in 1878 by Dr. Sorby for recognising the minerals in thin slices of rocks; and in 1884 by Dr. Bleekrode in determining the refraction of liquefied gases (*Proc.* Royal Society, vol. xxxvii. p. 343). In these two instances the ordinary method was unavailable. The proposed method has not been much used owing to the fact that the index of refraction cannot be at all depended upon beyond the third place of decimals.

Mr. Thompson considerably exaggerates the difficulty of the usual method by means of a hollow prism: the angle of the prism may be determined once for all; the position of minimum deviation presents no difficulty; and the use of monochromatic light is unnecessary. Indeed it would be objectionable, as it would prevent the determination of the dispersive power, which is often of equal importance with the refractive power of the substance. In my own experiments I have often taken observations both of the refraction and dispersion of five or six liquids during the course of an hour, including the cleaning of the prism between each.

The suggested method seems scarcely to admit of determining the temperature of the drop with any accuracy, which is an important matter where liquids are concerned. It may, however, doubtless be employed by those who have a good microscope, where great accuracy is not required.

There is an instrument called Abbe's refractometer, which I have recently used for preliminary determinations, and I find it gives accurate results to the third place of decimals. It is founded on the principle of total reflection. It requires also only a drop of the liquid, and as the index of line D is read off without any calculation a complete determination can be made in a minute or two. There is also an arrangement by which the dispersion D to F can be observed and calculated, but I do not find that this is accurate enough to be of much service. The instrument is to be obtained of Carl Zeiss of Jena.

17, Pembridge Square, June 26 J. H. GLADSTONE

Luminous Boreal Clouds

DURING the past two or three years what appears to the writer a distinct class of luminous night clouds in the north sky have occupied his attention. They have probably not escaped more competent observers, and been perhaps referred to simple auroral phenomena, thus escaping discussion. A very marked example was visible here the night before last (22nd inst.), of which inclosed is an illustration from a sketch at the moment.

I may premise the sky was generally clear, stars bright, temperature very low, and wind strong (N.B.) from north-west—a direction maintained for the past two days. Only a slight degree of illumination was imparted to the clouds by a low moon in the south-east, near last quarter. Some light cirrus "scud," high up, conformed to direction of wind.

Above and behind a dark but very limited bank of strato-cumulus, a luminous cloudlet of brilliant pearly lustre appeared, not concurrent exactly with either the magnetic or true meridians, in altitude from 5° to 10° from the horizon, and for 7° in horizontal arc. Its shape, character, and position little varied during observation from 11.30 p.m. to 2 a.m. The structure in this case (only partially realised in the sketch) was striated, the "strike" of main streaks being north-east and south-west. *Transverse bars of luminosity conformed closely to the direction of the cirrus clouds above, and of the wind.* On the three or four other occasions of such observations these luminous cloudlets have been devoid of structure, but in every case they have presented, as in this, an opaque pearly lustre, with definite outline.

Of an entirely different type to the eye are the sudden, diffuse, variable, and transient transparencies of auroræ. Avoiding premature discussion, one cannot but suspect the former occur in much lower and less rare sky-tracts probably than the latter, with a possible frictional factor in their development; and might be distinguished as *nub culæ boreal.s* if accorded a special place on further observation. The temperature has been keeping low, and sunset after-glows have in some degree reappeared during the past week; especially gorgeous being the cloud-tints at sunset of the 22nd inst. D. J. ROWAN

Dundrum, co. Dublin, June 24

Ampère's Rule

WITH regard to Ampère's rule I should be glad to know what is the general experience of actual teachers?

I have taught electricity to boys for four years, and when at Rugby I learned the subject for I think two years. My experience has been that "Ampère's rule" is *not* confusing; and as a teacher I find it best to give both this rule and the "screw-motion" rule. I see that Mr. Cumming gives both, on p. 222 of his book. W. L.

The College, Cheltenham

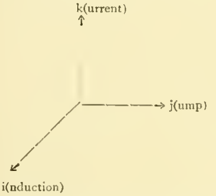
As Prof. Daehne (NATURE, June 24, p. 168) has called attention again to the treatment of Ampère's rule in my "Electricity Treated Experimentally," perhaps you will allow me to point out that the rule given by Ampère is quoted *historically* only, and for it is substituted a rule, due, I believe, to Clerk-Maxwell, which seems to me preferable to either the original rule of Ampère, or to that quoted by Prof. Daehne, namely, that the movement of a north pole is right-handed to the direction of the current. That is to say, if we assume any right-handed screw to be propelled along the current, the north pole will move in the direction of the twist in the muscles of the wrist in propelling it; and *vice versâ*, if the north pole move in

the direction of propulsion, the current urging it will be in the direction of twist in the muscles of the wrist. In treating the movement of a conductor carrying a current in the magnetic field, I have used a rule identical in character with Ampère's, and that was probably the rule to which J. T. B. referred in his critique, namely, that a figure swimming in the current and looking along the lines of force is carried to his left. I should be glad to find a rule at once as complete and more simple, although after a pretty wide experience, not always with the very brightest of pupils, I have not been sorely pressed with the difficulty J. T. B. seems to have felt. All the required attitudes are pretty familiar to a boy who is accustomed to diving in the water and swimming on his front, side, or back. L. CUMMING

Rugby, June 24

THE following version of Ampère's Rule is one which I communicated some time ago to a few friends, but it did not appear to me to be expressed in language sufficiently grave to justify its publication. Still, as the Rule is a simple and useful one, your readers, in general, may be disposed to overlook its levity.

Draw the three well-known Hamiltonian vectors, i, j, k. After i put (nduction), after j put (ump), and after k put (urrent). Then the figure explains the action of magnetic induction on an electric current. The figure in fact asserts that i(nduction) in i makes k(urrent) in k to j(ump) along j.



Of course the same figure gives the direction (according to the Law of Lenz) of the current generated by a motion (*i.e.* a jump) of a conductor in a given direction in a magnetic field in which the direction of the induction is given.

R.I.E. College, Cooper's Hill GEORGE M. MINCHIN

An Earthquake Invention

IN my letter to NATURE, vol. xxxiii. p. 438, I clearly showed that the supposition of Mr. D. A. Stevenson and Prof. Piazzi Smyth that I had endeavoured to claim the aseismatic joint of Mr. D. Stevenson was due to their imperfect acquaintance with seismological literature. I certainly intercalated a *note* about aseismatic structures in a report to the British Association on earthquake phenomena in general, without mentioning Mr. Stevenson's name.

Previous to this, when specially speaking or writing upon aseismic structures, I have *repeatedly* referred to the work of Mr. D. Stevenson. Such references were quoted. Under the circumstances I asked Messrs. Stevenson and Smyth to distinctly state whether they still considered themselves justified in continuing their accusations. If this point was overlooked the discussion might be considered as at an end. Mr. D. A. Stevenson has replied, but the question at issue *has been distinctly evaded* (NATURE, vol. xxxiii. p. 534).

I deeply regret that Messrs. Stevenson and Smyth should allow a discussion to terminate in such a manner.

Tokio, May 22 JOHN MILNE

[This must now close.—ED. NATURE.]

Professor Newcomb's Determination of the Velocity of Light

I HASTEN to correct an error which has crept into my account in last week's NATURE (p. 171) of Prof. Newcomb's measures of the velocity of light. The arrangement employed by Foucault in 1862 was *not* that adopted by Newcomb, and illustrated in Fig. 1, but that sketched in Fig. 2. In other words, he placed his lens between the revolving and fixed mirrors. His apparatus is described in *Comptes rendus*, t. lv. p. 792, where the velocity of the rotating mirror is stated to have been 400 revolutions a second, and the total length of path between the mirrors 20 metres. A. M. CLERKE

June 28

Solar Halo and Sun Pillar seen on June 5, 1886

WHEN approaching the Observatory, about 6.45 p.m., my attention was drawn to portions of a solar halo, which appeared as in Fig. 1.

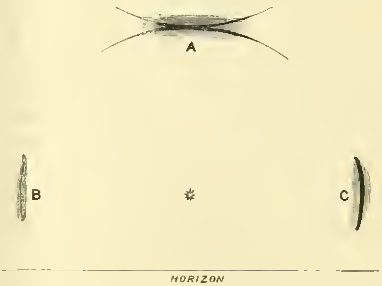


FIG. 1.—A, very bright; C, fainter; B, very faint.

This remained visible until after 7 p.m., and nothing more was seen before 7.30 p.m. When looking out at 7.40 p.m. G.M.T., I noticed something unusual, and came at once to the conclusion that it was a solar pillar, and made a sketch in a note-book and the following remarks:—

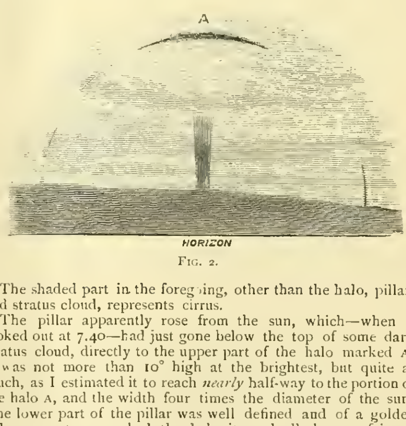


FIG. 2.

The shaded part in the foregoing, other than the halo, pillar, and stratus cloud, represents cirrus.

The pillar apparently rose from the sun, which—when I looked out at 7.40—had just gone below the top of some dark stratus cloud, directly to the upper part of the halo marked A. It was not more than 10° high at the brightest, but quite as much, as I estimated it to reach *nearly* half-way to the portion of the halo A, and the width four times the diameter of the sun. The lower part of the pillar was well defined and of a golden colour; as it approached the halo it gradually became fainter, and was then lost in the cirrus cloud. The upper part was somewhat wider; perhaps this was due to the greater amount of cloud there, which diffused the light.

At 7·55 G.M.T. all portions of the halo had gone except a small piece at A, and the pillar was fainter, but still quite visible. At 8·3 the halo and pillar had disappeared. The sun set at 8·14 p.m.

An ordinary halo ($22\frac{1}{2}°$) was visible, more or less bright and complete nearly from sunrise to sunset.

I could not fix the position of the pillar by stars, none near being visible. The sketch was made at the time from a window of the library of the Observatory.

That seen here on 1883 April 6, by Mr. W. A. Robinson, was about 15 minutes *after* sunset; this observation was 30 minutes *before*. The time at which the pillar was visible on the former date was given by nearly all your correspondents as

15 m. to 45 m. *after* sunset, and in most cases in a cloudless sky; but that on 1886 June 5 was the reverse in both points. Some notes will be found in NATURE, 1883, April and May, for the pillar observed in that year. That seen here partly resembles Fig. 4, Plate III. in *Symons's Meteorological Magazine*, 1871.

F. A. BELLAMY

Radcliffe Observatory, Oxford, June 21

The Enemies of the Frog

WHEN living at Mackay, Queensland, I frequently observed that the common house-frogs (*H. cærulea*) were injured in the hind-limbs, and on several occasions I would hear them croaking in pain; but on arrival all I saw would be a wretched exhausted frog weakly hopping away with a wound in the hind-leg, from which the blood would be oozing. Later on I found that rats attack the frogs. The rats catch the frog by the hind-leg, and apparently suck the wound they cause, then let the frog crawl away, attack it and suck it again, and so on until the rat has had enough. I believe the rats suck the blood, because I was never able to discover a frog so attacked on which the flesh had been destroyed.

Mr. W. P. Fletcher, a well-known local naturalist, once gave me the following account of a mantis attacking a frog. It was in the autumn of 1877, at Rockhampton, Queensland. He was "attracted by hearing the noise of a frog in distress, in the daytime, in some garden-shrubs about 6 feet high; he went to see the cause, and found a green frog about 2 inches long. A green mantis about 5 inches long, with one claw had hold of it across the neck, so that the frog could not move, and the mantis was chewing, and did chew off, the hind-leg, the blood flowing profusely." He called Mrs. Fletcher to see them, and then destroyed the mantis, whereon the frog crawled away.

At Lake Elphinstone (100 miles from Mackay) I once found a small frog (*H. rubella*) in the house in a very exhausted condition; on examination I found a large leech on its tongue. This specimen, with the leech attached, I gave to Mr. Boulenger at the British Museum, where it can be seen.

At Mackay the chief enemies of the frogs appeared to be the snakes and the *Agamidæ*.

H. LING ROTH

Chronology of Elasticians

IN forming a chronological list of writers on elasticity I have been unable to ascertain the following dates, which I should be much obliged if any of your readers would kindly supply: Mariotte (*né près de Dijon vers* 1620, Marie). Is nothing more definite known as to the date of his birth?

F. E. Neumann ... ... W. Weber ... ... Eaton Hodgkinson	Death years.
S. Haughton ... ... J. H. Jellett	Birth years.

University College, London, June 27

KARL PEARSON

SOLAR METEOROLOGY[1]

SIG. TACCHINI'S detailed report on the various phases of solar activity during the year 1884 deserves, as might be expected from the reputation of its author, most careful attention. Exceptionally fine weather permitted observations of sunspots and faculæ to be made at the Collegio Romano on 307 out of the 365 days, so that the materials accumulated were more than usually abundant. We are thus particularly well informed regarding the symptoms attending the protracted maximum which culminated in February 1884.

This is the more fortunate as that maximum was distinguished by features of special interest. It was delayed considerably beyond the usual term, the interval from the maximum of 1870 being no less than 13·4 in lieu of the normal 11·1 years. And to this delay corresponded a greatly reduced intensity, in accordance with the law by which the undulations of the curve representing spot-frequency are low in proportion as they are long. The maximum of 1884, accordingly, was by much the feeblest which had occurred since 1830. It was moreover a hesitating—it might almost be called an abortive—maximum. Some unknown cause apparently interfered with its due and punctual development. Partial anticipatory outbreaks betrayed the tendency, continually repressed, to complete the cycle at the regular epoch, and with the regular expenditure of energy. Now perturbation—of whatever nature—is always instructive: hence Signor Tacchini's laborious statistical results acquire added significance.

They have been gathered along several closely connected lines of research. The various classes of solar surface-phenomena—spots, faculæ, prominences, metallic eruptions—have been studied apart, and the several resulting inferences as to the progress of solar disturbance subsequently confronted. The trifling discrepancies thus revealed show the mutual dependence of no two such species of commotion to be absolute. Each swells or subsides on the whole without immediate or invariable reference to any other, although under the obvious control of some common underlying cause.

Sunspot activity received a notable accession in the beginning of October 1883, the phase of excitement reaching its acme in the following February,[1] and persisting until the end of May. Since then, some slight oscillations notwithstanding, it has continually declined. The sun was not, however, observed at Rome to be free from spots on a single day in 1884. The maximum for prominences occured in March, and they continued exceptionally numerous down to the end of October. In all, 2714 were delineated and described in 242 observations with the spectroscope, being at the average rate of 11·22 per diem. Sixty metallic eruptions, observed on the same occasions, gave a mean diurnal frequency of 0·248 as against 0·171 for 1883. The richest crop was collected in November 1884, during which month ten eruptions were recorded in sixteen observations. The development of faculæ deviated so markedly from that of spots that their respective fluctuations were at times even inverted. It should also be noted that the mean area per spot in 1884 was of little more than half its value in the preceding year, and that the magnetic instruments at Rome remained throughout comparatively calm

Much valuable information is afforded by Signor Tacchini's careful inquiries as to the distribution on the sun's surface of the different orders of solar phenomena. All these showed, during 1884, a conspicuous prevalence of activity in the southern hemisphere; and the inequality—as appears from a note by the same author presented to the Reale Accademia dei Lincei, March 7, 1886—became still more striking in the ensuing year. No spot was observed in either hemisphere during 1884 at more than 30° from the equator; nor on the northern side, during the latter half of the year, at above 20°. With this contraction of the spotted zone coincided a close approach to the equator of the parallel of maximum frequency; and the usual equatorial minimum was both in 1884 and 1885 very imperfectly maintained.

Prominences were plentifully distributed between 60° north and 50° south latitude, with maxima between 20° and 30°. As during the spot-maximum of 1870, they showed no disposition to avoid the vicinity of the equator; while in 1880, 1881, and 1882, the equatorial minimum of prominences was very marked, and remained perceptible in 1883. Although some rare instances of metallic eruptions were detected in high northern latitudes, they affected chiefly a zone bounded by parallels of 20°. Faculæ occurred predominantly in the same region, and nowhere appeared in latitudes above 50°. On the whole, a concentration towards the equator of the whole range of phenomena was unmistakable, and might be thought to

[1] "Meteorologia Solare." Note di P. Tacchini. Estratto dagli *Annali della Meteorologia Italiana*, Parte 3 1884. (Roma: Tipografia Metastasio, 1885.)

[1] M. Rudolf Wolf places the maximum in November 1883.

correspond with a more advanced stage of the spot-cycle than was indicated by numerical data alone.

Signor Tacchini concludes his memoir with a survey of the vicissitudes of spot-formation during eight years—from January 1, 1877, to December 31, 1884. The results are graphically exhibited in a set of curves variously derived. No. 1 is constructed from the daily numbers of spots with their accompanying pores; No. 2 from the record of spots alone; No. 3 shows the frequency of groups; No. 4 follows the fluctuations of spotted area; No. 5 those of facular extension. All the first four methods agree in fixing the ab olute minimum in March 1879; Nos 1, 2, and 3 display secondary maxima in September 1880, March and July 1881, and March 1882: the absolute maximum was reached, judging by the statistics of spots and groups (curves 2 and 3) in February 1884; but in November 1883, according to those of spots with pores, and spotted area (curves 1 and 4). It is noticeable that the preliminary maxima are largely exaggerated in proportion to the true maximum, when pores are admitted into the account with spots; while the curve resulting from the simple enumeration of groups is very slightly indented. This last method is regarded by our author as that which should invariably be followed when recent have to be compared with ancient records of sunspots; but no system of observation with the eye can any longer compete with the simpler and surer one of photographic registration.

The curve of facular extension for 1877–84 is somewhat anomalous. It touched its lowest point in November 1878, five months previous to the spot-minimum, then sprang up to an absolute maximum in October 1880. This was followed by a secondary but very considerable rise in September 1881, after which, during two years, a tolerably high average level was maintained. The definitive decline which set in in September 1883 was only partially arrested in May 1884. The coincidence between the maximum of faculæ in September 1880, and a large and abrupt increase in the formation of pores, just a month earlier, should not be overlooked. It is also remarkable that a maximum of prominences, but slightly inferior to that of 1884, occurred in 1881.

The condition of the sun in 1885 is epitomised in the note by Signor Tacchini already referred to. That year was, in his opinion, distinguished as one of continued agitation by the persistent abundance of its various symptoms in the neighbourhood of the equator. A zone of 40° north and south covered all the spots, and (save one example of each kind) all the faculæ and metallic eruptions observed. The tranquil or "hydrogenic" description of prominences, on the other hand, figured indifferently in all latitudes. Their general equality of diffusion was but slightly infringed by a southern preponderance; while the frequency in the same hemisphere of spots, faculæ, and eruptions was, in each class, almost double that of its northern occurrence. The alternating activity of the solar hemispheres, thus exemplified in one of its most conspicuous phases, is one of the many enigmatical features of solar disturbance.

SEISMOLOGY IN JAPAN[1]

FOUNDED only in 1879, the Seismological Society of Japan is already able to point to a good record of accomplished work. The Society was happy in the time and the place of its birth. No home could be more fitted to nurse the enthusiasm of the seismologist than one whose foundations are shaken, on the average, a little oftener than once a week. One may take a rather half-hearted interest in other natural phenomena, but, while it lasts, an earthquake certainly commands undivided attention. And the Society came into being just when a few zealous investigators were striving who should be first to solve the problem of obtaining an accurate record of how the ground moves in an earthquake. Lord Byron has described a thunderstorm in the Alps as the joy of the hills "o'er a young earthquake's birth;" but the joy of the hills, if more loudly expressed, was nothing like so deep as the joy with which the inventor of a new "earthquake machine" felt the first convulsion that came to test its powers. In these congenial conditions it is not surprising that the Society's early volumes record the history of what is nothing less than a new departure in observational seismology. Of late the Society has suffered by the removal from Japan of some of its more active members; but this latest volume of its *Transactions* gives satisfactory evidence that, while it has not yet lost all its foreign supporters, some of the Japanese themselves are ready to step forward and continue the work. So long as Prof. Milne remains, the Society will not lack material for publication; the present volume, like many of its predecessors, is largely the work of his pen.

The first paper, on "Seismic Experiments," is by Mr. Milne, and contains an account of eight series of experiments on artificial earthquakes, as well as some laboratory work. Part of this work was done in conjunction with Mr. T. Gray, and much of it has already been described in other papers. The vibrations of the ground were produced in some instances by letting fall heavy weights, in others by the use of dynamite. Several observing-stations were selected, at various distances from the source of disturbance, and generally in one straight line with it. At these stations seismographs of various kinds were placed, and Prof. Milne seems to have preferred the horizontal pendulum seismograph of the present writer as an instrument for recording separately two rectangular components of the horizontal motion of the ground. By placing the pair of pendulums so that one recorded vibrations in the direction of the line joining the station with the source, while the other recorded vibrations at right angles to this, Prof. Milne was able to separate without difficulty the normal from the transverse constituents of the disturbance, and to see the normal vibrations arrive sooner than the transverse vibrations at each station, as the theory of waves in elastic solids requires. In this instrument the two components of horizontal motion are separately recorded on a moving plate of smoked glass. Another instrument was used to record the whole horizontal movement on a fixed plate, and, as might be expected, the diagrams it gave showed first a movement in the line of the source, quickly followed by a confused wriggle of vibrations in all azimuths. By telegraphically connecting the moving plates of the horizontal pendulum seismographs, Prof. Milne endeavoured to determine the interval of time between the arrival of the disturbance at successive stations, and so to infer the velocity of transit. From the results he has concluded that the velocity decreases as the disturbance travels away from the origin, but the figures on which this conclusion is based seem to the present writer to furnish very insufficient evidence. In one series of experiments there is, in the average of three pairs of observations, a loss of about 6 per cent. in the velocity between the second and third stations as compared with the velocity between the first and second stations; but, when we examine the individual observations, we find in one case a gain of velocity amounting to 14 per cent. And, on turning to what is apparently the most complete series of automatically-recorded diagrams (which are reproduced in lithographed plates), it is clear that the time-intervals cannot have been measured with the precision necessary to establish this result, still less to justify the further conclusion that the velocities of normal and transverse waves become more nearly equal as the disturbance spreads. The velocity of transit is, in fact, a term of very vague meaning, unless we can follow an individual wave along its course. As Mr. Milne's

[1] *Transactions* of the Seismological Society of Japan, vol. viii. (Tokio: Published by the Society, 1885.)

own observations show, the shock loses much of its individuality as it travels further from the source. It becomes more and more preceded by a vanguard of small waves, and, for this reason, seismoscopes of different degrees of sensibility will differ in the time at which they chronicle the arrival of the group. Mr. Milne's results, as summarised by him at the end of the paper, are too numerous to be taken up in detail. The assiduity with which he has pursued these experiments deserves the greatest praise, especially as the experiments themselves are of a very high order of difficulty. It is perhaps to be regretted that Mr. Milne has not given his attention more to perfecting a single series than to multiplying results which, as he himself remarks, are often "most discordant." Seismographs will not tell the truth unless they are very well made and very carefully tended. Some of the jagged outlines of the curves are much more likely to be due to friction and shakiness and want of rigidity in the instruments than to any characteristic in the motion of the ground; and unless the lithographer has done Mr. Milne a serious injustice, there are cases where the ground suffers a considerable displacement in a good deal less than no time. He has himself observed this in one instance, and ascribes it to what must (if his explanation be correct) be called a faulty mode of setting the seismographs. It is not impossible to get results free from these defects; and a single really good set of diagrams would do much to remove the uncertainty which now attaches to many of Mr. Milne's results.

Besides the experiments with artificial earthquakes, the paper describes a laboratory investigation of the stability of cylindrical columns standing on a platform which vibrates horizontally, and of the velocity of projection of detached bodies. The projected bodies were balls, held in L-shaped notches at the top of a vertical wooden post; the post, bent slightly to begin with, was allowed to spring; the velocity of projection of the ball was determined from its trajectory, while the greatest velocity of the post-head was measured by means of a revolving plate of smoked glass. The two agreed fairly well, and with a nearly frictionless ball supported in this manner no other result was to be expected. The late Mr. Mallet used to calculate the velocity of the ground's motion from observation of the horizontal distance traversed by projected bodies, but the velocity with which a body is projected depends too largely on the mode of support, and on the amount of adhesion between the body and the support, to allow the result to be, in general, of the slightest value. With regard to the overthrow of columns, it would seem that the author falls into the error of supposing that when the resultant force got by compounding the weight of the column with its resistance to acceleration passes outside the base, the column will fall. But since the disturbing force is of short duration, all that necessarily happens in such a case is that the column will rock; whether it will fall or not is a question of much greater difficulty.

The second paper is a note by Mr. S. Sekiya, "On Prof. Ewing's Duplex Pendulum Seismometer, with Earthquake Records obtained by it" The paper is a brief but very clearly written account of a form of the duplex pendulum seismograph designed by the present writer in 1883, and now in constant use under Mr. Sekiya's care. An earlier form of the instrument has already been described in NATURE (vol. xxx. p. 152): the latest modification of it was exhibited to the British Association at Aberdeen, and will shortly be illustrated in this journal. Its function is to draw on a fixed plate a magnified diagram of the ground's horizontal motion; the figures, which are generally of great complexity, are given by Mr. Sekiya for a number of recent Japanese earthquakes.

A short paper follows by Mr. E. Knipping, "On the Meteorology of Japan," gathered from observations made at twenty-three stations during the year 1883. It mentions that the annual variation of temperature for that year in Japan was more than double that of Britain, and that changes of 14° C. or 15° C. at one station in twenty-four hours are not unfrequent in the spring and autumn.

A paper by Father Faura, S.J., of Manila, describes the Cecchi seismograph—an instrument belonging so decidedly to the old school of seismology that, by allowing its description to appear without criticism, the Society at least shows its catholicity of spirit.

Dr. Dubois contributes some notes on the earthquakes of Ischia, and refers to the effects—or rather absence of effects—of the earthquakes in excavations there, in support of the fact that seismic shocks which do much damage on the surface may pass unperceived at a certain distance beneath.

The volume concludes with a catalogue of earthquakes registered in the meteorological observatory of Tokio by Palmieri's recording seismoscopes. The list for 1884 shows the respectable total of seventy distinct disturbances, and twenty-eight were registered in the first four months of 1885.

J. A. EWING

RECENT ADVANCES IN SANITARY SCIENCE

"HYGIENE," in the words of the late Professor Parkes, "is the art of preserving health; that is, of obtaining the most perfect action of body and mind during as long a period as is consistent with the laws of life. In other words, it aims at rendering growth more perfect, decay less rapid, life more vigorous, death more remote." The art of preserving health is correlative with the science of prevention of disease, since perfect health means the absence of disease and of tendencies to disease. Hygiene is thus the art of preserving health and the science of preventing disease; and in taking into account recent advances in sanitary science we must consider recent acquisitions in our knowledge of the origin, causes, and spread of disease, more especially of those diseases known as "preventable," as well as the methods of improving the natural conditions or social relations surrounding us, which are instrumental in preserving health and counteracting disease.

The etiological relations of all diseases are a subject of interest to the sanitarian, but those which have received the most attention of recent years, and in which the most striking advances of knowledge have either already been made, or are imminent in the near future, are perhaps Asiatic cholera, typhoid or enteric fever, diphtheria, and phthisis or tubercular disease of the lungs. The mode of origin and spread of Asiatic cholera has attracted great popular attention, both on account of its possible introduction into this country from infected districts of the Continent, and from the alleged discovery by Koch of a *Spirillum* or *comma-Bacillus* asserted to be the specific cause of this terrible disease. The Report of the Government Commission consisting of Drs. Klein and Heneage Gibbes, who visited India in 1884 with the object of undertaking researches into the etiology of Asiatic cholera, has lately appeared, and in this Report the conclusions arrived at by Koch from his own researches are very directly traversed. This Report, too, has received a very cordial support from a Committee consisting of many eminent physicians and physiologists, which was convened by the Secretary of State for India for the purpose of taking it into consideration. It must be apparent, however, to any one who makes an impartial study of the literature of the subject, that, if Koch's organism has not yet been proved to be the actual cause of the disease, it has been proved to differ from all other organisms asserted to be identical with it, from the fact that its growth in various nutrient media is characteristic, and serves to distinguish it from all other organisms. As far as our knowledge at present extends, difference in manner of

growth in nutrient media affords as just a basis for distinction between micro-organisms as difference in microscopical appearance or other morphological characteristics. Koch's comma-Bacillus is therefore diagnostic of the disease, and this fact has now placed in the hands of medical men the power of at once recognising a true case of Asiatic cholera, the isolation of the organism from others in the choleraic discharges and its cultivation in suitable media being alone needed. The results of Koch's researches, whether fully accepted or not, have not affected, nor are they likely to affect, the measures on which reliance alone can be placed for the prevention of outbreaks and spread of the disease. In the words of the Committee before alluded to, "Sanitary measures in their true sense, and sanitary measures alone, are the only trustworthy means to prevent outbreaks of the disease, and to restrain its spread and mitigate its severity when it is prevalent Experience in Europe and in the East has shown that sanitary cordons and quarantine restrictions (under whatsoever form) are not only useless as means for arresting the progress of cholera, but positively injurious."

The view that typhoid fever cannot arise *de novo*, but is always propagated by a specific contagion from a previous case of the disease, is steadily gaining ground, as the number of epidemics where the disease has been definitely traced to specifically polluted air or water increases. In many other cases, although the specific pollution has not been definitely proved, the probabilities in favour of such a view have been very great. No micro-organism has yet been found which can lay claim to be regarded as the specific contagion of the disease, but we are in possession of so many facts concerning the mode of origin and spread of this disease, that any discovery of that nature would probably not greatly affect the measures now taken for its prevention.

The etiology of diphtheria has lately received very careful study, but so far without the attainment of any results capable of exact formulation. It is not a disease invariably dependent on insanitary conditions, such as typhoid fever is, but that such conditions favour its spread and severity is more than probable. The far greater comparative frequency of diphtheria in rural districts than in large towns in this country is well known, and has been attributed to the presence in the air of the latter of the products of coal combustion. This view appears the more probable seeing that Continental cities, where wood and not coal is chiefly used as fuel, enjoy no such comparative immunity from the disease. Excessive moisture in the air of a house, whether arising from defective construction of the walls or roof, or from a water-logged soil, are conditions very often associated with diphtheria. The fact also that the disease is most prevalent in the damper seasons of the year, when vegetable matter is undergoing decay and fungus life is most active, favours the theory that the specific contagium of this disease is a mould or fungus, which flourishes most strongly in a damp and smokeless air. It is a remarkable fact that diphtheria is sometimes associated with scarlet fever in one epidemic, the two diseases appearing to be interchangeable; but this is a subject that requires further elucidation. The contagion of diphtheria is extremely persistent and long-lived, clinging with great pertinacity to infected articles, so that every article which is likely to have become contaminated requires very thorough disinfection, preferably by heat. There can be no doubt that school attendance is often a chief factor in the propagation of the disease amongst children.

Koch's discovery of the *Bacillus tuberculosis*, a micro-organism now proved to be the specific contagium of tubercular disease in men and animals, has placed tubercular phthisis in the category of contagious diseases. A peculiar disposition or tendency, whether hereditary or acquired, is no doubt wanted to enable the germ to take up its habitat in the human lung, but the fact that this idiosyncrasy can seldom be definitely recognised renders great caution necessary both on the part of members of a family in their association with a consumptive relation, and of hospital authorities in admitting into a general ward cases of tubercular disease, or of massing together into one institution patients in every stage of the disease. The Bacillus is constantly present in the sputum and probably in the breath of phthisical patients, and this points to the necessity of free ventilation of living and sleeping apartments, and disinfection of soiled articles of clothing and furniture. The external conditions which, of all others, cause a predisposition to consumption are, a damp subsoil, causing excess of moisture in the air, and the constant breathing of an atmosphere vitiated by human respiration. It has been asserted that tubercle can be propagated from animals to man by the consumption of diseased meat, or, in the case of the cow, from the milk of a tuberculous animal. Further proof is required before we can accept such an hypothesis, but there is nothing improbable in such a mode of conveyance of the disease, especially in the case of children with a tubercular predisposition.

Besides the diseases which we now know to have been propagated through the agency of milk—enteric fever, scarlet fever, diphtheria, &c, in which the introduction of the morbid matter is accidental, the milk serving only as a means for its conveyance and perhaps for its growth—there is a complaint fairly definite in character, which has been attributed to the consumption of the milk of cows suffering from foot-and-mouth disease. Here the morbid quality is inherent to the milk as taken from the cow, and is not due to an accidental introduction. The symptoms described in the epidemics recorded are fever, vesicular eruptions on the lips and in the throat and mouth, and enlargement of the glands of the neck. During the prevalence of foot-and-mouth disease, all milk taken by a household should be boiled before consumption. In view of the many dangers which threaten us through the agency of milk, it would perhaps be advisable, especially where children are the chief consumers, that this precaution should be always adopted; at least until the sanitary authorities in towns have the power of inspecting and controlling the farms and dairies in the country from which the chief part of the milk-supply is derived.

The possibility of the transmission of the contagion of small-pox for considerable distances, not exceeding one mile, through the air, has been warmly supported. There are many facts in favour of such a view, and its great probability will be seen from the following considerations. The contagion is almost undoubtedly a micro-organism of the class Bacteria, but as it has not yet been isolated and identified, we are unaware if it is capable of spore-formation or not. The spores of Bacteria can resist external agencies—heat, cold, drying, and antiseptics—to a much greater extent than the fully formed organisms, and it is probable that those diseases in which the contagion remains dormant for long periods are transmitted through spores capable of existing for long periods outside the body. But in small-pox it is not necessary to rely upon spore-formation to support theories of aërial transmission. The contagion as given off from the body of the patient is inclosed in minute epithelial scales and dry pus accumulations. Here, protected from the air and from external destructive agencies, it may be wafted as a minute dust through the air, to descend at considerable distances. That the radius of infection from a small-pox hospital as a centre does not exceed a mile may be due to the great dilution of the contagion as it is diffused through greater distances than a mile from its centre of origin, the hospital. The observations of Dr. Miquel, at the observatory of Montsouris near Paris, have shown the number and variety of solid particles which are carried in the air, and the immense distances which some of them, as pollen and spores, may be presumed to have travelled. An educated

public opinion will soon, if it does not already, regard small-pox hospitals as possible centres of infection, and will insist on their removal outside inhabited areas.

The compulsory notification of infectious diseases to sanitary authorities, either by the householder in whose house the case occurs, or by the medical attendant, or by both, has been adopted in numerous provincial towns during the last five years. This measure has done much to furnish the authorities with early information of the occurrence of infectious disease which would not otherwise have been obtained, and such information has doubtless enabled the sanitary officials to stamp out many an epidemic in the bud, which might otherwise have reached large dimensions. The more universal adoption of a measure of compulsory notification in our large towns is urgently needed.

In the domain of domestic sanitation the advances of recent years have been mostly limited to the practical applications of sound principles already acquired to the carrying out of works of construction, drainage, or water-supply of the dwelling. Houses built for the use of the well-to-do classes (not those of the speculative builder) in recent years will most generally be found to be planned and fitted on modern sanitary principles. Thorough ventilation of the drain and soil-pipe, disconnection of the waste-pipes of baths, sinks, and lavatories, and of the overflow-pipes of cisterns from the drainage system, are now understood to be necessaries of modern life. A break in the connection between the house-drain and the public sewer by means of a manhole chamber and water-seal or trap, though not considered necessary or desirable by all, is now very usually practised. We cannot doubt that the air of a public sewer is sometimes the means of disseminating disease, and any method which practically excludes such a source of danger from our houses is one to be encouraged. As knowledge extends, the simplest form of apparatus is found to be the best; many of the more complicated kinds of traps and contrivances for excluding sewer air are now discarded by builders and architects for those simpler forms which are equally effective.

In the matter of water-supply, the belief is steadily gaining ground that a water once polluted by sewage cannot be regarded as safe for drinking purposes. Safe it may be so long as filtration on the large scale is efficiently performed, but any failure to thoroughly filtrate and aërate the water in times of epidemic visitation might be attended with disastrous consequences, even supposing that filtration through sand and gravel is destructive of disease organisms or their spores. The introduction of a constant supply of water into towns, in the sense that cisterns and receptacles for storing water are no longer necessary, has been of great benefit—especially in the poorer parts of towns, where water stored on the premises is usually highly contaminated.

Of the scientific witnesses who were examined before the Royal Commission on Metropolitan Sewage Discharge, nearly all were in favour of the principle of separation of the rainfall from the sewage. "The rain to the river, the sewage to the soil." In view of the ultimate disposal of the sewage, the advantages of the "separate method" are very great, and would now probably lead to its adoption in any new scheme of sewerage for a town where the circumstances are favourable. From the public health point of view, it is also desirable to have impermeable pipe or brick sewers of small size, so that contamination of the soil by leakage into it of the contents of sewers may be avoided. In any such scheme of sewerage it must not be forgotten that not only are channels on the surfaces of the streets and roads required to convey away surface water, but pervious drains laid in the subsoil are absolutely necessary in the health interests of the town to keep the subsoil water at a permanently low level. For the disposal of the sewage, the value of a regular daily flow, and the elimination of the necessity in times of heavy rain of dealing with an enormous and uncontrollable volume of dilute sewage, must be obvious. The surface waters of towns are certainly not clean, but where the streets are efficiently scavenged they are free from taint of human excretal refuse, and fit for admission into the rivers which nature intended as drainage channels of the surrounding high lands.

The extreme importance of thoroughly ventilating sewers, is now very generally understood. Pipe sewers require as much ventilation as brick sewers, although the absence of deposit on the smooth internal surfaces of the pipes, and their consequent freedom from smell due to decomposition of deposited organic detritus, originally led to the belief that ventilating openings were not required in pipe systems of sewerage. It was not until Dr. Buchanan showed in the case of Croydon that the absence of proper ventilation in the pipe sewers of that town was in all probability instrumental in aiding the spread of enteric fever that the opinion of engineers on this matter underwent a change. Displacement of air in pipe sewers of small diameter is greatly more sudden than in brick sewers of larger diameter, and it is plain, says Dr. Buchanan, that "means of such ventilation are wanted more numerously in proportion as the displacements of air may be local and sudden." Openings into sewers from the street level are still regarded as the best practicable means for the admission of fresh air, and the exit of sewer air. Charcoal trays, Archimedean screws, and other contrivances for purifying the issuing air, or hastening its exit, are now generally abandoned as useless and inconvenient.

The purification and utilisation of the sewage of towns is a subject of much importance both in its public health and commercial aspects. The idea, so long entertained, that town sewage could by various methods be made to yield a manure which would give rise by its sale to an enormous profit is now exploded. The highest degree of purification, we now know, can only be attained on land naturally suitable from its porosity and other properties, and artificially prepared by extensive under-drainage. The agents which purify sewage in its passage through soil, by converting the nitrogenised organic matters into inorganic salts—nitrates and nitrites of the alkaline and earthy bases, and ammonia—have been discovered to be Bacterial micro-organisms, resident chiefly in the superficial 18 inches of soil, and far more abundant in some soils than in others. Sewage farming has been ascertained to be profitable, under suitable conditions. The sewage must flow from the town to the farm by gravitation—the cost of pumping will neutralise profits from the sale of farm produce; a part of the farm must be laid out as a filter bed, so that the sewage, when not required on the cultivated land or when so dilute from the presence of storm waters as to be inapplicable, may be purified on a small very porous area by the process of intermittent downward filtration. Very few growing crops are benefited by the application of sewage, except the various kinds of grasses, and of these such enormous quantities can be produced that, unless converted into "silage," or utilised on the farm in the production of stock and dairy produce, they may be expected to result in a loss, from the absence of any demand for such large quantities at all periods of the year.

In this country, the sewage farm at Birmingham is probably the best example of what has been done to solve a most difficult problem by the application of sewage to land. Here, the sewage is first freed from its suspended matters by a process of precipitation, a proceeding necessary not only to prevent warping of the land with offensive solid matters, but also to withdraw the metallic salts and acids incidental to the sewage of a manufacturing town, which would be injurious to vegetation. Even this magnificent example of dealing satisfactorily with the most difficult municipal problem of modern times is eclipsed

by the city of Berlin on the Continent. The sewage farms at Berlin have successfully dealt with the sewage of 387,500 people—nearly twice the population of Birmingham—whilst London is still allowing to run to waste an enormous amount of valuable material, at the same time polluting a river—the highway of its commerce to an extent never previously dreamt of.

Processes of precipitating sewage by chemicals are now known to exert only a partially purifying influence. The best process yet discovered can do little more than free the sewage from its suspended matters, allowing all the dissolved constituents of sewage—by far the most valuable portion agriculturally and chemically—to pass away in the effluent. Lime dissolved as lime water, sulphate of alumina, and perhaps proto-sulphate of iron, taken together and added to the sewage in the proportion of not more than 10 to 15 grains to the gallon, are the best, most economical, and most effective precipitants. Other more valuable substances, added to the sewage with the view of increasing the value of the precipitated sludge or manure, are in large proportion lost in the effluent water, and as they do not assist precipitation might just as well be added to the sludge afterwards, if fortification is required. Half-a-crown and no more is the value per ton of the precipitated solids of sewage. This value will generally pay for the cost of their carriage a mile or so in agricultural districts, but no further.

A great improvement in dealing with the semi-liquid sewage sludge has been lately effected. The sludge containing over 90 per cent. of water was formerly allowed to dry in the air or in a drying chamber, and a most intolerable nuisance resulted. It is now possible by means of hydraulic filter-presses to convert the semi-liquid sludge into solid cakes containing 40 to 50 per cent. of water, and in this form it is innocuous to the senses, and can be readily conveyed away by cartage.

The knowledge already acquired demands that now, and in the future, the sewage of towns should, whenever possible, be utilised on land in the production of crops or dairy produce; failing this, the sewage should be freed from its solids by precipitation, and subsequently purified on land laid out as filter-beds, efficient purification, and not the production of crops, being alone aimed at. If application to land is impossible, then precipitating processes alone must be relied on, and where the sewage can be turned into the sea, and effectually got rid of without nuisance, there it may be allowable to waste valuable matter which cannot be utilised except at a cost destructive of all profits from its utilisation.

SALE OF THE JARDINE ORNITHOLOGICAL COLLECTION

THE dispersal of an ornithological collection so large, and of such historic interest, as that formed by the late Sir William Jardine, F.R.S., is an event deserving of notice. The collection was begun more than sixty years since, and was the occupation of half a century's diligent care. From its contents were described, and often figured, a majority of the species treated of in the late baronet's many works, ranging from the "Illustrations of Ornithology," commenced in 1825, to papers in journals of comparatively recent date, and it included a greater number of "type-specimens" than any other that has ever been brought to the hammer.

On Sir William's death in November 1874, it was understood that the collection would be speedily sold, and a strong hope was entertained by ornithologists that it should pass, *as a whole*, into one or other of the great museums of this country. However, this was not to be. The comparatively small "British" portion was, after a time, purchased by the Museum of Science and Art in Edinburgh, a very fitting destination for it; but the rest, consisting of between 8000 and 9000 specimens, remained in the hands of Sir William's heir. At last that gentleman determined to dispose of it by auction, and for that purpose selected Messrs. Puttick and Simpson, the well-known firm of Leicester Square, by whom it was accordingly sold on Thursday, June 17 last. However, the attendance at the sale was but small, and except in a very few instances, the prices obtained were below the average often reached at sales of collections in every way inferior in interest, while not one of the lots attained a price that may be called high. There was a certain competition among a few experts, but even this was not carried to any excess, and as a rule the prizes of the collection were knocked down for very small sums. It is a satisfaction to read, however, that most of the "type specimens" were secured for the British Museum or for that of the University of Cambridge; but few, it is believed, falling into the hands of dealers, and hardly any, as was to be greatly feared, into those of the "plume-trade." The low prices realised were due, no doubt, to the fact that notice of the sale had reached few amateur collectors in time, and added to this was the fact, obvious on inspection, that the sale catalogue supplied very little of the information which collectors require. It was the general impression in the auction-room at the time, and has since been confirmed by the opinion of practical ornithologists, that had the catalogue set forth the special quality of the specimens, and the sale been made known more widely, a very different result would have followed, and something like the competition which attended the great sale of Mr. Bullock's museum in 1819 might have been attained, for collectors are as keen now as ever, and such a chance as this is not likely to occur again to the present generation. The long period, too, which has elapsed since Sir William Jardine's death (recorded in NATURE, vol. xi. p. 74) possibly helped also to divest the sale of his collection from a good deal of the interest which it would have inspired had its dispersal taken place soon after his decease, for memories are short in these days. The agent of the British Museum has to be congratulated for his promptness in recognising and securing at a nominal price for that institution one "type-specimen" (that of Bulwer's petrel), which, not being mentioned in the catalogue nor occurring in its expected place among the other specimens of its family, had escaped the notice of all the other ornithologists who had viewed the collection.

NOTES

AMONG the Colonials on whom honours have been conferred are Dr. Julius Von Haast, F.R.S., who has been made K.C.M.G., and Dr. A. R. C. Selwyn, who has been made C.M.G.; Dr. G. Watt, of the Indian Department of Revenue and Agriculture has been made a C.I.E.

PROF. PAUL WAGNER, on behalf of the Comité Salitero, sends us the following statement as to the result of the nitrate of soda competition. Carrying out the scheme of prizes offered by the Committee of the Saltpetre Producers' Association (Comité Salitero at Iquique, Chili) for the best popular essay treating of the importance of nitrate of soda as a manure, and the best mode of its application, the judges—Prof. L. Grandeau, Nancy (France); Prof. Adolf. Mayer, Wageningen (Holland); Prof. A. Petermann, Gembloux (Belgium); Prof. G. Thoms, Riga (Russia); Prof. Paul Wagner, Darmstadt (Germany); Mr. R. Warington, Rothamsted (England)—have examined the essays sent in, namely, thirteen German, thirteen English, and four French, and have made the following awards:—(1) To the essay with the motto, "Grau, theurer Freund, is alle Theorie," a partial prize of 350*l*. (7000 marks); (2) to the essay with the motto, "Pour pratiquer l'agriculture . . . . " a partial prize of 150*l*. (3000 marks). On opening the accompanying envelopes, the author of the first essay was found to be Dr. A. Stutzer.

Principal of the Agricultural Experimental Station at Bonn; and the author of the second essay, M. A. Damseaux, Professor in the Agricultural Academy at Gembloux. It should be remembered that essays competing for the second part of the prize offered—namely, 500*l.* for the best essay treating of the same subject, on the basis of *new, personal, experimental investigations*—must be sent to one of the above-named judges, on or before January 1, 1887.

THE Local Committee for the Birmingham meeting of the British Association have their arrangements well forward. A considerable contingent of Canadian and other colonial men of science will no doubt be present, and every effort will be made to extend a hospitable welcome to them and to all the members of the Association who may be able to visit Birmingham. The Great Western, the London and North-Western, and the Midland Railway Companies offer exceptional facilities to intending visitors. The Council of the Birmingham and Midland Institute have placed their spacious lecture theatre, with its convenient suite of rooms attached, at the disposal of the Local Committee as a reception-room, officers' and ladies' rooms, &c. The meetings of the Sections will be held in the Council House, the Mason Science College, the Medical Institute, the Birmingham Municipal School of Art, and in the offices of the Board of Guardians—buildings which closely adjoin each other; and the use of rooms in the Council House has also been granted by the Mayor for other purposes. The Town Hall will be utilised for the evening meetings. Various clubs and scientific and literary institutions will be thrown open to members and associates by the courteous invitation of the governing bodies, and the Committee of the Birmingham Botanical and Horticultural Society will open their Gardens during the week of the meeting to members and associates. The Committee are also preparing an extensive exhibition of the products of local industries and of local manufacturing processes, which will be held in Bingley Hall. A collection of the flora and fauna, together with the rocks and fossils of the district, will be shown in connection with this exhibition. The Committee are arranging a series of excursions to various localities of great beauty and interest, and many kind offers of hospitality have been received in connection with the projected excursions. The Committee are engaged in the preparation of a guide-book of the town, which will include an account of its history and antiquities, trade and manufactures, a description of its modern government, papers on the geology and physiography (accompanied by a geological map), and the zoology and botany of the district.

SOME of the friends of the late Dr. Walter Flight are anxious to collect a fund to be invested for the benefit of his widow and children, who have been left with extremely inadequate provision. A Committee has been formed with the Rev. Prof. Bonney, F.R.S., as chairman, to carry out this object. The honorary treasurer is Mr. L. Fletcher, Natural History Museum, Cromwell Road, S.W., and the honorary secretaries T. W. Carmalt Jones, M.D., 6, Westbourne Street, Hyde Park, W., and John M. Thomson, King's College, Strand, W.C. Contributions may be paid to the account of the "Flight Memorial Fund" with Messrs. Robarts, Lubbock, and Co., to the honorary treasurer, or either of the honorary secretaries. We need not say a word to impress upon our readers how deserving is such a case as this.

A MOVEMENT is on foot for obtaining subscriptions to purchase an annuity for Mr. J. B. Dancer, who has done so much for the improvement of photography. But photography is only one of the many arts and sciences indebted to him. There is the stereoscopic camera with twin lenses, which he was the first to make. He invented microscopic photographs, which so much delighted and astonished us twenty-five or thirty years ago. He also introduced photography to the magic lantern, being the first to show a photographic transparency on a screen. The lantern itself is also indebted to him, not only in its optical parts and in its construction generally, but also particularly in the application of the oxy-hydrogen light, and for a dissolving gas tap, which saves half the gases and produces the best dissolving effect. Then there should be mentioned, as of much greater importance than the above, the automatic "contact-breaker," used probably by the million at this moment, in every induction coil in the world. Prior to Mr. Dancer's invention, contact used to be made and unmade by hand, in a vessel containing mercury. The first helical coil with the vibrating interrupter was constructed by Mr. Dancer, and was exhibited long after by him at one of the *soirées* of the British Association, when the meeting was held in Manchester. When Mr. Dancer established himself as an optician in Manchester, his presence soon made itself felt amongst the few microscopists then living in the district. Good microscopes were then costly, and worthless ones very common. Mr. Dancer successively brought out several forms of instruments, as excellent in their mechanical and optical arrangements as they were moderate in price. It is sad to have to say that, notwithstanding Mr. Dancer's talents and achievements, he is now living in very straitened circumstances, is morever afflicted with almost total blindness, and therefore unable to follow the optical business to which his life has been devoted. It is not an unusual thing for a man of great mechanical ingenuity and skill to be an indifferent man of the world, and so it has been with him; as a business man he has been a failure. He has made improvement after improvement, invention after invention, any one of which might in "pushing" hands have made a fortune; but, more interested in science than in money-making, he has allowed the golden chances to become public property, and has thus remained poor himself, while the world has reaped the advantage of his labours. Mr. Dancer is now in his seventy-fourth year, and, it is to be hoped that in his hour of darkness the world will pay back to him something for that which it has freely received at his hands. A Committee has been formed for the purpose of receiving subscriptions, and we commend the movement strongly to the support of our readers. The Committee are: J. P. Joule, LL.D., F.R.S., Sale; Prof. W. C. Williamson, LL.D., F.R.S., Owens College; Prof. Balfour Stewart, LL.D., F.R.S., Owens College; John Dale, F.C.S., Cornbrook, Manchester; Leo H. Grindon, Manchester; S. Platt, J.P., Oldham; Charles Bailey, Hon. Treasurer Manchester Literary and Philosophical Society; James Birchall, Hon. Sec. Liverpool Literary and Philosophical Society; Abel Heywood, jun., Higher Broughton (Hon. Sec. *pro tem.*).

DR. ULLMANN, of Vienna University, who spent several weeks with Dr. Pasteur in Paris, and brought back some of the virus with him, began on Monday, in the presence of several eminent professors, to inoculate against hydrophobia. He had for patients thirteen men bitten by rabid dogs and one woman bitten by a rabid pig.

WE regret to announce the death, on the 23rd ult., at his residence, Glenoir, Galway, of William King, D.Sc., Emeritus Professor of Geology, Mineralogy, and Natural History in the Queen's College, Galway, in his seventy-eighth year. Upon the foundation of the Queen's Colleges in Ireland, in 1849, Dr. King was selected to fill the Chair of Geology in the Galway College, a post that he occupied, and of which he fulfilled the duties most assiduously and laboriously, until 1883. In that year, owing to a severe attack of paralysis, Dr. King was most reluctantly obliged to relinquish his professorial duties. In 1882 the additional performance of the business of the Natural History Chair devolved upon him: the double task proved too onerous. Subsequent to his resignation, the Corporate Body of the College presented Dr. King with an address, as a testimony

of their esteem for his devoted services, of which latter the Geological Museum in the College is a lasting record. Numerous volumes and also pamphlets upon various branches of geology bear witness to his attainments and indefatigable zeal.

FROM the report just issued by the Swedish Academy of Sciences we gather some interesting particulars of the scientific work prosecuted under the auspices of this Institution last year. On the recommendation of the Academy the following sums were granted by the Government towards scientific research, &c. :—A sum of 150*l*. to the Academy's Zoological Station in the province of Bohus; 200*l*. to Prof. H. Gyldén for the development of his theory respecting the movements of the larger planets; 250*l*. towards the publication of the *Acta Mathematica;* 300*l*. towards the purchase of a zoological and ethnographical collection of objects brought by Dr. C. Bovallius from the West Indies and Central America; 60*l*. towards Prof. Liljeborg's work, "Scandinavian Fishes"; and two sums of 50*l*. each towards Herr Westerlund's work, "Fauna der in der palàarctischen Region lebende Binnenconchylien," and Dr. Lindeberg's exsiccate work on the Rubi of Scandinavia. In addition to these sums, various smaller amounts were granted by the Academy to a number of gentlemen towards scientific researches, as, for instance, for the study of certain algæ on the coast of Bohus, for the study of the Scanian moss flora, for the study of the anatomy and histology of the marine Annelidæ in the same province, and that of the Gasteropoda, &c. Of other scientific work continued last year may be mentioned the work on the great publication recording the scientific of the expedition to Spitzbergen, 1882–83, of which the researches on the aurora borealis and the electricity of the air are now in the press, and the rest, on other branches of science, will shortly follow; and further, the arrangements of the valuable collection for the State Museum of ethnographical objects from all parts of the world—some 6000 in number—made by Dr. Stolpe during the voyage of the corvette *Vanadis* round the world. The Academy also purchased a large estate near Stockholm with the funds bequeathed by Prof. Bergius for the establishment of a horticultural garden, similar to that of Kew, to be under the supervision of the Academy. An important change has been decided on with reference to the publishing of the Academy's *Journal*, viz. to divide it into two parts—Proceedings and Appendix—the former to be issued monthly, containing reports of meetings and short papers and the latter to be issued at intervals, containing longer and more scientific papers; and these will be divided into four sections, each embracing a certain branch of science, which will enable a specialist to find at once the paper desired, and not necessitate the purchase of the whole yearly series.

AN Australasian Meteorological Society has been formed at Adelaide, South Australia.

ON Edison's system of telegraphing with trains in motion, the *Scientific American* (February 20, 1886) says:—The receiving apparatus at both the car end and the fixed end of the line is a telephon·. The sending apparatus is also similar at both ends, and consists of an interrupter or vibrating tongue driven by an independent battery, and making 500 vibrations per minute; this vibrator is in circuit with the line battery, an ordinary Morse key, and the primary of an induction coil. The secondary of the induction coil on the car is in connection with the tin covering the entire roof of one or more cars; the secondary coil at the fixed station is in connection either with condensers or with other induction coils, which in turn are in connection with the ordinary line wires by the side of the track. Suppose a message to be sent from the fixed station to the car. The vibrator is always working, but till the Morse key is put down no current passes. The message is sent by the ordinary Morse signal, only instead of a continuous current being sent to line each time, it is an alternating one; this induces a current in the secondary coil, and through it the condensers, for example, are charged alternately. The charge of the condensers is propagated through the line wires with which they are in connection, and influences the tin roof of the car, and ultimately the telephone by which the signals are read.

MR. BLANFORD, Meteorological Reporter to the Government of India, has issued a memorandum on the Himalayan snowfall in the past season. A few years ago, it will be remembered, Mr. Blanford propounded a theory of a connection between this snowfall and the monsoon, to the effect that the later and heavier the snowfall in winter and spring the later and feebler would be the following monsoon. The forecasts based on this theory were fairly accurate last year, and accordingly his forecast this year was looked forward to with anxiety on account of the great value of early and copious rain to Indian agriculture. This year Mr. Blanford arrives at the conclusion that, although a considerable amount of snow fell in the North-Western Himalayas and the hills of Eastern Afghanistan, during the winter and spring, especially in January and February, there has been on the whole less than in the previous year. The snow range, as seen from Simla, is less thickly covered than it was in 1885, and the snow is at a higher level. The winds have been less northerly than usual on the west coast, and more decidedly southerly and easterly in the Punjaub. Hence he thinks that there will be no retardation of the monsoon on the Bombay side; and the barometric levels are favourable to an advance of the easterly branch of the monsoon, so that no apprehension need be felt about the rains in Upper India.

AT the meeting of the Royal Society of Tasmania on April 13, the Curator of the Museum stated that during the past month Mr. Vimpany had captured a black snake (*Hoplocephalus curtus*) at Longley, measuring about 4 feet 3 inches in length. On opening it the unprecedented number of 109 young ones were found in her. The specimens now before the meeting are the largest ones, the measurement being from $8\frac{1}{2}$ inches to $\frac{3}{4}$ of an inch in length. Mr. Morton stated that the greatest number he had known previously to be taken from a similar snake was 32, but he had been informed by a resident of Tasmania that over 70 had been taken from a similar species.

IN the *Stonyhurst Magazine* for May 1886, is a list of the flora of the Stonyhurst district. It contains a list of all plants of whose occurrence within a radius of ten miles from Stonyhurst satisfactory record can be found.

FROM a communication by M. Nikolsky to the St. Petersburg Society of Naturalists (vol. xvi. 2), it appears that the drying up of Lake Balkhash is going on at a very rapid rate, and so far as the observations of the inhabitants may be relied upon, its level is lowered by no less than two feet every ten years. The maps of 1852 show that a very great reduction of the surface of the lake has taken place during the last thirty years. As to the fauna of Lake Balkhash M. Nikolsky makes the following interesting remarks. It does not include a single species of those fishes which are characteristic of the Aral-Caspian ichthyological region. On the other hand, there is a very great resemblance between the fishes of Lake Balkhash and the lakes on the high plateau of Central Asia, for instance, of Lob-nor. Three species are common to the Ili River and the Tarim, tributary of Lob-nor. M. Nikolsky concludes from the ichthyological data that there is no ground to admit of any direct connection between Lake Balkhash and Lake Aral. If there ever was a sea which covered the Siberian lowlands as well as the depressions of Lake Balkhash and the Aral-Caspian, the Balkhash was

separated from the latter at a period when the connection between the Aral-Caspian depression and the Arctic Ocean still existed. There was certainly, in recent geological time, a connection between the rivers of the Balkhash basin and those of the Lob-nor basin, which connection probably followed the Kunges, the Yulduz, and the Tarim Rivers.

THE total area of the Crown forests of Sweden at the beginning of 1885 was 5,785.535 hectares, being a seventh part of the total forest area of the country. The revenue from the same was a little less in 1885 than in 1884, but this is believed to be only incidental, it having risen from 750,000*l.* in 1880 to 890,000*l.* in 1882, and 1,120,000*l.* in 1884.

WE have received part 2, vol. iv. of the *Transactions* of the Norfolk and Norwich Naturalists' Society, containing the papers selected for publication and the address read by the President, Major Feilden, F.G.S., at the seventeenth annual meeting of the Society. From the report it appears that the Society now numbers 260 members, and is both numerically and financially in a very satisfactory condition. For his address Major Feilden chose the fascinating problem of the origin of life, which he strove to show must have had its advent at the poles of the earth, a subject which his study of the fossil and recent fauna of the polar regions as naturalist to the Arctic Expedition of 1875-76 gave him special opportunities of studying. The conclusions to which Major Feilden arrives are that through the secular cooling of our planet the poles became first fitted for the reception of life; that in palæozoic times the North Pole possessed a climate as warm, at least, as that now enjoyed at the equator; that the temperature at the North Pole during the Miocene period, though gradually cooling, supported a flora which spread southwards; and that in all probability animal life likewise originated at the poles, and spread towards the equator. Amongst the published papers, one by Mr. Clement Reid, F.G.S., on the "Flora of the Cromer Forest Bed," is of especial interest. Mr. Reid enumerates sixty species of plants, which he has obtained by the careful washing of clays from various localities near Cromer, and calls attention to the curious fact that all these, with the exception of *Trapa natans*, three firs, and *Isoetes lacustris*, are still indigenous to the county of Norfolk, and two-thirds of them are aquatic or marsh plants, identical in species with those found at the present day in almost all the Norfolk morasses. Mr. Edward Bidwell contributes an account of a visit to the Isles of Scilly in the nesting season of 1885; the Rev. H. A. Macpherson a paper on the habits and plumage of the Manx shearwater; Mr. G. Smith some notes on the habits of the Fulmar petrel; meteorological notes by Mr. A. W. Preston; notes on the herring fishery of 1885, by Mr. Southwell; a second paper, by Mr. Reid, on Norfolk amber; and a list of the birds of Norfolk, with remarks, by Messrs. Godney and Southwell; also a valuable paper on the gradual assumption of the adult plumage in the honey buzzard, by Mr. J. H. Godney.

WE have received a "Liste Alphabétique" of the Correspondence of Christian Huygens, which the Dutch Society of Sciences proposes to publish. The list may be obtained from Enschedé and Son, Haarlem.

DR. G. F. MARTINEAU, of Yorke House, Stourport, writes with reference to the article "On the Origin of our Potato," in NATURE of May 6 last, p. 7, that in turning over, the other day, the leaves of the sumptuous "Hortus Eystertensis" of Basil Besler, printed in Nuremberg in 1613, he found an excellent plate of the plant (of which he sends a tracing), with a clear and full description. Certainly it is quite worth while to draw attention to Besler's figure and text, but it tells one nothing new. There are good figures in Gerarde, 1597, and Clusius, 1601, Besler's being 1613. "The potato," another correspondent writes, "is not *wild* in Virginia; it must have been carried there from Peru and Chili. The only wild United States potatoes are high up in the Rocky Mountains. A. De Candolle's idea is that the potato was first brought to Europe, not by the English, but by the Spaniards."

THE additions to the Zoological Society's Gardens during the past week include a Chimpanzee (*Anthropopithecus troglodytes* ♀) from West Africa, presented by Capt. Reginald E. Firminger; a Rhesus Monkey (*Macacus rhesus* ♂) from India, presented by Mr. G. Ballentyne; a Macaque Monkey (*Macacus cynomolgus* ♀) from India, presented by Mrs. S. M. Grove-Grady; a Banded Ichneumon (*Herpestes fasciatu*) from West Africa, presented by Mr. G. F. Stimpson; two Egyptian Geese (*Chenalopex ægyptiaca*) from Africa, presented by Col. Harris Burland; a Larger Hill-Mynah (*Gracula intermedia*) from India, presented by Miss Maud Bendall; a Martinique Gallinule (*Ionornis martinicus*) from South America, presented by Mr. W. J. Rae; Aldrovandi's Skinks (*Plestiodon auratus*) from North-West Africa, presented by the Hon. Walter de Rothschild; a Geometric Tortoise (*Testudo geometrica*), a Semiserrated Tortoise (*Testudo semiserrata*), an Angulated Tortoise (*Chersina angulata*), two Dwarf Chameleons (*Chamæleon pumilus*), two Keeled Euprepes (*Euprepes carinatus*), a Spotted Slowworm (*Acontias meleagris*), a Bipes (*Scelotes bipes*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; a Sand Lizard (*Lacerta agilis*), European, presented by Master Stanley S. Flower; two Ruffs (*Machetes pugnax*), a Common Viper (*Vipera berus*), British, deposited; a Silver-backed Fox (*Canis chama*) from South Africa, twelve Black-tailed Godwits (*Limosa ægocephala*), European, purchased; a Thar (*Capra jemlaica*), a Pigmy Hog (*Porcula salvania*), twelve Mandarin Ducks (*Æx galericulata*), a Chilian Pintail (*Dafila spinicauda*), a Red-crested Pochard (*Fuligula rufina*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

BLACK TRANSIT OF JUPITER'S FOURTH SATELLITE.—The fourth satellite of Jupiter was observed in black transit by Mr. E. E. Barnard of Nashville, Tenn., U.S.A., on May 8 with a 6-inch refractor. It was first noticed as a black spot at 9h. 20m. local mean time. Some little time previous to this it had been looked for on the disk but could not be seen either as a white or dark spot. The satellite was followed until 9h. 43m., and was then very black and rather small and round when best seen.

COMETS BROOKS I. and III.—The following ephemeris for Comet Brooks I. is by Dr. A. Berberich (*Astron. Nachr.*, No. 2731):—

For Berlin Midnight

1886	R.A. h. m. s.	Decl. ° ′	Log *r*	Log Δ	Brightness
July 3	8 11 33	8 43·6 S.	9·8945	0·1128	2·0
7	8 37 32	9 49·2	9·9405	0·1400	1·4
11	9 0 24	10 37·4	9·9811	0·1680	1·0
15	9 20 32	11 13·6	0·0172	0·1957	0·8
19	9 38 22	11 41·5	0·0498	0·2227	0·6
23	9 54 15	12 3·9	0·0794	0·2487	0·5
27	10 8 30	12 22·7	0·1064	0·2734	0·4
31	10 21 25	12 39·1 S.	0·1314	0·2969	0·3

The brightness on April 29 is taken as unity.

Comet Brooks III. is now very faint, and will be soon altogether out of sight. Dr. S. Oppenheim gives (*Astron. Nachr.*, No. 2735) the following places for Berlin midnight on July 4 and 8:—

	R.A. h. m. s.	Decl. ° ′
July 4	13 18 40	16 42·6 S.
8	13 29 17	18 48·3 S.

NOVA ORIONIS.—The new star discovered by Mr. J. E. Gore near χ_1 Orionis appears for some unexplained reason to be a difficult object for photometric observations, the estimates of its magnitude made by various observers differing remarkably. Thus Dr. G. Müller found it a little brighter than the 6th magnitude

in the last days of December 1885—December 19, 5·86 m.; December 20, 5·76 m.; December 30, 6·00. Profs. Glasenapp and Pritchard both found it considerably fainter than the 6th at this time, the former giving it as 6·7 m. on December 30, the latter 6·42 m. on December 28. Profs. Müller and Pritchard give closely accordant results for the middle of January 1886, the magnitude being about 6·8 m., whilst Prof. Glasenapp and Mr. Gore found it about 7½ m. at the same time. Profs. Pritchard and Müller disagree a little later on, and differ by a full magnitude at the end of February and beginning of March, the former regarding the star as about the 7th magnitude, the latter about the 8th, whilst MM. Glasenapp and Gore consider it as nearly the 9th. There is a better agreement amongst three of the observers as to the range of magnitude through which the star has passed; Dr. Müller and Mr. Gore, agreeing in giving 2·4 m. for the change from about December 20 to March 8, and Prof. Glasenapp finding nearly the same value, but Prof. Pritchard, on the other hand, only finds a change in the same period of about seven-tenths of a magnitude.

10 SAGITTÆ.—Mr. Espin, in *Circular* No. 5 of the Liverpool Astronomical Society, gives the interval from maximum to minimum for this star as 4·4d.; maxima for July, 1·6d., 9·9d., 18·3d., 26·6d.; minima, 6·1d., 14·4d., 22·7d., 31·0d.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *JULY* 4–10

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on July 4

Sun rises, 3h. 52m.; souths, 12h. 4m. 6·4s.; sets, 20h. 16m.; decl. on meridian, 22° 53′ N.: Sidereal Time at Sunset, 15h. 7m.

Moon (three days after New) rises, 7h. 7m.; souths, 14h. 36m.; sets, 21h. 53m.; decl. on meridian, 13° 19′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	5 42	13 39	21 36	20 33 N.
Venus	1 36	9 24	17 12	19 13 N.
Mars	11 14	17 16	23 18	0 23 S.
Jupiter	10 53	17 6	23 19	1 44 N.
Saturn	3 53	12 2	20 11	22 30 N.

July	h.	
4	2	Saturn in conjunction with the Sun.
7	7	Jupiter in conjunction with and 0° 33′ south of the Moon.
7	13	Mars in conjunction with and 2° 1′ south of the Moon.

Variable Stars

Star	R.A. h. m.	Decl. ° ′			h. m.
U Cephei	0 52·2	81 16 N.	July	4,	0 33 *m*
			,,	9,	0 13 *m*
Algol	3 0·8	40 31 N.	,,	5,	3 16 *m*
R Bootis	14 32·2	27 14 N.	,,	8,	*M*
δ Libræ	14 54·9	8 4 S.	,,	10,	22 40 *m*
U Coronæ	15 13·6	32 4 N.	,,	8,	2 57 *m*
S Herculis	16 46·7	15 8 N.	,,	4,	*m*
R Ophiuchi	17 1·2	15 56 S.	,,	4,	*M*
U Ophiuchi	17 10·8	1 20 N.	,,	7,	0 40 *m*
X Sagittarii	17 40·4	27 47 S.	,,	10,	2 0 *M*
β Lyræ	18 45·9	33 14 N.	,,	9,	21 30 *M*

M signifies maximum; *m* minimum.

NATIONAL SMOKE ABATEMENT INSTITUTION[1]

DURING the year the interest in the subject of smoke prevention and in improved apparatus for the consumption of fuel has been steadily increasing, and the gradual extension of knowledge on the subject has led the general public to take a much more intelligent and active interest in the question of smoke abatement, which was at first considered by the great majority of the community to be almost a sentimental evil rather than a matter entering into the calculation and care of ordinary life. The Council regret, however, to note that the Annual Report of the Commissioner of Police, issued in August last, is strangely deficient with regard to information as to the operation of the Metropolitan Smoke Abatement Acts, which are administered by the police; and the Council thought it their duty to write to the *Times* and other daily papers, calling attention to this want of information in the Report, and also to the very anomalous character of the fines inflicted in the case of convictions; they also laid the matter before the Home Secretary, calling special attention to the following facts—

(1) That in numerous cases of nuisance which are reported by the police no proceedings are taken.

(2) That when proceedings are instituted, and convictions obtained, the penalties inflicted by the magistrates do not comply with the Acts of 1853-56, the average fine being below the legal minimum.

(3) That no proceedings whatever appear to be taken to enforce the abatement of smoke from steamers, &c., on the River Thames, although an enormous quantity of smoke is evolved by them, causing a very serious nuisance, not only in the waterside districts, but by polluting the general atmosphere of the metropolis.

(4) That such great development has taken place during the last few years in the methods of preventing smoke from the works falling under the provisions of the statutes, that they may be more rigidly enforced without hardship.

(5) That the area within which the Smoke Abatement Acts apply no longer corresponds with the area within which smoke is produced.

The Council were supported in thus calling the attention of the Home Secretary to the matter, by the fact that the Annual Report of the Commissioner of Police for the preceding year (1883) remarks strongly on the inadequacy of the fines, and states that, "The fact of recent changes in heating systems having brought about some very considerable commercial advantages of various kinds, has operated in a marked degree in mitigating hostility to the enforcement of the Acts."

The Council have also, through the medium of the Press, called attention to the fact that the London School Board are neglecting a public duty and losing a valuable opportunity of instructing the public, by having the large buildings recently erected for schools fitted up with heating apparatus without due regard to their smoke-consuming capabilites.

They have also endeavoured to influence public opinion by bringing under notice pledges which appear to have been given by some Parliamentary candidates, that they would endeavour to exempt bakers from the operation of the Smoke Abatement Acts, this pledge having been obtained by certain bakers who wished to maintain the use of a particular class of furnaces which ordinarily produce a large amount of smoke. It is scarcely necessary to point out that the exemption of bakers from the operation of the Smoke Nuisance Acts would be prejudicial to the public interest, as it is a fact that smoke can be and is in some bakeries entirely prevented, not only to the advantage of the public, but also to that of the men who work in the bakeries. The Parliamentary candidates themselves were also communicated with upon the subject.

The unreasonableness of the suggestion that bakers should be exempt from the provisions of the Smoke Acts is the more noticeable from the fact that the Commissioner of Police, in his Annual Report for 1883, alluding to the general improvement of heating methods, says: "The most important changes perhaps have been made in the case of bakers' and confectioners' oven furnaces, which have hitherto caused, and still continue to cause, the greatest number of offences charged under the Smoke Acts. Some of them are now adapted by a simple alteration, which can be made without stoppage of the daily trade, to the use of gaseous fuel (ordinary coal gas mixed with atmospheric air), instead of coal; while other ovens are heated by coke applied either directly to the purpose, or by steam, which is generated in pipes heated by means of coke-fired furnaces."

It may be added that the Council have had before them an offer from a good firm of oven builders, stating that they are prepared to fit up fifty bakers' ovens at half price, to prove the practical working of one system rendering such ovens entirely smokeless.

In various trades, notably baking confectionery, tile and porcelain burning, glass staining, japanning, &c., considerable advantages, in addition to the prevention of smoke, have been found to result from the use of coal gas instead of solid fuel for

[1] Report of Council of the National Smoke Abatement Institution, submitted at the ordinary general meeting, December 18, 1885.

furnaces and engines, but to obtain the same result the cost of gas is greater than that of coal. Although the directors of the gas companies of the Metropolis are apparently not unwilling to advance the cause of smoke abatement, and thereby of public sanitation, by making a reduction in the price of gas used for trade purposes, they are prevented doing so by their Acts of Parliament. The Council are keeping the matter in view, and watching a favourable opportunity to urge the Government to grant the necessary powers.

Correspondence has been carried on on the subject of the gas stoves at the Bank of England, insisting on the necessity of flues being provided to carry off the products of combustion from all gas stoves used for warming purposes, and letters have been received thanking the Institution on behalf of the clerks for calling the attention of the Bank authorities to the matter. Voluminous correspondence has also been carried on with makers and inventors of stoves and smoke-prevention appliances, and of patent fuels, and with others, giving information and suggestions on points connected with the subject too various to be set out.

During the year several tests have been carried out by the Institution, and they have now under consideration the preparation of another volume of detailed reports of tests. The volume would include tests of various forms of furnaces, steam and other boilers, blow-pipe furnaces, smoke-preventing appliances, ventilating fans, non-conducting compositions, mechanical stokers, condensers, gas cooking and heating stoves, and various heating and cooking appliances using gas and coal as fuel.

The Council had at one time intended to exhibit at the Parkes Museum typical forms of heating and smoke-abatement appliances, but for various reasons they considered it undesirable to carry out the scheme, and they propose instead to promote periodical exhibitions of special heating apparatus, or new methods of heating and smoke prevention, as opportunity may offer.

In connection with this branch of the subject, reference may be made to the exhibition of the Sanitary Institute held at Leicester in September, at which various stoves and smoke-preventing appliances were exhibited. Exhibitions of gas stoves for heating and cooking purposes have also been held in many of the chief provincial towns during the year.

A memorial, praying for a grant from the surplus funds of the International Health Exhibition, signed by the Duke of Westminster and other influential persons, was unavailing, although the object of this Institution so directly affects public health, the improvement of which was the avowed aim of the Health Exhibition. This is much to be regretted, as the lack of funds not only militated against the general operations of the Institution, but it prevented the Council establishing a testing department, which is a necessary adjunct to the Institution for the advancement of its objects.

During the year a lecture given in the Parkes Museum by Mr. T. Fletcher, of Warrington, on Smoke Abatement, and a pamphlet containing three prize essays on the same subject, have been printed by the Institution and circulated. A paper by Mr. W. R. E. Coles, on the Hygienic, Moral, and Economic Aspects of the Smoke Question, read at the Leicester Congress of the Sanitary Institute, is now being prepared for circulation.

By order, E. WHITE WALLIS,
Secretary

THE WINGS OF BIRDS[1]

THE power of flying through the air is one of the principal characteristics of the class of birds. Although some members of the other great divisions of the Vertebrates—the bats among Mammals, the extinct pterodactyle among Reptiles, the flying-fishes among Pisces—possess this power in a greater or less degree, these are all exceptional forms, whereas in birds the faculty of flight is the rule, its absence the exception. Among Invertebrates this power is possessed in a very complete degree by the greater number of insects.

In the normal structure of the vertebrate animals there are two pairs of limbs, anterior and posterior, never more. It often happens, however, that one pair, and sometimes both, are suppressed, being rudimentary, functionless, or entirely absent. Flight is always performed by the anterior or pectoral pair, more or less modified for the purpose. The super-addition of wings to arms, as in the pictorial representations of angels, has no counterpart in nature. The wings of the bird, the bat, the pterodactyle, and flying-fish, are the homologues of the arms of man, the fore-legs of beasts. In the flying-fish the power is gained simply by an enlargement of the pectoral fin, and the function is very imperfect; in the pterodactyle, by immense elongation of one (the outer) finger, and extension of the skin between it and the side of the body; in the bats, by elongation of the four outer fingers, and extension of a web of skin between them and the body. In the bird the flying organ is constructed mainly of epidermic structures, peculiar outgrowths from the surface, called *feathers*—modifications of the same tissue which constitutes the hair, horns, scales, or nails of other animals. Feathers are met with only in birds, and are found in all the existing members of the class, constituting the general covering of the surface of the body.

The framework to which the broad expanse formed by the feathers is attached is composed of bones, essentially resembling those of the fore limb of other Vertebrates. The distal segment, manus, or hand, in the vast majority of birds, has three metacarpal bones and digits, the former being more or less united together in the adult state. The digits appear to correspond with the pollex, index, and medius of the typical pentadactyle manus; the second is always the longest. Both it and the pollex frequently bear small horny claws at their extremity, concealed among the feathers and functionless, but very significant in relation to the probable original condition of the avian wing. These claws are altogether distinct from the large, and often functional, spurs developed in many species from the edge of the metacarpal bones, resembling both in use and situation the corresponding weapons in the hind-feet The third digit does not bear a second phalanx or claw in any existing bird.

The quills, remiges, or flight-feathers attached to the bones of the manus (called "primaries"), never exceed twelve in number, and are (as has been recently shown by Mr Wray) in the very great majority of birds distributed as follows:—Six, or in some few cases (flamingo, storks, grebes, &c.), seven to the metacarpus; of the remainder or digital feathers, one (*ad-digital*) is attached close to the metacarpo-phalangeal articulation, and rests on the phalanx of the third digit; two (*mid-digital*) have their bases attached to the broad dorsal surface of the basal phalanx of the second digit, which is grooved to receive them; the remainder (*præ-digital*) are attached to the second phalanx of the same digit. These last vary greatly in development, in fact their variations constitute the most important structural differences of the wing. In most birds there are two; the proximal one well developed, the distal always rudimentary; but the former may show every degree of shortening, until it becomes quite rudimentary, or even altogether absent, as in *Fringillidæ* and other "nine-primaried" birds, in which there are six metacarpal remiges, one ad-digital, two mid-digital, and no præ-digitals, or only a very rudimentary one. The smaller feathers at the base of the quills, called upper and under coverts, have an equally regular arrangement. The webs or vanes of all the flight-feathers are made up of a series of parallel "barbs" which cohere together by means of minute hooklets, and so present a continuous, solid, resisting surface to the air.

Such is the characteristic structure of the wing in almost all carinate birds, whether powerfully developed for flight, as in the eagles, albatrosses, or swifts, or whether reduced in size and power to practically useless organs, as in the extinct great auk, the dodo and its kindred, weka rail, notornis, cnemiornis, &c., most of which, being inhabitants of islands containing no destructive land mammals, appear to have lost the principal inducement, and with it the power, to fly.

In the penguins (*Sphenisconorphæ*) the feathery covering of the wing entirely departs from the normal type. Each feather is like a flattened scale frayed out at the edges, the barbs are non-coherent and have no hooklets. They form an imbricated covering of both surfaces of the wing, including the broad patagium which extends from the cubital side of the limb, but appear to have no definite relation to the bones, and cannot be divided into distinct groups, corresponding to those described above. The structure of the wing separates the penguins sharply from all the other carinate birds.

The Ratitæ, or birds without keel to the sternum, form another very distinct group, distinguished by the rudimentary or imperfect condition of the remiges or quills, which never have coherent barbs, and are therefore unfitted to the purpose of flight. In the ostrich and rhea the bones, though comparatively

[1] Abstract of Lecture by Prof. W. H. Flower, LL.D., F.R.S., at the Royal Institution. February 19, 1886.

small, are distinct and complete, and the feathers large and definitely arranged. The emu, cassowary, and apteryx show various degrees of degeneration, which apparently culminated in the dinornis, no trace of a wing-bone of which bird has ever been found. The question which naturally presents itself with regard to these birds is, whether they represent a stage through which all have passed before acquiring perfect wings, or whether they are descendants of birds which had once such wings, but which have become degraded by want of use. In the absence of palæontological evidence it is difficult to decide this point. The complete structure of the bony framework of the ostrich's wing, with its two distinct claws, rather points to its direct descent from the reptilian hand, without ever having passed through the stage of a flying organ. The function of locomotion being entirely performed by powerfully developed hind-legs, and the beak mounted on the long flexible neck being sufficient for the offices commonly performed by hands, the fore-limbs appear to have degenerated or disappeared, just as the hind-limbs of the whales disappeared when their locomotory functions were transferred to the tail. This view is strengthened by the great light that has been thrown on the origin of the wings of the flying birds by the fortunate discovery of the *Archæopteryx* of the Solenhofen beds of Jurassic age, as in this most remarkable animal, half lizard and half bird, the process of modification from hand to perfect flying bird is clearly demonstrated. The three digits which in the existing forms are more or less pressed together and imperfect, still retain their freedom and complete number of phalanges, and are each armed with terminal claws, while the flight feathers and remiges of the cubital, metacarpal, and digital series are fully developed and evidently functional. The earlier stages in which the outer digits were still present, and the feathers imperfectly formed or merely altered scales, are not yet in evidence.

Some conception of the process by which a wing may have been formed may also be derived from the study of the growth of feathers on the feet of some domestic varieties of pigeons and poultry, illustrations of which were shown at the lecture.

THE SUN AND STARS[1]

VII.

WE have now to endeavour to apply to the more distant stars some of the facts which I have brought before you touching the nearest one—our sun. What we have to do in the short time at our disposal is to choose those facts which will give us the greatest amount of knowledge concerning the greatest number of those stars.

When the star that is nearest to us has set, the number of stars which a pair of eyes can see on a dark night, whether they happen to be north of the equator or south of it—for the number of stars is pretty equally distributed north and south—is something under 3000. But when we leave behind us the power of the unaided eye, and consider what results can be obtained by the optical means now at man's disposal, we have to increase these 6000 to something like forty or fifty millions, so that, if we can by any chance obtain facts touching one star that are applicable to others, we do a great deal. We are, in fact, dealing with 50,000,000 bodies instead of one.

The first thing regarding these distant bodies to which I have to draw attention is that they have been divided for purposes of convenience—astronomical and other—into magnitudes such that the first magnitude means the brightest star we can see; and so we go on till now we go down to the sixteenth magnitude.

The order of diminution of brightness is not quite exact from the first magnitude to the faintest visible to the naked eye, but it may be taken on the average to be about two-fifths. If we take this ratio as the normal one down to the sixteenth magnitude we get the following values nearly:—

$2\frac{1}{2}$	stars 2nd mag.	=	1 star	1st mag.
6	3rd	=	,,	
16	4th	=	,,	
40	5th	=	,,	
100	6th	=	,,	
⋮	⋮			
10,000	11th	=	,,	
⋮	⋮			
1,000,000	16th	=	,,	

[1] A Course of Lectures to Working Men delivered by J. Norman Lockyer, F.R.S., at the Museum of Practical Geology. Revised from shorthand notes. Continued from p. 45.

We not only get the stars thus visible, but, as they can be photographed in a certain period of time, this period measures their photographic brightness. We find, for instance, that a first magnitude star can be photographed in the three-thousandth part of a second; that a star of the seventh magnitude can be photographed in about one second; and when we come to the twelfth magnitude we must turn seconds into minutes, and we shall require two of them to get an impression on the plate; till, working on gradually to the sixteenth magnitude, we find that the photographic plate, which requires only the three-thousandth part of a second for a star of the first magnitude, requires one hour and twenty-three minutes (or eighty-three minutes) to receive the impression, we find the ratio of two-and-a-half times to be practically indicated by the times of exposure.

The relative photographic light of stars of all magnitudes when the most rapid dry plates are used is shown in the following table:—

Magnitude	Time of exposure m.	s.
1st		0·005
2nd		0·013
3rd		0·03
4th		0·08
5th		0.2
6th		0·5
7th		1·3
8th		3·0
9th		8·0
10th		20·0
11th		50·0
12th	2·0	
13th	5·0	
14th	13·0	
15th	33·0	
16th	83·0	

We must not for one moment imagine that, because for many reasons it has been necessary to divide stars into magnitudes, all the stars are of exactly the same size at different distances, or of different sizes at the same distance. We know very little at present relatively. But this we do know, every new fact has shown us that some of the apparently fainter stars may be very large, and some of apparently the brightest stars may be small. You can understand that the light which we get from the stars will depend upon these two things. Take the case of the sun for instance. We know that the sun is a small star, and yet it gives us a great deal of light because it is near to us. We know that some of the other stars are very distant, and they give us a small amount of light, not because they are small, but because they are so far away.

We are living now in a very interesting time, because people are beginning to work here and there, not in too many places, to get the stars to write their own autobiography, so to speak. In fact, a very important attempt is being made at the present moment to replace observations of the positions of the stars by actual photographs. Observations, you know, being human, are always liable to error. This plate, which I am about to show you, is a photograph that I have received from the Brothers Henry of Paris only this morning, showing what photography can do in registering the exact positions and brightnesses of an almost innumerable army of stars by simply exposing a plate in a telescope.

If it is wished to obtain photographs of stars of the sixteenth magnitude, the plate will have to be exposed eighty-three minutes. If we are content to get stars of the seventh magnitude, then two minutes will be enough.

All the stars that you see here are visible in a very restricted portion of the sky in the constellation Cygnus, not very far from the Milky Way. You can understand what a happy thing it will be for the astronomer of the future if, when he wants to know the state of the heavens in this nineteenth century, instead of having to consult musty books of observations which may probably be wrong, he can refer to a book of which the leaves are made of glass, and on which is recorded the autobiography of every square degree of the heavens as you see on this diagram before you.

In our attempt to apply to these other bodies the knowledge which we have acquired touching the sun, of course we have to consider chiefly the light sent to us by them. You will see in a moment that if the sun were very much farther away from us than it really is—imagine it for a moment so far away that

instead of appearing to us with a disk it should appear to us as a star, like Sirius or Capella, for instance—the only difference between its spectrum now and its spectrum then would be that there would be less of it. There would be less light. Consequently it would not be possible for us to see it in all its exquisite detail. But so far as the spectrum went there would be no change in kind, although there might be a change in degree.

Now, if you just assume that for a moment, you see that we shall be in a very fair way to make a very important application of this knowledge, because I was careful to tell you that in the solar system we have indications of a considerable amount of absorption of blue light; so that, if the sun's atmosphere were away and the earth's atmosphere were away, the sunlight, if we are now right in calling it white, would then certainly appear to us as blue, for the reason that the blue light now stopped by the sun's atmosphere and by our own would then be added to the light which we get at the present moment, and the total light therefore received by our eyes would be very much richer in blue rays than it is at present.

Now then, having the fact of this blue absorption in our minds, let us suppose it—to begin with the simplest case—to be enormously increased. Let the blue absorption creep on into the spectrum till at last it reaches the green or the yellow or the red. It is clear that then the sun that we should see would be a red sun, and that sunlight would be no longer white, but red.

Let us next, on the other hand, reduce the quantity of the existing blue absorption. Let us have a solar spectrum as long as the spectrum of the electric light, for instance.

Now let us do something else. Let us suppose that in the solar spectrum, as in very many of the spectra that we can observe in our laboratories, there is superadded to this blue absorption a strong absorption of the red, beginning at the other end of the spectrum. We shall get the yellow and the red, say, absorbed on the one side of the spectrum, while we get the blue and violet absorbed on the other. We shall therefore only get the green light to pass.

Do we get evidence that in the heavens among other stars such conditions as these hold? Certainly. A very considerable number of the stars in the heavens are called coloured stars. They are red, or they are blue, or they are green, for the most part, and you see that simply dealing with the absorption of the blue with which we have become familiar in the case of the sun, playing with it a little, giving it a little rope here, shortening the rope there, and adding another exactly equivalent absorption at the other end of the spectrum, we can at once account simply and sufficiently for the colours of the coloured stars. This is one advantage that we have in working from the known to the unknown. If we had begun with the stars and dealt with their phenomena first, it would have been difficult to explain; but now that we know how a thing happens in the case of the sun, it is quite easy for us to imagine the mechanism which must be at work in the atmosphere of the coloured stars to give us in some cases red suns, in others green suns, and in others still blue suns.

So much then for coloured stars.

There is another matter. As I shall have to show you by and by, one of the most important distinctions between the stars in the heavens is one not depending upon their magnitudes, not depending upon their distances, or upon their mass, or upon anything of that kind, but depending upon conditions which we do not know very much about at present, but which bring about this result, that the spectrum in one case is different from the spectrum in another, exactly as in our laboratories we find the spectra of bodies with which we are perfectly acquainted become different if the temperature which we employ is made to differ. For instance, in the case of the vapour of carbon we may employ a low temperature, and get a certain spectrum of the vapour which is called a spectrum of flutings. If we increase the temperature, and then again observe, the flutings have disappeared. They have given way to a system of lines in which the irregularity is just as striking as the exquisite rhythm of the flutings was in the former case. From hundreds of these observations the student of spectrum analysis is not afraid to say that when he sees a spectrum of flutings he knows that he is dealing with the action of vapours at a much lower temperature than exists in those conditions in which the flutings are replaced by lines. And, more than that, so definite is this, so much do we know about the fluted spectra of those substances which exist in the solar atmosphere—giving us, at the temperature of the sun, the line spectrum—that it is easy for us to take the responsibility also of saying that, if the sun's atmosphere were to be suddenly cooled to-morrow, we should get a spectrum of flutings, instead of a spectrum of lines; so that when we get, if we do get, the fluted spectrum in the spectrum of a star, we are justified in saying that some cause has been at work in that star equivalent to a cooling process in the atmosphere of our own star. Thus, if we cooled the sun to-morrow we should produce the spectrum of flutings, and as in cooling down the sun will in all probability pass through a stage indicated by flutings, so also while it was acquiring its present temperature it passed through the same stage.

What, on the other hand, would happen if we had the sun very much hotter to-morrow? It is important to think this out very carefully. According to the views which I have brought before you, we have, outside all, solids absorbing every part of the spectrum. Then we have liquids and dense vapours doing the same: less dense vapours absorbing the red, and finer vapours still absorbing the blue. We have flutings also, but chiefly we have vapours at an enormous temperature which give us the familiar absorption spectrum of Fraunhofer lines.

We have the Fraunhofer spectrum in short giving us the summation of the line absorption of every stratum in the sun's atmosphere. We have also a wonderfully simple spectrum of the chromosphere, of which I gave you the list of lines, writing down for us the absorption of the hottest part of the sun's atmosphere that we can get at.

Now try to think this out quite completely.

The first obvious thing which will strike us is that, if the sun could be made hotter to-morrow than it is to-day, the thing that we should be quite certain about, whatever might happen to the other conditions, would be that the gases which give us that simple spectrum of the chromosphere would have a larger share in the absorption-spectrum, and that therefore the absorption-spectrum of the star would gradually get nearer and nearer to the absorption-spectrum which would be given by the chromosphere itself if it could be seen in all its simplicity. I think that way of reasoning is right. Well, if you think it is, you will find that it will lead us to a very interesting conclusion. If we find any star with practically the spectrum of the chromosphere, we shall be bound to admit that the atmosphere of that star must be hotter than the *average* temperature of the atmosphere of our sun as its spectrum approaches that of the *hottest* part of the sun's atmosphere.

There is one other point that I have to bring before you before I go further, and it is this. We have had a great deal to say about the photosphere of the sun and the surrounding envelopes. We saw that when any vapours were located between our eye and the bright sun in the centre we then got absorption-lines, for the reason that the sun was hotter than the vapour on this side of the sun, so to speak, and therefore light was stopped by the cooler vapour in the atmosphere, and we got a dark line. The moment however, we work outside the disk, and study a prominence on the limb of the sun, or even a part of the corona, we observe them by means of their bright lines—by means of their radiation. There is no hotter light source behind them, and therefore we deal simply with radiation.

Now, that being so, you will understand how it is that in the general spectrum of the sun all the lines are dark, because we found that while the bright central part of the sun was not very much less than the whole volume, something like a tenth, it was very much hotter, so that we get many thousand times more light from the centre of the sun. If a substance in the outer atmosphere gives us a bright line corresponding with a dark line given us from this central portion due to the atmospheric absorption, all it can do is to reduce the intensity of the dark line produced by the intensely illuminated central portion.

It is a question of area. The difference of area is small, smaller than the difference of illumination, and therefore anything which happens outside does not get its record written at all, the area being five or six to one, and the intensity of the light in the centre being, say, ten thousand to one.

Now let us consider another case. Let us suppose that there is a star (never mind which it is) the atmosphere of which is so enormous that its diameter to the diameter of the central photosphere is represented by two concentric circles—one very large, the other very small. Here the difference of area between the inner circle, which gives us dark lines, and the larger exterior space, which gives us bright lines, if it gives us anything, is so enormous that it may be greater than the difference of the intensity of the light; so that if the inner light is ten times brighter than the light which comes from the outer

area, which, let us say, is a couple of hundred times greater, in that case we shall be bound to have bright lines from the exterior regions mixing with the dark lines coming from the interior regions. Hence we see that the spectra which we may get from stars will not depend upon the diameter of the stars at all, but may depend upon the difference of area simply which we should get by cutting a section at right angles to the line of sight from the earth through the star and its whole atmosphere.

It comes to this: Suppose some stars have very large coronal atmospheres; if the area of the coronal atmosphere is small compared with the area of the section of the true disk of the sun, of course we shall get an ordinary spectrum of the star; that is to say, we shall get the indications of absorption which make us class the stars apart; we shall get a continuous spectrum barred by dark lines. But suppose that the area of the coronal atmosphere is something very considerable indeed, let us assume that it has an area, say fifty times greater than the section of the kernel of the star itself; now, although each unit of surface of that coronal atmosphere may be much less luminous than an equal unit of surface of the true star at the centre, yet if the area be very large, the spectroscopic writing of that large area will become visible side by side with the dark lines due to the brilliant region in the centre where we can study absorption; other lines (bright ones) proceeding from the exterior portion of that star will be visible in the spectrum of the apparent *point* we call a star.[1]

Those things, then, being premised, we are now in a position to approach the subject of stellar spectra. Much work is now being done in this direction, but we must not forget the early workers. We must not forget that it was Fraunhofer at the beginning of this century who first saw and carefully observed several spectra of stars, and we must be all the more careful to remember that, since really more than half a century passed before anybody took the trouble either to repeat his observations or to extend them. Some twenty years ago, however, several observations had been brought together by the labours of Italian and American men of science (scarcely a stellar spectrum had been observed in England). This enabled a distinguished American, Mr. Rutherfurd, to begin to put a little order into the facts which had so far been acquired.

He pointed out that it was easy to arrange these stars into classes—that all the spectra were not alike. There was a wonderful family likeness among three groups of them, and he showed that you might divide these spectra into three very definite classes. After him came two countrymen of our own, Dr. Huggins and Dr. Miller, who, when they did begin their work, certainly put into it an amount of vigour and assiduity which had never been approached before their time. They not only gave us careful drawings of the spectra of the stars which they

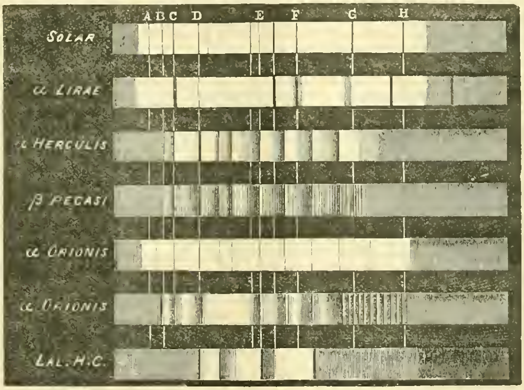


FIG. 21.—Various types of stellar spectra.

observed, but with infinite care and patience they made comparisons, as we may say, to determine the origin of the lines in exactly the same way as I have pointed out that Kirchhoff, Ångström, and Thalén discovered the origin of the lines in the spectrum of the sun. Indeed, they did not rest here, or rather, one of them did not rest here, for Dr. Huggins subsequently introduced a system of photography, and now, thanks to his skill, we have several photographs, of priceless value, of some of the brighter stars. And while I am lecturing to you here in London there is one observer in Berlin, Dr. Vogel, and another in the north of Europe, Dr. Dunop, doing all they can to give us a complete and perfect spectroscopic catalogue of every star that shines in the northern heavens, so that you can see that the work is going on.

Now, before I say any more about it, I will refer to a diagram which gives an idea of the kind of thing that one sees when these observations are being made.

We will just run through them one by one. There is a very rough and general view of the spectrum of the sun. The actual spectrum of the sun has been thrown on the screen before you, and therefore it will be quite understood that there we have a very rough copy of it for diagrammatic purposes, indicating merely the most obvious among the Fraunhofer lines. When we pass from the sun to α Lyræ, we pass from a star having a relatively large number of lines to one having a small number; and this small number of lines is further remarkable from the fact that the lines are much thicker than those seen ordinarily in the solar spectrum. Keeping to the stars which give us spectra of lines, here in α Orionis we get another case in which the lines do not occupy the places occupied by lines in the spectrum of the sun, nor, at the same time, are they so thick as the lines in stars of the Lyra type. We can also learn from this diagram, by the examination of the spectra of α Herculis and β Pegasi, that we get flutings from stars as well as lined spectra. We also see that these flutings are not all exactly in the same place, by which we can infer that the flutings are not all probably of the same chemical origin. Of that further by and by. The use of the diagram is to give a general idea. J. NORMAN LOCKYER

(*To be continued.*)

SCIENTIFIC SERIALS

The American Journal of Science, June.—The Biela meteors of November 27, 1885, by H. A. Newton. From a general survey of the observations made in various places, the author infers that the maximum of the shower was about 6h. 15m. Greenwich mean time; that the total hourly number of meteors visible at one place in a clear sky was at the utmost 75,000; that the densest part of the stream was not over 100,000 miles in thickness; that the meteors of November 27,

[1] *Proc.* Roy. Soc., No. 185, 1878.

1872 and 1885, did not leave the immediate neighbourhood of the Biela comet earlier than 1841-45, and may be treated as having at that time orbits osculating that of the comet.—The ultra-violet spectrum of cadmium, by Louis Bell. The ultra-violet spectrum of cadmium having long served as a standard of reference in the measuring of other spectra, an attempt is here made to determine its principal wave-lengths more accurately than is possible by Cornu's ingenious process. By taking photographs on Stanley instantaneous dry plates, Mr. Bell believes the wave-lengths here determined will be found correct to probably within 1/50,000 part of their respective values. The total number of lines accurately determined in the entire spectrum was thirty, of which the wave-lengths are tabulated with the corresponding figures obtained by Hartley and Cornu.—Communications from the United States Geological Survey, Rocky Mountains Division. The present communication (No. vii.) deals with the occurrence of topaz and garnet in lithophyses of rhyolites, and is contributed by Mr. Whitman Cross, who had already described the occurrence of minute crystals of topaz in the small drusy cavities of a coarsely crystalline rhyolite from Chalk Mountain, by Fremont's Pass, Colorado. The present specimens of topaz and small dark red garnets are from the trachyte on the Arkansas River, opposite Nathrop, Chaffee County, Colorado. The mode of formation of the topaz and garnet in the lithophysal cavities of the rhyolite in this district is not fully determinable, but they are evidently not secondary, but primary products, produced by sublimation or crystallisation from presumably heated solutions contemporaneous, or nearly so, with the final consolidation of the rocks.—On the strain-effect of sudden cooling exhibited by glass and by steel, by C. Barus and V. Strouhal. The experiments here described confirmed the views already announced by the authors, that the annealing of steel, considered physically, is at once referable to the category of viscous phenomena; also that the existence of the characteristic strain in glass-hard steel is the cause of electrical effects so enormous, that any additional effects caused by any change of carburation may be disregarded, and the electrical and magnetic results interpreted as due to variations in the intensity of the said strain. The chief results here arrived at have since been substantiated by polariscope evidence and by the investigation of the density of the consecutive shells of the "Prince Rupert drop." An account of these results will be given in their next paper.—Upon the origin of the mica-schists and black mica-slates of the Penokee-Gogebic iron-bearing series, by C. R. Van Hise. The iron-bearing formation of this region extends for over 80 miles from Lake Numakagon in Wisconsin to Lake Gogebic in Michigan; and at Penokee Gap, Wisconsin, the series is 13,000 feet thick, the upper 11,000 feet being mica-schists and black slates. The Muscovitic and biotitic greywacke, biotite-schists, and other formations here described furnish a graded series from the slightly altered greywackes to the crystalline mica-schists.—On two masses of meteoric iron of unusual interest, by Wm. Earl Hidden. One of these specimens, found on July 2, 1885, on a height to the east of Batesville, Independence County, Arkansas, weighs 94 lbs., and belongs to the class holosiderite of Brezina. It is specially remarkable for a hole piercing it near the edge, and cone-shaped from both sides. Analysis yielded: iron, 91·22; phosphorus, 0·16; nickel and cobalt, 8·62 by difference. The other, found in 1857 in Laurens County, South Carolina, weighs only 4 lbs. 11 oz., but is noted for the perfection of the Widmanstätter lines and unusual abundance of nickel and cobalt. Analysis: iron, 85·33; nickel, 13·34; cobalt, 0·87; phosphorus, 0·16, with trace of sulphur.—Notice of a new genus of Lower Silurian Brachiopoda, by S. W. Ford. This nearly perfect specimen of the ventral valve of the species described by E. Billings under the name of *Obolella desiderata*, and now preserved in the collection of Walter R. Billings, Ottawa, may be taken as the type of a new genus, probably including several described Lower Silurian species. It differs from *Obolella* in the form and arrangement of its muscular impressions, in the possession of a thinner shell and in other respects. The author, therefore, proposes for it the new generic name of Billingsia in honour of Mr. E. Billings, the late eminent palæontologist of the Canadian Geological Survey.

Bulletin de l'Académie Royale de Belgique, April 3.—Determination of the remainder in Gauss's quadrature formula, by M. Mansion. By a definite integral the author completes this formula, which thus becomes applicable to non-parabolic curves.—On some remains of cetaceans from the foot of the Caucasus, by M. P. J. Van Beneden. These remains, comprising portion of a skull with some vertebræ from the district east of Vladikavkas, and an almost perfect vertebral column, with ribs, radius, and humerus from the bed of the Kuban River, all belong to the same species, the *Cetotherium rathkei*, Brandt. By their means the author is enabled to determine the true characteristics of the Cetotherium, which shows some affinity to the Pachyacanthæ of the basin of the Danube, but was quite distinct from the extinct species of the Antwerp basin.—On some rocks dredged off the Ostend coast, by M. A. F. Renard. These include granites, porphyries, diorites, &c., such as occur along the French seaboard and in the Channel Islands; also Jurassic and Chalk formations identical with those of Boulogne and the cliffs of Dover. There is nothing to show that any of these rocks have been transported either from the south or from the Scandinavian regions during the Glacial epoch.

Bulletin de la Société des Naturalistes de Moscou, 1885, No. 1.—Revision of the numerical values of the repulsive force, by Prof. Th. Bredichin. In his preceding researches the author had determined it approximately by means of the rough formula of Bessel. Now, he corrects these results, either by direct evaluations by means of more exact formulæ, or indirectly by means of the isodynames constructed upon his rigorous formulæ. Taking 40 different comets (since 1472) M. Bredichin classifies them under three different types, and, on the former method, receives for the first type, $R = 14$, while the initial speed (due to the ejective force) varies between $g = 0·1$ and $g = 0·34$, the average being 0·22; for the second type, $R = 1·1$, and $g = 0·05$ (varies between 0·03 and 0·07); and for the third type, $R = 0·2$, and $g = 0·1$ to 0·2.—On the oscillation of the emissive of comets, by the same (with a plate). From a careful study of the comet 1862 III. the learned professor concludes that the oscillations of its emission ought to be considered beyond doubt, as they result not only from measurements, but also from all the *ensemble* of phenomena afforded by the head and tail of the comet.—Third report upon my herbarium, by Ed. Lindemann (in German).—Plantæ Raddeanæ Monopetalæ (continuation of Labiatæ), by Ferd. Herder.—Letters from Dr. A. Regel dated from Bokhara, Merv, &c., between May 1884 and April 1885.—Notice of a journey to Akhal-Tekke, by A. Becker, with a list of plants found at Kyzyl arvat.—On northern *Aucellæ*, by H. Trautschold.

No. 2.—Enumeration of the vascular plants of the Caucasus, by M. Smirnoff, continued from the preceding issue, and forming an introduction to the flora of the Caucasus.—Birds of the Transcaspian region, by M. Zaroudnoi.—Thirty-five years of observations on the earliest and latest times of blooming of wild and cultivated plants in the neighbourhood of Kishineff, by A. Dœngingk, followed by remarks on vegetable parasites and noxious insects. Four hundred plants are on the lists of the author.—Revision of the copulatric armatures of the males from the *Philecemide* tribe, by Gen. Radoszkowski (with two plates).—The appendix contains the third part of the systematic catalogue of the herbarium of Moscow University, published by Prof. Goroshankin.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 13.—"On the Structure of Mucous Salivary Glands." By J. N. Langley, M.A., F.R.S., Fellow and Lecturer of Trinity College, Cambridge.

The cells of mucous salivary glands I have previously described as consisting of a framework or network, containing in its spaces hyaline substance and granules. The granules of the mucous salivary glands are rendered very distinct by irrigating a mounted specimen of a fresh gland with moderately dilute solutions of neutral or alkaline salts. In these fluids the granules can scarcely be distinguished from small fat globules; those of the submaxillary gland of the dog have a diameter of 1 to 2 μ. In the resting gland the granules are fairly closely packed throughout the cell, in a line stretching from basement membrane to lumen; there are 8 to 12 granules. Both hyaline substance and granules give rise to mucin.

During secretion both the hyaline substance and the granules are turned out of the cells; after prolonged secretion the cells consist of an outer zone, chiefly of freshly-formed substance, and of an inner zone of network, hyaline substance, and granules, as in the resting state. When the saliva has a high percentage of

solids, both the hyaline substance and the granules can be seen in it. The hyaline substance is more soluble than are the granules, and is thus less commonly seen; it is partly dissolved, partly swollen up into a continuous mass; the less swollen parts appear as strings or blebs. The granules in saliva vary greatly in appearance; they may be very slightly swollen, and have fairly sharp outlines; or they may be more swollen and run together, forming pale masses of various size; occasionally in more dilute saliva they are just visible as pale spheres.

Although the mucous cells are able to turn out bodily their products, they do not disintegrate during secretion. As the decrease in the interfibrillar substance takes place, there is a fresh formation of substance in the outer part of the cells, *i.e.* as the cell secretes it also grows. In saliva there is no evidence of broken-down cells, nor are nuclei present except those in "salivary corpuscles," which, as stated by Pflüger, are leucocytes. Further, there is not any satisfactory proof that the demilune cells multiply during secretion, and give rise to mucous cells. During secretion there is no increase in the number of nuclei undergoing indirect division. As I have previously said, I hold the demilunes to be secreting cells of a different nature from that of the mucous cells. Glands with demilunes are simply glands in which the "albuminous" element is reduced to a minimum. The apparent increase in size of the demilunes, described by Lavdowsky as taking place in the first stage of secretory activity, I take to be due to the decrease in the size of the alveoli, so that the ordinarily flat demilunes become more spherical. Moreover, the demilune cells show signs of secretory activity. The "young" cells described by Heidenhain and by Lavdowsky are chiefly altered mucous cells.

The network of the cell consists of two parts—one in the cell-membrane, the other stretching from this throughout the cell. The peripheral network consists of very delicate fibres; at some of the nodal points there are small spherical swellings. From lumen to basement membrane there are twelve to fifteen meshes. The internal network is connected with the peripheral network, but it appears to me to have much larger meshes. From basement membrane to lumen there are in the submaxillary gland of the dog four to six meshes, *i.e.* the number of meshes in a given direction in the cell is about half that of the number of granules.

May 27.—"A General Theorem in Electrostatic Induction." By John Buchanan, B.Sc.

Part I. of this paper deals with the effect of change of the specific inductive capacity of a dielectric which is placed in a field of electric force, and it is proved that in general, under these circumstances, the dielectric becomes electrified.

By translating the theorem into the language of magnetism a theorem in magnetic induction is obtained.

The mathematical proof leads to an expression of the form

$$h = -\left(\frac{d\pi}{dV} - V\frac{d^2\pi}{dV^2}\right),$$

where h denotes the rate of change of the apparent electrification of the dielectric with regard to the specific inductive capacity as independent variable; π denotes the rate of change of the work done against electrical forces with regard to the same independent variable; and V denotes the potential.

The conditions that there may be no electrification of the dielectric are next obtained. The result is arrived at that, in order to have no electrification, when the specific inductive capacity is altered, the whole field of force must be occupied by an electrically homogeneous dielectric. It is then pointed out that the equations obtained express the effect of heterogeneity in the constitution of the dielectric medium.

In Part II. the above theorem and some of the results obtained by Dr. Kerr in his experiments in "electro-optics," are applied to obtain a theory of electrification by friction.

The discussion leads to these conclusions:—

"Positive" liquids tend to become positively electrified by friction.
"Negative" ,, ,, negatively ,, ,,
"Positive" solids ,, negatively ,, ,,
"Negative" ,, ,, positively ,, ,,

All these conclusions are verified by the experimental results given in the paper.

June 10.—"Fluted Craterless Carbons for Arc Lighting." By Sir James N. Douglass.

On December 8, 1858, at the South Foreland High Lighthouse, and with the direct current magnetic machines of Holmes, the first important application of the electric arc light, as a rival to oil and gas for coast lighting, was carried out by the Trinity House, under the advice of Faraday.

The carbons then used, and for several years afterwards, were sawn from the residuum carbon of gas retorts; they were square in section, $6\frac{1}{4} \times 6\frac{1}{4}$ mm., and the mean intensity of the arc measured in the horizontal plane was 670 candle units, being 17 candle units per square millimetre of cross-sectional area of the carbon. The crater formed at the point of the upper carbon of the "Holmes" lamp was so small that no appreciable loss of light was found to occur, and the arc proved to be very perfect in affording an exceptionally large vertical angle of radiant light for application with the optical apparatus.

The most reliable and efficient machine that has yet been tried for lighthouse purposes is the large size alternate current magneto machines of De Meritens. The average results with these machines are as follows, viz.:—

	One machine	Two machines supplying current to one lamp
E.M.F.	38 volts	48 volts.
Mean current	206 amperes	372 amperes.
Carbons (cylindrical) ...	35 mm. diam. ...	50 mm. diam.
Diameter of crater in carbon	13 mm.	18 mm.
Mean intensity of arc measured in the horizontal plane (candle units) ..	15,000	30,000
Light per square millimetre of carbon section (candle units)	12	12

It will be observed from this statement that the intensity of the arc in the horizontal plane per square millimetre of sectional area of carbon is about 35 per cent. less than it was with the small square carbons used by "Holmes," although it might reasonably be expected that with the improvements since effected in the manufacture of carbons, the efficiency of the old carbons would at least be maintained. The relative inefficiency of the large carbons used with the powerful currents now available appears to be due (1) to the loss of a large portion of the most intense part of the arc which is confined within the crater of each carbon; and (2) to the fluctuations in the intensity of the arc caused by the current passing between various points of the end of each carbon.

For a new electric light installation about to be made by the Trinity House at St. Catharine's Lighthouse, Isle of Wight, it is intended to utilise the large De Meritens machines that were used at the recent South Foreland experiments for determining the relative merits of electricity, gas, and oil as lighthouse illuminants. The electric light at St. Catharine's is intended to be "single-flashing" at periods of 30 seconds. Each flash is to have a duration of $5\frac{1}{2}$ seconds, and to be followed by an eclipse of $24\frac{1}{2}$ seconds. It is intended to use one De Meritens machine during clear weather, and two machines whenever the atmosphere is found to be so impaired for the transmission of light that the flashes are not reaching their intended range.

The defect here arose which is common to all electric flashing lights where a minimum and a maximum intensity of flash are adopted, viz. that the duration of the flashes of minimum and maximum intensity would vary in the ratio of the difference in the diameter of the carbons employed with one and two machines respectively, which in this case should be 50 mm. and 35 mm., this mean difference amounting to $36\frac{1}{2}$ per cent. nearly. It is evident that such a variation in the duration of flash would seriously impair the distinctive character of the signal.

It occurred to me, however, that, if carbons of a fluted cross-section were employed, the carbons for minimum and maximum intensity could be made of corresponding diameter, their sectional areas being proportioned to the minimum and maximum currents employed, and thus the flashes of minimum and maximum intensity would have exactly the same duration. As all carbons for electric arc lights are now made in moulds, I saw that such a form would not involve any more difficulty in manufacture than if made cylindrical, while there would be less liability of internal fracture occurring, as is often the case with large carbons in the process of drying and baking. Other advantages to be obtained with fluted carbons are: (1) a larger vertical angle of radiant light from the arc, and with a higher coefficient of in-

tensity in consequence of the unobstructed radiance through the fluting at the points of each carbon; and (2) a steadier light is obtained owing to the localising of the current at the central portion of each carbon.

The result of many experimental trials with fluted carbons 50 mm. diameter have entirely confirmed my expectations. No crater is formed in either of the carbon points, and their form is all that can be desired for utilising fully the maximum light of the radiant arc. My experiments have not been sufficient to determine accurately the additional intensity of light obtained from the arc of a pair of the fluted carbons as compared with that from the arc of a pair of cylindrical carbons, but I am of opinion that the gain with fluted carbons is not less than ten per cent.

Geological Society, June 9.—Prof. J. W. Judd, F.R.S, President, in the chair.—The following communications were read:—On the volcanic rocks of North-Eastern Fife, by James Durham, F.G.S., with an appendix by the President. After describing the general distribution of the volcanic rocks of Old Red Sandstone and Carboniferous age in the counties of Forfar and Fife, the author called attention to a fine section exhibited where the Ochil Hills terminate along the southern shore of the Firth of Tay. In immediate proximity to the Tay Bridge, a series of the later volcanic rocks, consisting of felstones, breccias, and ashy sandstones are found let down by faults in the midst of the older porphyrites (altered andesites) which cover so large an area in the district. The breccias contain enormous numbers of blocks of a red dacite (quartz-andesite), and inclosed in this rock angular fragments of a glassy rock, resembling a "pitchstone-porphyry," are found, everywhere, however, more or less converted into a white decomposition-product. The youngest igneous rocks of the district are the bosses and dykes of melaphyre (altered basalt and dolerite) which have been often so far removed by weathering as to leave open fissures. In the appendix three very interesting rocks were described in detail. The rock of the Northfield Quarry, which is shown to be the augite-andesite, has a large quantity of a glassy base with felted microlites, and contains large porphyritic crystals of a colourless augite. The rock of the Causewayhead Quarries is described as an enstatite-andesite; it has but little glassy base, being made up of lath-shaped felspar crystals (andesine), with prismatic crystals and grains of a slightly ferriferous enstatite; there are no porphyritic crystals, but the enstatite individuals are sometimes curiously aggregated. The red porphyritic rock from the breccias near the Tay Bridge was shown to be a mica-dacite, and the glassy rock associated with it to be the same material with a vitreous in place of a stony base. The glassy base exhibits very beautiful fluidal and perlitic structures. The crystals of first consolidation in this rock are oligoclase and biotite, often showing marks of injury in transport; those of the second consolidation appear to be orthoclase. In conclusion, the successive stages by which the andesitic rocks of the area were altered, so as to assume the characters distinctive of porphyrites, were fully discussed, as well as the change of the glassy rock into its white decomposition-product.—On some eruptive rocks from the neighbourhood of St. Minver, Cornwall, by Frank Rutley, F.G.S. The rocks described in this paper were derived from Cant Hill, opposite Padstow, and from a small quarry about half a mile from Cant Hill, near Carlion. At the former locality the volcanic rocks are much decomposed, but from their microscopic characters they may be regarded as altered glassy lavas of a more or less basic type. No unaltered pyroxene, amphibole, or olivine is to be detected in the specimens described, but there is a considerable amount of secondary matter which may include kaolin, serpentine, chlorite, palagonitic substances, &c. There is evidence of fluxion-structure in some of the sections; others are vesicular, and the vesicles are usually filled with siliceous or serpentinous matter. The relation of these lavas to the underlying Devonian slates was not ascertained. The rock occurring near Carlion contains numerous porphyritic crystals of augite in which the crystallisation is interrupted by the co-development of small felspar crystals, which appear, as a rule, to have been converted into felsitic matter. Ilmenite is also present in patches which indicate a similar interrupted crystallisation to that shown by the augite. The rock has the mineral constitution of an augite-andesite, but since it is a holocrystalline rock, exception would be taken by many petrologists to the employment of the term andesite. The lavas of Cant Hill were also probably of an andesitic character, so that, so far as original mineral constitution is concerned, there is some apparent justification for the mapping of both of these rocks as "greenstone" by the Geological Survey.—The Bagshot beds of the London Basin, by H. W. Monckton, F.G.S., and R. S. Herries, B.A., F.G.S. The authors stated that their object was to describe more fully the Lower Bagshot beds, and to disprove the view lately advanced by Mr. Irving that, in certain places, the Upper Bagshots overlap the Lower, and rest directly on the London Clay. They described or referred to a number of sections all round the main mass, beginning at St. Ann's Hill, Chertsey, where they considered that the mass of pebbles and associated greensands must be referred to the Middle Bagshot. The outliers near Bracknell and Wokingham were shown to consist of Lower and Middle Bagshot, which does not appear in the valley north of Wellington College. The Aldershot district was explained, and it was shown that the beds there resting on the London Clay were Lower and not Middle Bagshot, and the occurrence of fossils in the Upper Bagshot of that district was recorded. The conclusions that the authors came to were, that a well-marked pebble-bed was almost always present, marking the division between the Upper and Middle Bagshots, but that there were other pebble-beds of a less persistent character occurring both in the Middle and Lower Bagshots; that the Lower Bagshots generally consist of false-bedded sands with clay laminæ and no fossils except wood, whereas the Upper Bagshots are rarely false-bedded, and are characterised by the absence of clay bands and the presence of marine fossils; and that the Middle Bagshot is a well-marked series consisting of greensands and clays. They claimed, in conclusion, that there was no reason for disturbing the old reading of the district, and that there was no evidence of an overlap of the Lower Bagshots by the Upper.

Physical Society, June 12.—Dr. J. H. Gladstone, Vice-President, in the chair.—Dr. Samuel Rideal and Mr. E. C. Wellington were elected Members of the Society.—The following communications were read:—On an electric-light fire-damp indicator, by Messrs. Walter Emmott and William Ackroyd. The Royal Commission on Accidents in Mines point out in their recently-issued report, as a serious objection to the use of the electric light in mines, notwithstanding its many great advantages, that the light of an incandescent lamp, being produced within a vacuum, cannot admit of any device for the indication of fire-damp such as is given by the Davy, for example. The present apparatus is the outcome of an attempt to overcome this difficulty. It consists of two incandescent lamps, one with colourless and the other with red glass, and the circuit is so arranged that in an ordinary atmosphere the colourless lamp alone shines, but in fire-damp this goes out, and the red one is illuminated. This is effected in a simple manner by the motion of a mercury contact occupying the lower part of a curved tube, one end of which is open, and the other connected with a porous pot of unglazed porcelain, the motion of the mercury being due to the increased pressure in the porous pot occasioned by diffusion.—On a method of distinguishing rays of solar from those of terrestrial origin, by Prof. Cornu. It has been shown by M. Fizeau that, owing to the rotation of the sun upon its axis, there is a displacement of the spectral lines produced by solar absorption towards the red or towards the violet, according as to whether the light examined emanates from those parts of the sun which are receding from or approaching us. If, however, the lines are the result of absorption by the earth's atmosphere no such displacement should occur. It has been the aim of the author to make this principle the basis of a simple and instantaneous method of determining the origin of any given line. The displacement is very minute, amounting to about 1/150 of the distance between the D lines for rays in that part of the spectrum when the light is from the extremity of the solar equator, but it has been found quite sufficient. Observations have been made with a Rowland grating, the mean distance of the lines being ·00176 mm. An image of the sun is formed upon the slit of the spectroscope by a lens. By a slight oscillatory motion given to the lens by a lever from the hand, any part of the sun's image can be brought upon the slit. A heliostat sends the rays always in the same direction, and by a prism the image has its equator horizontal. To distinguish between a line of solar and one of terrestrial origin the line is brought near the vertical wire of the eye-piece, or, better still, one of those inevitable grains of dust which are always seen on the horizontal wire. The lever connected to the lens is then oscillated so as to bring alternately the two ends of the solar equator tangentially upon the slit. If the ray is of terrestrial origin it remains abso-

lutely fixed, if it is solar it oscillates with the lever.—On a hyperbolagraph, by Mr. H. H. Cunnynghame. It is not an unfrequent want to be able to find a rectangle of greatest or least area contained between a curve and rectangular co-ordinate axes. In several problems connected with motion and pressure in steam-engines this is useful, and even in political economy the graphic representation of monopoly curves depends on maxima and minima of this nature. For the solution of such problems it is often very useful to be able to describe rectangular hyperbolas, and the author has devised a machine to effect this. It depends on a mathematical property of the rectangular hyperbola, which he believes to be new, and which is as follows: From a fixed point let any line be drawn to meet a fixed line, and from the point of meeting draw the line perpendicular to the fixed line, and equal in length to the first line. The locus of the extremity of the second line is a rectangular hyperbola, or if from a fixed point O a line OP be drawn to meet a fixed line in a point P, and PQ be taken perpendicular to the fixed line, so that $OP + OQ$ be constant, then again the locus of Q is a rectangular hyperbola. In the machine the latter construction is mechanically and continuously carried out. A pencil, whose point corresponds in position to the point Q, slides along a rule which is carried across the paper always perpendicularly to the fixed line. A fine steel wire attached to the pencil passes over round a roller at P, and is then carried to and coiled round a similar one at O. The use of a steel wire is a special feature of the apparatus, and has a great advantage over string, which, owing to the facility with which it stretches, cannot give good results. The finest wire should be used: it unrolls f om the one roller as much as it laps over the other, and its use may be extended to nearly all curve-drawing machines.—A voltaic cell with a solid electrolyte was exhibited by Mr. Shelford Bidwell. Its construction is as follows: upon a plate of copper is spread a layer of quite dry precipitated sulphide of copper; if on this a clean plate of silver is placed, and the cell joined up to a galvanometer, a slight deflection is observed due to the unavoidable presence of moisture. If, however, the silver plate be covered with a slight film of sulphide of silver, by pouring on it a solution of sulphur in bisulphide of carbon and evaporating the free sulphur by heat, and then placed with the prepared side down as before, a deflection is obtained far greater than, and in the opposite direction to, the former. The resistance of the cell was very great, but was enormously reduced by compression; the E.M.F. was about ·07 volt.

Mineralogical Society, May 21.—Prof. M. F. Heddle, M.D., F.R.S.E., in the chair.—The following papers were read:—On the nomenclature of the hydrocarbon compounds, with a suggestion of a new classification, by Andrew Taylor, F.C.S.—On new localities for diatomite, by Prof. W. Ivison Macadam.—On new localities for the mineral agalmatolite, with notes on its composition, by W. Hamilton Bell.—On a new locality for agalmatolite, with analysis, by Prof. W. Ivison Macadam.—The metallic ores of Chilé, by John F. Kerr, illustrated by a splendid collection of specimens.—On the chemical composition of the mineral found by Mr. Wallace at Loch Bhruithaich, Ross-shire, by Prof. W. Ivison Macadam.—Note on serpentine from Creag Mhòr Thollie, Loch Maree, by Prof. W. Ivison Macadam.—Notice of mica trap from Farley, near Beauly, by T. D. Wallace.—An excursion was made in the afternoon to the Spindle and Buddo Rocks, under the guidance of Prof. Heddle.

Edinburgh

Royal Society, June 21.—Sheriff Forbes Irvine, Vice-President, in the chair.—Mr. Omond, of Ben Nevis Observatory, read a paper on the diurnal variation in the direction of the summer winds on Ben Nevis. These varying winds seem to be entirely local, and are caused by the heating of the one side of the mountain by the sun, while the other is cooled by radiation. The air consequently passes over the mountain from the hot to the cold side.—Mr. A. Buchan read a paper on the meteorology of Ben Nevis. He referred chiefly to three points:—(1) temperature-variation; (2) variation of barometric pressure; (3) wind-speed. As regards temperature, there is the usual morning minimum and afternoon maximum, which tend to be obliterated in the winter months. The barometer reads below average in the early morning, and above average in the afternoon. There is an afternoon minimum, which tends to disappear in summer. The wind-speed is below average during the night, and above average in the afternoon. The barometer reads low when the wind is high at the top of the mountain. Mr. Buchan pointed out that the great advantage of the Observatory is that simultaneous observations are made at the top of the mountain and at the foot, the station at the foot being on an incline sloping down to sea-level. If this latter condition is not satisfied, no reliance can be placed upon deductions from the results obtained as to the rate of diminution of temperature with height. The Observatory at Hong Kong is so conditioned, and the rate of diminution, as deduced from results obtained there, is 1° F. per 281 feet. From the Ben Nevis observations Mr. Buchan finds it to be 1° F. per 270 feet.—Mr. G. W. W. Barclay described some algoid lake-balls found in South Uist.—Dr. W. Hunter read a paper on the duration of life of the red blood-corpuscles, as ascertained by transfusion. Three weeks is the average period given by his experiments. When there is no devitalising action in the corpuscles by the method of observation employed, it is probably from three to four weeks.

Paris

Academy of Sciences, June 21.—M. Jurien de la Gravière, President, in the chair.—Improvement of the bar at the mouth of the Senegal River, by M. Bouquet de la Grye. Having studied the question on the spot during the year 1885, the author proposes some simple measures by which the dangerous effects of the bar might be obviated and the navigation of the Senegal waters greatly improved.—On some double phosphates of thorium and potassium or of zirconium and potassium, by MM. L. Troost and L. Ouvrard. By preparing a certain number of phosphates of thorium and the corresponding compounds of zirconium by the dry process, the authors have endeavoured to verify the analogy pointed out by several observers between thorine and zircon. They find that the metaphosphate and the pyrophosphate of potassa yield with thorine and zircon double phosphates which have analogous compositions, but are not isomorphous. The orthophosphate of potassa gives double phosphates which have different compositions; nor is there any isomorphism between thorine and zircon obtained by calcination of the double phosphates at very high temperatures.—On the ammonia present in the ground, by MM. Berthelot and André. In reply to M. Schlœsing's last paper the authors claim to have made good their original statement that the ammonia present in the ground should be analysed without any dessication or previous treatment. They also join issue on various incidental points raised by M. Schlœsing himself during the controversy.—On the extension to a class of analogous forms of the theorem relative to a number of asyzygetic invariants of a given type, by Prof. Sylvester.—On the discovery of a new metal, austrium, announced by M. Ed. Linnemann in the *Monatshefte für Chemie* for April 1886, by M. Lecoq de Boisbaudran. From the description given of its chemical properties, its electric spectrum, and the process of its extraction from the orthite of Arendal, the author thinks that this substance is very probably gallium, a small quantity of which might easily be contained in orthite. The two rays of austrium approximately measured by M Linnemann are $\lambda = 403{\cdot}0$ and $416{\cdot}5$, those of gallium being $403{\cdot}2$ and $417{\cdot}05$. For both the ray 417 is the strongest.—Remarks accompanying the presentation of three volumes of the *Annales du Bureau central météorologique* for 1884, by M. Mascart. Attention is drawn especially to M. Fron's paper on the distribution of thunderstorms in France during the year 1883; to M. Moureaux' memoirs on the methods employed at the Parc Saint Maur Observatory for the study of terrestrial magnetism; and to M. Teisserenc de Bort's paper on the distribution of cloudiness over the surface of the globe.—Observations of Brooks's Comet III. (c, 1886), made at the Observatory of Algiers (0·50m. telescope), by M. Ch. Trépied.—Developments in trigonometrical series of certain functions verifying the equation of the potential $\Delta F = 0$, by M. Appell.—Note on some new groups of surfaces of two dimensions in spaces of n-dimensions, by M. Giovanni Bordiga.—Observations on M. Ledieu's note relative to the roll of vessels at sea, by M. de Bussy.—On the vapours emitted by a mixture of volatile substances, by M. P. Duhem. It is shown that the partial pressure of the vapour emitted by each of the two fluids mixed together is less than the tension of saturated vapour of the same fluid taken in the pure state.—Dynamics of the molecule of water: velocity of the propagation of sound; compressibility; heat of fusion of ice; specific heat of ice, by M. M. Langlois.—Calorimetric study of iron at high temperatures, by M. Pionchon. A detailed exami-

nation is made of the characteristic modifications experienced by iron at a temperature of about 700° through the extremely rapid absorption of heat in a comparatively slight interval of temperatures.—Conditions under which is realised the maximum of useful work in an electric distribution, by M. Vaschy.—Note on atmospheric refraction, by MM. J. Chappuis and Ch. Rivière. This phenomenon is here studied by a method based on the employment of Jamin's interferential refractometer.—New facts bearing on the phenomenon of the apparent oscillation of the stars, by M. Aug. Charpentier. Several observations are made, tending to show that the phenomenon is of a purely subjective character, due especially to the unequal fatigue of the muscles of the eye, or rather to their innervation.—On the presence of a new element in samarskite, by Mr. W. Crookes. The already described abnormal orange band $\lambda = 609 = \frac{1}{\lambda^2}2693$, which the author supposed due to a mixture of the two earths yttrium and samarium, he now finds cannot be due to either of these, the only probable alternative being that it belongs to some new element. Until it can be separated from the asssociated substances and its chief properties determined, he proposes to name it Sα, the initial letter S indicating its samarskite origin.—On the dissociation of the hydrates of the sulphate of copper, by M. H. Lescœur.—Action of the acids and bases on emetic solutions, by M. Guntz.—Action of water and of ammonia on the chloride of methylene, by M. G. André.—Some new properties of cyanated camphor, by M. Alb. Haller.—A contribution to the study of the alkaloids, by M. Œchsner de Coninck. The author applies the method of MM. Hoogewerf and Van Dorp to the treatment of some iodides of pyridic ammonium—isomethylate of pyridine, C_5H_5N,CH_3I, and iodethylate of pyridine, C_5H_5N,C_2H_5I.—On the normal dinitriles

$$CN-(CH_2)_n-CN,$$

by M. L. Henry.—Chemical researches on the products of the eruption of Mount Etna during the months of May and June 1886, by M. L. Ricciardi. The sands collected at Cibali were of a blackish colour, consisting mostly of amorphous detritus mixed with crystalline fragments of labradorite, olivine, and pyroxene readily affected by the magnet. The ashes ejected on May 28–29 present similar characteristics with a larger quantity of salts soluble in water.—Volumetric analysis of the sulphur in the sulphides decomposable by hydrochloric or sulphuric acid, by M. Fr. Weil.—Researches on the growth of beetroot, by M. Aimé Girard. This paper deals especially with the stalk, which during growth consists of a tissue, in the elementary organs of which water and sugar, forming a constant quantity, are mutually replaced according to the circumstances.—Researches on the structure of the scorpion's brain, by M. G. Saint-Remy.—On the structure of the germ vesicle in *Siphonostoma diplochætos*, Otto, by M. Et. Jourdan.—On the post embryonic evolution of the vitelline sac in birds, by MM. Charbonnel Salle and Phisalix.—On the vascular system of *Spatangus purpureus*, by M. H. Prouho.—On the glands of insects: a pretended "new type of elastic tissue," by M. J. Gazagnaire. The paper deals especially with the unicellular glands first described by Meckel in 1846, and afterwards studied by Stein, Sirodot, Leydig, and others. To these are referable M. H. Viallanes' pretended "elastic cellules."—On some histological peculiarities of the digestive tube in the simple Ascidians, and especially the Cynthiæ, by M. L. Roule.—On the geological constitution of the Pyrenees: the Triassic system, by M. E. Jacquot. The author's investigations lead to the conclusion that along the French slope, from the banks of the Nive to the Teck valley, the Triassic formation presents a uniform composition, recalling that of the ranges in Franche-Comté, Provence, and Lorraine.

Berlin

Physical Society, May 21.—Dr. König spoke on the modern attempts towards laying down an unexceptionable basis of mechanics. Among the axioms of mechanics the law of inertia set up by Newton was the most important, but neither the conceptions of time, which lay at the basis of the idea of uniformity, nor the conception of the straight line, were precisely definable without further assumptions. A whole series of attempts had been made to fix these fundamental conceptions, attempts which the speaker briefly sketched. He came to the conclusion that as standard of time not the movement of translation, which could never be absolutely measured, but the movement of rotation must be recognised. The movement of rotation was perceptible in itself, namely, through the oblateness of the rotating ball. With regard to the straight line, that is with regard to our co-ordinate system in space, the speaker accepted the ideas set forth last year by Herr Lange of Leipzig, who started with a notion developed by Prof. James Thomson of Glasgow. Dr. König gave a graphic representation of the idea which had been only mathematically developed and established. According to this representation it was possible, when three points described in a particular space any paths whatsoever, to follow with a co-ordinate system these movements in such a manner that all three points moved rectilinearly. Experience taught that when three points described straight lines to such a co-ordinate system, each fourth, fifth, and so on, did it as well. Thus in the movements of rotation, and in the mobile co-ordinate system, unexceptionable bases of mechanics might be found whereupon to raise a superstructure, just as mathematics was built up on its axioms.

BOOKS AND PAMPHLETS RECEIVED

"Official Guide to the Museums of Economic Botany, Kew," No. 1. "Dicotyledons and Gymnosperms," 2nd edition (Eyre and Spottiswoode).—"Quarterly Journal of Microscopical Science," June (Churchill).—"*Challenger* Reports," vol. xiv. "Zoology."—"Hygiene of the Vocal Organs," by Dr. M. Mackenzie (Macmillan).—"Disorders of Digestion; their Consequences and Treatment," by Dr. T. L. Brunton (Macmillan).—"Photo-Micrography," by I. J. Jennings (Piper and Carter).—"Birds on the British List," by Rev. G. Smart (R. H. Porter).—"Proceedings of the Linnean Society of New South Wales," vol. x. part 4 (Cunninghame, Sydney)—"Proceedings of the Physical Society of Moscow," tome viii. No. 2.—"Bourne's Handy Assurance Directory, 1886" (Bourne, Liverpool)—"Journal of Anatomy and Physiology," July (Williams and Norgate).—"General Index to the Year-Book of Pharmacy for the Years 1864 to 1885" (Churchill).—"The Great and Growing Question of Vaccination" (E. W. Allen).—"Studies from the Biological Laboratory, Johns Hopkins University," vol. iii. No. 7.

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THURSDAY, JULY 8, 1886

THE ETIOLOGY OF SCARLET FEVER

A REPORT has just been issued by the Medical Officer of the Local Government Board, the importance of which, as regards the etiology and prevention of a widespread infectious disease, deserves the most careful attention of sanitary officers and the general public alike. Hitherto the general assumption prevailed that infection with scarlet fever has always had its origin from the human subject, that is to say, that scarlet fever is always transmitted to the human subject from a human being affected with the malady, either by direct contagion in its wider sense, or through milk, cream, &c., previously contaminated with the contagium derived from a human source. In the present Report we have an account of an extensive outbreak of scarlet fever in the north of London at the end of last and the beginning of the present year amongst the consumers of milk derived from a particular farm at Hendon. The first part of the Report of the Medical Officer contains an account by Mr. W. H. Power, Inspector to the Medical Department of the Local Government Board, of an investigation into this outbreak, and the evidence brought forward by Mr. Power is absolute and conclusive: it proves by a chain of circumstantial evidence as complete as can be wished, that this particular outbreak of scarlatina was transmitted by milk which could not have been previously contaminated from a human source.

Moreover, Mr. Power proves that certain milch cows recently added to the dairy and affected with a particular malady were the source from which the contagium had been derived; further, that as this malady once introduced by a few cows into the dairy spread to other milch cows, so the amount of milk containing the contagium, and also the number of cases of scarlatina amongst the consumers, increased, and as the milk-supply was discontinued so the spread of scarlet fever abated.

The malady with which the cows were affected consisted chiefly in a particular kind of ulceration of the teats and udder, and perhaps some slight cutaneous disorder. As regards the general health, the feeding and milking capacity, the cows seemed to present very little alteration.

The second part of the Report contains an account, by Dr. Klein, of the minute pathology and etiology of this cow disease. In the first place, Dr. Klein ascertained that the local disease on the teats and udder is inoculable in its specific characters into healthy calves; secondly, that the cows affected with the local disease of the udder and teats were at the same time affected with a disease of the viscera, as proved by the *post-mortem* examination, in many respects similar to a mild form of scarlet fever in the human subject.

From the ulcers of the cow Dr. Klein isolated by cultivation a streptococcus or chain-micrococcus, possessed of distinct and special characters, both as to morphology and mode of growth in various nutritive media, particularly in milk: in this latter it grows in a peculiar manner, and very luxuriantly. With artificial cultures of this streptococcus a disease was produced in calves by subcutaneous inoculation which bears a striking resemblance to scarlet fever in man. The conclusion is thus forced on us that this streptococcus is identical with the materies morbi; further, that the scarlatina produced in the human subject by the consumption of milk from the Hendon farm was an experiment, carried out on a large scale, of infection with a cultivation in milk of the above streptococcus; and lastly, that the milk of the cows affected with the specific ulcers of the teats and udders became charged with the contagium by the hands of the milker during the act of milking. Although there are many details still wanting to complete the research, particularly those regarding the transmissibility of scarlatina from the human subject to the cow, there is sufficient evidence at hand already to warrant the hope that by a proper and effectual mode of superintending milk-farms it will be possible to considerably limit this dire scourge. A suggestion that at once presents itself is this: granted that the above-mentioned streptococcus is the real cause of the malady, there is no reason to doubt that boiling the milk would effectually destroy its life and infective power, just as is the case with all micrococci. True, the danger to contract scarlatina would hereby not be altogether annihilated, since cream cannot thus be disinfected, and since scarlet fever can unquestionably be contracted from a human source, but it must be obvious from this conclusive Report that milk *per se* coming from an infected cow plays a considerable *rôle* in conveying scarlatina from the cow to the human subject.

OILS AND VARNISHES

Oils, Resins, and Varnishes. Edited by James Cameron, F.I.C. (London: J. and A. Churchill, 1886)

THIS work, according to the preface, is intended to be "a hand-book useful to all interested in oils and varnishes, and especially to analysts, pharmacists, manufacturers, and technological students." The editor further states that in preparing this volume he used the information in Cooley's "Cyclopædia," which he has "supplemented from the latest publications." The modern literature of oils and varnishes exists chiefly in the form of workshop recipes, in trade journals, technological dictionaries and pharmaceutical publications, and if anybody ever wanted to know anything about the useful and heterogeneous products comprised under these terms he not unfrequently found it necessary to waste a good deal of time in hunting up the required information. This last addition to Messrs. Churchill's Technological Hand-books will therefore be valuable to those engaged in several distinct branches of industry, and the editor has certainly displayed considerable judgment in the selection and arrangement of the scattered materials which he has brought together in this little volume of some 370 pages in length.

Chemically speaking the word "oil" has no precise meaning. It seems in fact that an oil may be anything that is not water, since we have oils among such distinct families of organic compounds as the alcohols, acids, aldehydes, hydrocarbons, &c. Thus in Chapter I., on the "Chemistry of Oils," these compounds are in the first place classed under the usual heads of "fixed" and "volatile." Animal and vegetable fixed oils being generally ethereal salts of glycerol and acids of the fatty and

oleic series, we have in this chapter brief descriptions of glycerol (nitroglycerol and dynamite), the fatty acids from butyric upwards, and the acrylic series from oleic acid upwards. The descriptions of the acids are concisely given, and their occurrence, preparation, and physical properties briefly described. Of the chemistry of the higher homologues of the acids of these series but little is known, and the name of the acid which heads each paragraph is simply followed by its empirical formula This treatment is all that is necessary in such a work as the present, but it will certainly occur to the more advanced chemical readers that a very wide field of investigation is offered to those who interest themselves with the question of isomerism among these complicated compounds. Considering the cheapness and abundance of the commoner animal and vegetable oils in daily use it does appear somewhat remarkable that more work has not been bestowed upon them by scientific chemists, and that the information which we have concerning them should be almost confined to their commercial testing and valuation.

Animal oils, fixed and volatile, are described in the second chapter, the former being divided into animal oils proper (butter-fat, lard, neat's foot, tallow, &c.), fish oils (cod-liver, seal, sperm, whale, &c.), and insect oils (niin, ant-grease, &c.). Among the volatile oils of this class we have bone-oil, castoreum, and civet oils (animal), and ambergris (fish). The third and fourth chapters are devoted to vegetable oils, a list of 19 drying and 23 non-drying oils being given. Linseed and olive oils, the most important members of this group, naturally claim the largest amount of space, and the technology of these products is well treated of. The volatile vegetable oils are very fully dealt with, no less than 56 pages being devoted to their consideration. After a description of the various methods of extraction by distillation, solvents, &c., the oils themselves are described individually according to their vegetable sources, the botanical names of the class and order being followed by a descriptive list of the oils obtained from each group of plants. Thus under *Aurantiaceæ* we have the oils of bergamot, cedrat, citron, lemons, limes, neroli, and orange ; under *Caryophyllaceæ*, cajeput and clove oils, and so forth.

In the fifth chapter, empyreumatic, medicated, mixed, and perfumery oils are treated of. We give a specimen of the editor's conscientiousness in his description of medicated oils:—"EARTHWORM OIL. *Syn. Oleum lumbricorum* (E. Ph. 1744). Washed earthworms, ½ lb. ; olive oil 1½ pint ; white wine, ½ pint. Boil gently till the wine is consumed, and press and strain." We are not informed what special merit is possessed by this gruesome concoction, but it was no doubt applied in good faith in the last century. Under "mixed oils" will be found a collection of strange mixtures, some of which might have formed ingredients in that "charm of powerful trouble" brewed by the witches in Macbeth. The familiar "nine oils" of the past generation of housewives, and even furniture oil, find place herein, together with some three dozen others. Chapter VI. contains an account of waxes, which are classed as animal, vegetable, and artificial, a useful method of distinguishing these substances by their behaviour with chloroform concluding the section.

The seventh chapter, a somewhat lengthy one, is devoted to mineral oils, viz. those obtained by the distillation of shales, coal, lignite, and peat, and those found naturally formed in various parts of the world. The treatment of coal-tar and the petroleum industries are well described, and the chapter concludes with sections on the storage of petroleum and the construction of petroleum lamps. Oil refining is treated of in the eighth chapter, which is a short one—almost too short considering the large number of processes which are now or have been formerly in use. The methods for refining tallow, wax, petroleum, and resin oil are included in this chapter, besides the purification and bleaching of animal and vegetable oils proper.

The longest chapter in the book is the ninth, which extends to 109 pages, and is devoted to the important subject of the testing of oils. This chapter is certainly a good one, both for thoroughness and the arrangement of its contents. Thus the testing of an oil may have for its object, the determination of purity, the lubricating efficiency, or the illuminating value. The purity may be ascertained by chemical or physical tests, both of which methods are very fully and lucidly treated of for each class of oils. Among physical tests are described the various methods of determining the specific gravity and melting point, cohesion figures, &c. The descriptions of the latter, which are quoted from a paper by Miss Crane, would have been of more value if figures had been given. The chemical tests, qualitative and quantitative, are given with great completeness. A figure of Abel's petroleum tester and the method of using it as prescribed by the Act of Parliament finds place in this section. For testing the lubricating value the machines of Stapfer, Thurston, and Bailey are described and figured ; for viscosity the apparatus devised by Lamansky, and by Townson and Mercer ; and for fluidity the apparatus of Bailey is also described and figured. The section on illuminating efficiency is not so full, and might be advantageously expanded in a future edition.

Chapter X. is devoted to resins and varnishes, and the last chapter contains descriptions of Mills' bromine absorption process and Hirscholm's method of testing resins. The appendix contains some useful tables of prices, of the amount and value of the export of seed oils during 1882, 1883, and 1884, and of the production of shale oil in the United Kingdom during the last five years.

From the foregoing epitome of the contents it will be seen that the volume, although a small one, gives a most comprehensive view of the subject of which it treats, and the amount of useful information which has been condensed into this small compass is mainly due to the concise mode of treatment which the editor has adopted. We can certainly recommend it to those for whom it is written.

R. MELDOLA

HARTLAUB ON THE MANATEES

Beiträge zur Kenntniss der Manatus-Arten. Von Dr. Clemens Hartlaub (Bremen). Separatabdruck a.d. *Zoologischen Jahrbüchern*, Band I. (1886.)

AMONGST other interesting articles with which Dr. Spengel's new zoological journal has commenced its career is one by Dr. Clemens Hartlaub (son of the

veteran ornithologist of the same name) which deserves special attention, as devoted to a somewhat neglected and imperfectly known group of the class of mammals—the Manatees or "sea-cows," as they are popularly called. The Manatees constitute, as is well known, one of the three modern representatives of the formerly more extensive order of Sirenians, or "Herbivorous Cetaceans," as they are sometimes, though not very correctly, denominated; for it is doubtful whether they have any near relationship to the true Whales. One of these three forms—the *Rhytina stelleri*—is already extinct; the other two—the Manatee and Dugong—are rapidly diminishing in numbers before the advancing tide of civilisation, and it is highly desirable that full details of their structure and habits should be obtained and recorded before they are "improved" off the face of the earth.

Dr. Hartlaub, having examined the skulls and other specimens of Manatees preserved in the various museums of the Continent, presents us with a *résumé* of his investigations in two well-ordered and well-illustrated essays. In the first of these he describes the skull of the African Manatee (*Manatus senegalensis*), and compares it bone by bone with that of the American *M. latirostris*, fully establishing the specific difference of the two forms, which has been doubted even by some of our most recent and best authorities.[1] In the second memoir he describes for the first time the skull of the South American *Manatus inunguis*, a species absolutely ignored by the great majority of naturalists, and shows its distinctness from *M. latirostris*.

It is hardly necessary even to recapitulate the points of difference between these three forms of Manatees, which Dr. Hartlaub has given at full length in these memoirs, and which seem to be sufficiently obvious on reference to his well-drawn figures. But a few words may be added on the geographical distribution of the three living Manatees, so far as this is at present known to us.

The African Manatee inhabits the west coast of that continent from the Senegal down to the Quanza, and penetrates up the larger rivers far into the interior. In the Senegal it has been recorded by Adanson, in the rivers of Liberia by Büttikofer, in the Niger and Benuë by Barth and Vogel, in Gaboon by Du Chaillu, in the Lower Congo by Johnston and Pechnel-Loesche, and in the Quanza by Monteiro. Whether the "*Charuf el bachr*," or water-sheep, ascertained to exist in the Uelle by Schweinfurth, which is probably the same as the supposed Manatee found in the Shari and Lake Tchad by Barth and other travellers, should be referred to *Manatus senegalensis*, or is even a Manatee at all, remains an interesting subject for future inquiry. But it seems tolerably certain that some sort of Sirenian inhabits the inland basin of Lake Tchad, and the probability is that it will turn out to be a *Manatus*.

In America the exact boundaries of the two species, *Manatus latirostris* and *M. inunguis*, cannot yet certainly be stated, owing to the confusion that has hitherto existed between these two forms. But it is certain that the Manatee occurs on the Atlantic coast of America from 25° N.L. to 19° S.L., and that those of the Antilles, the Gulf of Mexico, and Surinam, are referable to *M. latirostris*. On the other hand, *M. inunguis* is only certainly known from the Amazons and its tributaries, where it was first discovered by Natterer. Dr. Hartlaub is inclined to believe that the Manatee of the coast and rivers of South-East Brazil must be likewise *M. inunguis*, but this does not seem to be probable. It is more likely, we think, to turn out that one species is found all along the Atlantic sea-board, penetrating only slightly up the rivers, while the other is confined to the interior, and is a purely fresh-water species.

[1] Cf. Flower, "Catalogue of Vertebrates" in the Museum of the Royal College of Surgeons, part 2, p. 528, 1884.

OUR BOOK SHELF

Infant-School Management. By Sarah J. Hale. (London: Stanford, 1886.)

THIS is one of the best books on infant-school management that we have seen; the authoress knows exactly the kind of information infants can most readily assimilate, and how best to impart it; while on the other hand she is fully aware how dangerous and worse than useless the forcing process is.

The second part of the book consists of sketches of lessons in natural history, natural phenomena, food-plants, and common objects; and if science is to be taught in all our infant schools in the manner our authoress suggests, we may look forward to a largely increased taste for science in the rising generation.

Here is an extract from the introduction to the second part showing the method of teaching which she recommends:—

"In every case the teacher must bring plenty of illustration to bear upon the lesson. In natural history *the real animal* or a good picture, and if possible, some thing or things that it furnishes us with, as, for instance, the fur of the otter, the shell of the tortoise, the quills of the porcupine. Also the teacher should carefully provide herself with pictures of animals which afford strong contrasts to the one with which she is dealing, as well as those which bear some general resemblance to it, that she may exercise the *discriminative* as well as the *assimilative* faculty of her pupils. In all object lessons, various specimens of the object should be produced for examination and description; the little ones themselves must do the main part of the latter under the teacher's guidance, for these lessons are not only to enable the children to form new ideas, but they are also intended to train them in giving expression to such ideas. The teacher must make good use of the black-board, and should practise drawing objects, so that she may illustrate with facility and precision any particular point of her lesson which can be so illustrated. All the materials, pictures, diagrams, &c., which the teacher provides from time to time, should have their place in the school museum ready for future needs, and the children should be encouraged to bring contributions to such a museum, particularly such as the lessons they receive may suggest. Object-lesson cards, pictures, and all illustrations should be carefully used, and when not in use, have their proper places on wall or shelf. The teacher should arrange all specimens in the museum, and have each addition neatly labelled and catalogued."

A Year in Brazil. By Hastings Charles Dent, C.E., F.L.S., F.R.G.S. With 10 Full-page Illustrations and 2 Maps. (London: Kegan Paul, Trench, and Co., 1886.)

THIS is a very interesting account of a year's sojourn in an interesting country, and although the author went out for a special purpose, to survey for a railway, every moment of his spare time was taken up in making collections and taking notes in most of the branches of natural history. The scientific interest of the book is mainly

confined to the notes on animals, birds, reptiles, insects, and the parasitic torments of Brazil, notes on botany and on geology, together with a discussion of the theory of evolution and observations made on protective colouring and mimicry.

With reference to the theory of evolution the author states that he has constantly endeavoured to oppose it, on the ground apparently put forward by theologians many years ago before they knew what the theory really was; and we think that if our author will continue his scientific studies a little longer he will probably find that the arguments he uses against it are really not in point.

The Colloquial Faculty for Languages. By Walter Hayle Walshe, M.D. Second Edition. (London: Churchill, 1886.)

THIS is a book full of pleasant gossip round the central idea embodied in its title; hence we have essays on the nature of genius, the conditions regulating colloquial faculty, and the causes of variety of colloquial faculty and faculty for translation.

In the chapter on composition in foreign tongues it is pointed out that the man of science proves now and then well capable of wrestling effectively with the humorist on his own ground of the *literæ humaniores*, and gives as an example Herbert Spencer's *exposé* of abounding errors in a passage from Addison quoted by Matthew Arnold, as an example of classical English.

We gather from our author that the English race is not the most gifted with the colloquial faculty, and a remark of Prince Bismarck's is quoted that he had always found that an Englishman who could speak good French was a doubtful character.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Periodicity of Glacial Epochs

PERMIT me to ask, as a matter of international comity in science, the help of my learned British colleagues in the following matter.

I am just now occupied upon a work treating upon the periodicity of glacial epochs, a question which has already been broached by me in previous writings several years ago. The cause of this phenomenon being attributed by the astronomers, as well as by the majority of geologists, to the displacement of the perihelion, whose cycle is 21,000 years, it follows that, according to the actual position of this point, the ice now covering the Antarctic regions had its maximum of intensity at about the year 1250 of our era. For the same reason, the ice of the boreal hemisphere must have offered at this same epoch its minimum of intensity. Consequently the latter must have been increasing since the close of the thirteenth century, while the former must have been receding. The researches of European geologists must have shown a marked extension of the glaciers of Spitzbergen, Greenland, &c., since the beginning of the fourteenth century, and a recession of vegetation from the latitude of Sicily to the Polar Circle. But we in France are not informed of what has happened in the southern hemisphere since the arrival of the first navigators. I would therefore, in the name of science, beg of any British officers, consuls, or scientific observers who are, or may have been, collecting facts at stations near the South Pole, in Patagonia, New Zealand, Tasmania, and elsewhere, to communicate to me directly, or through your columns any information they may have upon this subject. I wish to know whether, since the first arrival of Europeans in those regions, the ice field has shown a recessive movement, accompanied by an inverse tendency of vegetation.

Tarascon, Ariège, June 27 ADOLPHE D'ASSIER

Evidence of Man and Pleistocene Animals in North Wales prior to Glacial Deposits

SOME of the results recently obtained during the researches carried on at the Tremeirchion caves under the superintendence of Mr. E. Bouverie Luxmoore and myself, seem to me of so much importance that I have thought it advisable to communicate them, in anticipation of the full report which will be presented to the British Association, especially as an important section is now exposed, and may be examined by any one desiring to do so, which will probably have to be covered before the end of the summer.[1]

In continuing our explorations this year, by means of a grant from the British Association, we found that the Cae Gwyn Cave (described in my paper in the *Quart. Journ. Geolog. Soc.* for February last) had come to an abrupt termination in a plateau of Glacial deposits. On further examination it was found that this must have been the main entrance into the cavern when it was occupied by the Pleistocene animals, and that the Glacial beds in and upon it must have been deposited subsequent to the occupation by the animals. As in the other parts of the cavern, the cave-earth at the entrance—a brown sandy clay, contained fragments of a stalagmite floor and of stalactites along with angular fragments of limestone. The bones also occurred at all angles, showing that the contents had been greatly disturbed by water action. The bone earth was covered over at the entrance and for some distance inwards by a few feet of stratified sand, containing well-scratched boulders, and it, as well as the sand, was traced for a distance of fully 6 feet beyond the entrance under the series of Glacial deposits, shown in the section.

In digging outside the entrance, the floor of which is 20 feet below the surface of the field, it was soon found that we could not extend our researches outwards, owing to the nature of the lower deposits, chiefly sands and gravels, without making an opening into the field. By the kindness of the owner, Mr. Edwin Morgan, a shaft was allowed to be dug in front of the opening, about 9 feet across at the surface and over 5 feet at the bottom. This shaft was subsequently widened at the bottom, in consequence of some falls, and the lower part, except at one point, had to be carefully faced with timber. The upper part of the shaft is now much widened and sloped. To make it certain that the Glacial deposits are continuous from the shaft in a westerly direction, I had the beds probed at different points for a distance of about 70 feet; and subsequent examination showed clearly that there is here an extensive terrace of drift reaching to heights of between 400 and 500 feet above Ordnance datum. The section was carefully taken at two different points in the shaft by Mr. C. E. de Rance, F.G.S., of the Geological Survey, and myself, and in doing so we found well-scratched boulders in each of the deposits. Among the boulders found are granites, quartzites, flint, felstones, diorites, volcanic ash, Silurian rocks, and limestone. Silurian rocks are most abundant. It is clear that we have here some rocks from northern sources along with those from the Welsh hills, and the manner in which the limestone at the entrance to the cavern is smoothed from the north would indicate that to be the main direction of the flow. A small but well-worked flint flake was dug up from the bone earth on the south side of the entrance on June 28, in the presence of Mr. G. H. Morton, F.G.S., of Liverpool, and myself. Its position was about 18 inches below the lowest bed of sand. Several teeth of hyæna and reindeer, as well as fragments of bone, were found at the same place, and at other points in the shaft teeth of rhinoceros and a fragment of a mammoth's tooth. One rhinoceros tooth was found at the extreme point examined, about 6 feet beyond and directly in front of the entrance. It seems clear that the contents of the cavern must have been washed out by marine action during the great submergence in mid-Glacial time, and that they were afterwards covered by marine sands and by an upper boulder-clay, identical in character with that found at many points in the Vale of Clwyd, and in other places on the North Wales coast.

The facts obtained seem to me to prove conclusively that man and the Pleistocene animals must have lived in parts of the North Wales area, and have occupied some of the caverns, before the period of the great submergence indicated by the Moel Tryfaen and other high-level sands; hence certainly before the Upper Boulder-Clay was deposited. HENRY HICKS

[1] Tremeirchion is about four miles from St. Asaph, and less than two miles from Bodfari Station on the Chester Mold, and Denbigh line.

Ampère's Rule

BEGINNERS are certainly, as Herr Daehne says (NATURE, June 24, p. 168), liable to get a little "mixed" in reference to the above *mem. technica;* chiefly, I think, for want of some idea sufficiently prominent to fix itself on the mind to the exclusion of others.

I have found the following slight modification of the original rule pretty easily remembered and applied.

It may be taken as agreed—

(1) That the *head* is more important ("more worthy," as the old grammars put it) than the *feet*.

No one except an acephalous mollusk will deny this; and *it* is not a fair judge.

(2) That the *right* hand is more important than the left hand. The left-handed people are a mere minority (and a nuisance at cricket); and minorities are, according to modern Radical ideas, "une quantité négligeable."

3. That the N-seeking pole is that part of the compass-needle to which attention is mainly directed.

Now,—If a person places himself so as to face the needle, and a current goes from HEAD to foot, the N-SEEKING pole moves to his RIGHT hand.

This is practically the form in which the rule is given in Prof. Balfour Stewart's "Lessons in Physics." One of the small articulated wooden figures used as models in drawing is very useful for illustrating the above rule. Its right arm may be stretched out sideways at right angles to the body, and it may then be held close to the wire in various positions; paper arrows being tied to the latter, to mark direction of current.

Eton College H. G. MADAN

Halos

As the atmosphere appears recently to have reassumed in a marked degree some of the peculiar conditions which pertained to it during the time of the great sun glows, I have thought it worth while to send you notes from my diary of some effects observed by me:—

June 14.—Between 10 and 11 a.m. Complete solar halo of a coppery colour. It lasted more or less distinctly for some time, and gradually faded. I saw no trace of mock suns.

June 23.—Between 10 and 10.30 p.m. there was a curious pearly green light in the north-north-east, and some peculiar pearly green clouds (?) floated from north to west. At first I thought this was an auroral display, but probably it was due to the same cause as the "glows."

June 15, 23, 30, July 1, 2, and 3.—After-glows of the usual pinkish hue. J. H. A. JENNER

4, East Street, Lewes, July 3

The Microscope as a Refractometer

I HAD no idea that the short paper you did me the honour to print on this subject would have led any one to suppose that a claim was made for the discovery of a new principle in physics, or that the microscope was to be used for the first time in questions on refraction.

In so short a space it was impossible to tell over again the tale of progress in this branch of physical optics, and to signalise every worker in the field by name. So much has already been done in the perfecting of optical instruments, that the utmost one can now hope to do is, by a slight improvement here and there, to render them still more serviceable.

All that was claimed as new in my paper of June 17 was—

(α) The use of the marked slip, structure of cell, superposed cover-glass.

(β) The measurement of the linear distances between the images by a finely graduated "fine adjustment" screw.

(γ) The use of an objective of high amplifying power (a 1/20-inch homogeneous immersion may be used if the shoulder-pieces of the cell are made with talc, and the cover-glass very thin).

It is of course possible that one or all of these details is not new; but, in spite of the authorities quoted by Dr. Gladstone to show the previous employment of the microscope in questions of refraction, I still maintain their claims to novelty to be valid; and, even supposing they are not new (which has yet to be shown), my greatest offence is that of independently arriving at a previously known method. And, considering the attention that our most eminent physicists have bestowed upon the subject, the wonder is that this has not more frequently been the case.

As to the efficiency of the method, the only objections urged against it by Dr. Gladstone are: (1) its results cannot be relied upon beyond the third decimal figure; (2) the temperature of the drop of fluid under examination cannot be taken.

As to the first objection, if we take μ_1, μ_2 as the tabulated indexes of refraction of two known substances, δ as representing the difference of distance between the images of the marks viewed through them, and measured by the fine adjustment, μ and d the corresponding symbols for the fluid under examination, we have the following equation to determine μ:—

$$\frac{\mu_2 - \mu_1}{\mu - \mu_1} \times \frac{\mu}{\mu_2} = \frac{\delta}{d}.$$

And I see no more reason to limit the exactness of this to the third decimal figure than in the formula used with the hollow prism. Moreover, if a vernier is attached to the fine adjustment the result may be relied upon with still greater accuracy.

(2) As to the temperature. In the case of most fluids this may be taken from the bottle containing the fluid; no grave scientific error will arise from the difference in temperature of a drop of fluid in contact with glass on the stage of the microscope and the same fluid in a glass bottle by its side. In the case of ethers, &c., the cell may be temporarily sealed.

As to the practical use of the method, the opinion of so known an expert as Dr. Gladstone is of the greatest weight, but as any recognition of the novelty of my method escaped acknowledgment in his notice, I may still hope that its practical use escaped observation also. So thin a stratum of fluid is employed that the index of refraction of black ink may be obtained, a result which would puzzle any one to arrive at who restricted himself to the use of the hollow prism.

That the microscope has been previously used for experiments in refraction no one ever doubted; if Dr. Gladstone, before writing, had had the time to go step by step through my method, he could scarcely have refrained from acknowledging that in its essentials it was hitherto unpublished.

GORDON THOMPSON

St. Charles's College, Notting Hill, July 3

The Bagshot Beds

As you have given publication (NATURE, July 1, p. 210) to the abstract of the paper recently read by Messrs. Monckton and Herries before the Geological Society, in which they assert that their object was to "disprove" the view lately propounded by me, as to the relation of the Bagshot Beds of the London Basin to the London Clay, perhaps you will kindly afford me space to point out to the readers of NATURE (1) that these authors have ignored, in dealing with the question, whole chapters of the evidence upon which my view is based—evidence which is continually accumulating, as two forthcoming papers (one in the press for the *Proc.* Geol. Assoc., the other in the hands of the editor of the *Geol. Mag.*) will make manifest enough; (2) that in directing their attention merely to sections at the outcrop of the beds they have added little, if anything, substantially, to that on which the old view was based, while the lithological distinctions of the Upper and Lower Bagshot Beds (where the latter have been for ages undergoing oxidation) are not sufficiently marked to furnish, in disconnected sections, evidence which can be anything more than, to say the least, equivocal. A. IRVING

Wellington College, Berks, July 3

The Enemies of the Frog

IN connection with this subject the following incident may be of interest to some of your readers. One day, near the kitchen area, an unusual noise was heard: it seemed like the mewing of a cat combined with a well-sustained whistle. On going to the spot, it was found that the noise proceeded from a cat and a frog, but it was difficult to decide from *which* of the two. Every time the cat touched the frog the sound was produced and the frog hopped away. The cat exhibited in his attitudes and motions a sort of enjoyment mingled with awe. He would just touch the frog very gently with the tips of his paws, then watch it most attentively, and when the frog would emit its peculiar loud squeak—not the usual croak—he would give a sudden bound, as if both surprised and amused; but he never

attempted during the whole of the proceedings, which lasted about a quarter of an hour, to bite the frog. The frog was removed quite uninjured, but apparently exhausted either by fear or by muscular exertion.

T. MARTYR

St. Joseph's College, Clapham, S.W.

Hybrids between the Black Grouse and the Pheasant

IN Yarrell's "British Birds," 4th ed. vol. iii. p. 69 *seq.*, a number of hybrids between the cock pheasant and the gray hen are enumerated as having occurred in England. Being desirous to give a life-sized and coloured figure of such a hybrid in my forthcoming work on the black grouse, the capercailzie, and their allies, I wish to borrow a specimen for a short time, and, as my endeavours to procure one have so far been unsuccessful, I beg to make this known through your widely read journal, hoping that some fortunate possessor may be kind enough to communicate with me concerning his willingness to lend me a specimen for the said purpose.

A. B. MEYER

Royal Zoological Museum, Dresden, July 5

THE FINSBURY TECHNICAL COLLEGE CONVERSAZIONE

THE annual *conversazione* given by the students of the above College as the closing event of the session came off on Friday evening, July 2, and proved in every way a success. The large number of interesting objects brought together for exhibition certainly speaks well for the activity of the various committees which were intrusted with the work of organisation, and at the same time indicates how widely spread is the interest shown in the welfare of the College by the different firms of manufacturers who contributed to the exhibition. The electrical department exhibited in action most of the apparatus used for educational purposes in the College. In this department also were exhibits of apparatus and models by Messrs. Woodhouse and Rawson, the Electric Apparatus Company, Messrs. Mayfield's vacuum-tubes, and other electrical and physical apparatus made by this firm. The exhibits in the chemical department were especially numerous and representative of chemical technology in most of its branches. In the way of apparatus Messrs. Cetti, of Brooke Street, exhibited barometers, thermometers, vacuum-tubes, &c.; Messrs. Townson and Mercer showed a new carbonic acid generator, Schutzenberger's gas apparatus, filter pumps, nickel crucibles and basins, Pasteur flasks, inland revenue stills, Abel's petroleum testing apparatus, &c.; and Mr. B. Redwood lent a set of viscometers. Fine chemicals were exhibited by Messrs. Hopkin and Williams, and a splendid set of alkaloids and other products by Messrs. Howard of Stratford. Messrs. Pontifex and Wood exhibited sets of pigments and the materials used in their manufacture, Mr. C. Richardson a set of specimens illustrating the manufacture of cements, Mr. Ashley samples of English and foreign lubricating oils, and Messrs. J. and L. Cripps the materials and finished products representing the manufacture of size, glue, and gelatine. Glass manufacture was represented by a set of tools and specimens from the Whitefriars glass-works (Messrs. Powell). Messrs. Field showed a fine series of waxes and other materials used in candle-making, and a good exhibition was made also by Price's Patent Candle Company. The manufacture of soap was illustrated by a very complete set of specimens contributed from Messrs. E. Rider Cook's works at Bow and by Messrs. Knight, &c. Cotton seed and its products were shown by Messrs. W. and W. H. Stead of Blackwall and Liverpool. The specimens and diagrams sent by Gaskell, Deacon, and Co., of Widnes, gave an excellent idea of the alkali manufacture in this country.

The collection of coal-tar products was especially rich, specimens having been sent by the Badische Company, the Hoechst Colour Works, Messrs. Brooke, Simpson, and Spiller, the British Alizarine Company, and Messrs. Burt, Boulton, and Haywood. The latter firm exhibited a splendid model of their timber creosoting plant. Amongst other tar products was a set of preparations of the new sweetening substance, saccharine, sent by Dr. Fahlberg. The Broadburn Oil Company showed a very complete set of shale products. The sugar industry was represented by a set of polarimeters, models and specimens, exhibited by Mr. Newlands of the Clyde Wharf Sugar Refinery, and by the Beetroot Sugar Association. In the course of the evening Prof. S. P. Thompson gave a lecture on waves of light, and Mr. John Castell-Evans discoursed on explosives. The entertainment was on the whole highly creditable to the College, and many of the firms who sent objects for exhibition have signified their approval by presenting their exhibits to the establishment as lecture specimens.

THE RECENT DISCOVERIES AT TIRYNS[1]

THE excavations made during the last two years at Tiryns, by Dr. Schliemann and Dr. Dörpfeld, have thrown new light on what has been hitherto an almost unknown period of Greek history—that far-off time, more remote even than the age of the Homeric poems, when Hellenic civilisation had not yet emerged from its Oriental cradle, nor developed its highly cultured systems of social and political government out of the splendid but semi-barbarous tyrannies of Western Asia Minor.

The literature of Greece has made us familiar with the later times, when the individual was for the most part merged in the State, and when the wealth and artistic skill of each city was devoted to public uses, such as the Council-chamber, the Agora, or the stately temples of the gods, rather than to the luxury of any one person.

But at Tiryns a very different picture is presented to us: we see a single autocratic chieftain, ruling in a sort of feudal state, and occupying a magnificent palace, surrounded by the humbler dwellings of his circle of retainers; while, instead of the utmost resources of the architect, the sculptor, and the painter being lavished on the shrine of the presiding deity, a mere open-air altar is dedicated to the god, and it is the chieftain's house which is decked out with the splendours of gilt bronze, marble sculpture, and painted walls.

The rock in the marshy plains of Argolis, on which stands the citadel of Tiryns, is about three miles distant from the Gulf of Nauplia, and commands an extensive view reaching from Argos, with its rich olive-groves, to Mycenæ on its lofty crags, and, between the two, the once prosperous sea-port of Nauplia, by the blue waters of its sheltered bay.

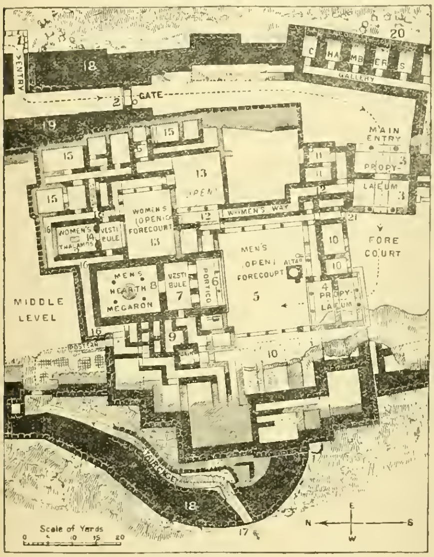
The massive fortification wall which surrounds the Tirynthian Rock was an object of wonder and admiration in the earliest historic times of Greece: its enormous stones keenly aroused the Greek imagination, and created legends which attributed them to mysterious Cyclopean builders, and peopled the walls with the demi-gods of the heroic age, such as Perseus and Heracles, whose early youth was fabled to have been spent in the Tirynthian city—the Τίρυνς τειχιόεσσα of Homer's "Iliad." This wonderful wall, some stones of which are no less than 11 feet long and 4 feet thick, was originally nearly 50 feet high at its loftiest part, measuring from its base outside: inside the city the height was very much less, as its lower part acted as a retaining wall, which kept up the loose earth which formed a level interior surface above the irregular contour of the rock.

The southern part of the Acropolis wall, where it incloses the great palace, is a very complicated structure,

[1] "The Prehistoric Palace of the Kings of Tiryns." The results of the latest excavations, by Dr. Henry Schliemann. The preface by Prof. F. Adler, and contributions by Dr. Wm. Dörpfeld. With 188 woodcuts, 24 plates in chromolithography, 1 map, and 4 Plans. (London: John Murray, 1886).

containing long rows of small vaulted chambers averaging about 14 feet by 12, all opening into a long vaulted passage, the whole formed in the thickness of the wall itself, which at some places consists of nearly 40 feet of masonry. Two stories of these chambers existed, one above the other: the lower story is of solid stone masonry, built of large hammer-dressed blocks, bedded in clay cement. The upper tier of chambers was built of sun-baked bricks, like the upper part of the wall round Athens, carefully protected from the weather by coatings of very hard durable stucco. This top story of rooms opened into a long open *loggia* or colonnade, with a flat roof supported by a range of wooden columns, each on a stone footing block.

The columns themselves are of course no longer in existence, but their size and positions are clearly shown



PLAN OF THE PALACE MEASURED BY DR. DÖRPFELD.

1, Main gate in outer wall; 2, inner gate; 3, main propylæum; 4, inner propylæum; 5, court of the men with surrounding cloister, 6, aithousa, or portico; 7, prodomos, or vestibule; 8, men's hall; 9, bath room and small chambers; 10, chambers round the court; 11, guard-room by the propylæum; 12, passage from propylæum to the women's part; 13, 13, courts of the women; 14, women's hall; 15, chambers of the women's part; 16, 16, passage from the women's part to the outer postern; 17, postern door, approached by rock-cut steps; 18, outer wall of Acropolis; 19, inner wall of entrance passage; 20, outer wall with rows of chambers; 21, side entrance to men's court.

by the stone base on which each stood. These extensive series of chambers in the wall of Tiryns served, no doubt, to accommodate the garrison of the place—exactly as was the case in the Phœnician cities of Thapsus and Carthage. The enormous number of 24,000 men are said to have had lodging in the walls of the Carthaginian Acropolis (Appianus of Alexandria, "Hist. Rom.," i. p. 220), and recent discoveries have shown that this statement may very possibly have been no exaggeration.

More than a third of the whole Acropolis of Tiryns was occupied by the palace of its ruler—a very complicated building designed with much architectural skill, and showing complete uniformity of design combined with very distinct adaptation to a clearly defined purpose.

With the exception of three very narrow posterns, there is only one entrance into the citadel; and this is very skilfully contrived so as to make its defence as easy as possible.

After breaking through the outer gate, which was commanded by a tower on the right or unshielded side of the enemy, the assailants would have found themselves in a narrow passage with massive walls on both sides, and strong inner gates at each end; and in this cramped space would be subjected to a heavy fire of missiles poured down upon them from all sides.

After passing the inner gate in a southern direction, a handsome propylæum or porch was reached: this was decorated on both sides by a portico of two columns *in antis*, and is very similar in plan to the propylæum of the sacred inclosure at Eleusis—a work of the fifth century B.C. (See the accompanying woodcut of the plan.)

After passing through an open court, a second propylæum of similar design was reached, and then the visitor found himself in the cloistered court of the men's part of the palace. In the central open space stands the altar to Zeus Herkeios, which is frequently named in the Homeric poems, built over a small pit cut in the rock, into which would fall the blood and ashes from the burnt offering on the altar above. Opposite the entrance to the court is the great hall or Megaron (about 40 feet by 32), approached through an open-columned portico and an inner vestibule. The roof of the hall was partly supported on four wooden columns, which appear to have carried an open "lantern," formed to carry off the smoke from the fire, which burnt on a round hearth in the middle of the room; very much as was the case in the hall of a mediæval house or college.

On the west side of the hall of the men are a number of small chambers—probably the sleeping-apartments of the bachelor members of the family. One small room, about 12 feet by 10, is of extraordinary interest. It is evidently a bath-room; its floor is formed of one enormous slab of stone, carefully chiselled so as to drain out at one point where a stone drain-pipe is carried through the wall, and so into the main drain of the house.

The bath itself, in size and form very like a modern one, was of terra-cotta decorated with a spiral ornament in red.

The walls of the bath-room were lined with wooden planks about 10 inches wide; their lower ends were fastened by wooden dowels to the stone flooring block, the edge of which all round the room is raised slightly where the wood wall-lining rested on it: evidently in order that water splashed on the stone floor might not soak under the wood lining: a piece of refinement which shows much thought and labour spent on matters of detail.

The eastern half of the palace consists of the apartments reserved for the women and the married members of the chief's family. This also has a hall with a central hearth, and is approached through a single vestibule from another open court. The smaller rooms, of which there are a great many, on this side appear to have been two stories high: traces of the staircase still exist.

Though a separate group of apartments appears to have been provided for the women, yet they were by no means shut off from ready access either to the outer world, or to the men's part of the house. There are at least three ways by which the women's rooms could be reached: one from the side of the outer propylæum, another through the court of the men, and lastly a long passage leads round the back of the men's hall to the long flight of rock-cut steps leading down to the postern in the semicircular bastion. Another door at a higher level gave direct communication between the hall of the men and the apartments of the women.

The walls of this palace were built of roughly-dressed stone bedded in clay up to a height of about 2 feet above

the ground: the rest of the wall was of sun-dried brick; the whole was then covered inside and out with three coats of stucco made of lime mixed with sand, gravel, and broken pottery, a mixture which set nearly as hard as stone, and must have been a most perfect protection even in the stormiest weather. Finally, where the stucco was to be painted a thin coat of pure lime was applied as a ground for the colours, which consisted of red, yellow, and brown ochres, with charcoal-black and lime-white; and lastly, blue and green *smalti* or pigments, made of powdered glass. All these colours were of the most durable sort, and could be applied, as appears to have been done at Tiryns, on freshly-laid stucco—true *fresco*.

The painted decorations are of the very highest interest, and very characteristic examples of primitive art, which show strong traces of Egyptian or Phœnician influence. Some of these wall-paintings are evidently copied from textile patterns, and, though rudely executed, have much true decorative value. Woven stuffs such as were made in Egypt are imitated by the painter, and even the fringes are carefully copied. Other pictures, of which only fragments remain, had large figures of animals or men with wide-spreading wings, the feathers of which are painted in alternating colours in a very brilliant and skilful way. These show strong signs of Phœnician influence. The most remarkable and best preserved of all is a picture of a bull galloping at full speed, on whose back a man is riding in an acrobatic sort of way, holding on by one of the bull's horns. The whole is painted with much vigour, and with a rapid sweeping touch of the brush, which shows considerable practice and skill on the part of the painter.

Some parts of the palace were evidently decorated in a much more magnificent and costly way—that is, the walls were lined with wooden boarding, and on this were nailed plates of gilt bronze beaten into *repoussé* reliefs—very similar probably in style to the ninth century gates of Shalmaneser II., now in the British Museum, and other bronze reliefs found at Olympia. Many small fragments of these gilt metal linings were found in the burnt debris of the palace; and there is little doubt that the wooden columns in the hall and its portico must once have been cased with similar metal sheathing: very like the bronze-cased wooden column which was found some years ago among the ruins of Khorsabad.

Nothing could exceed the splendour of this mode of wall-decoration—the whole surface enriched with its gleaming reliefs would appear one mass of shining gold, and we know now that the gold and silver walls of the Homeric palace of Alcinous were not merely the offspring of a poet's fancy. Fragments were discovered by Dr. Dorpfeld of another extremely sumptuous method of architectural decoration—a frieze about 20 inches deep sculptured in alabaster with a rich and minutely worked pattern of rosettes and geometrically treated flowers, thickly studded with carefully cut bits of jewel-like transparent blue paste or glass. The effect of these deep-blue jewels flashing light from the contrasting creamy white of the alabaster must have given a most striking effect to the room which was adorned in so costly a way, especially if the wall below the frieze were one of those which were coated with the gold reliefs.

Nor was the colour confined to the walls: even the floors were decorated with simple patterns in brilliant blue and red, applied after the design had been indicated by lines incised on the surface. These floors were made of strong lime and gravel concrete carefully laid in three or four layers, each of finer material than the one below—a method exactly similar to that described by Vitruvius and used so skilfully by the Roman builders.

A very interesting point about the Tirynthian palace is its very careful method of drainage, partly with neatly fitted clay drain-pipes, and partly with large culverts built of rough stone and puddled inside with clay: this latter form was used for the main drains, while the branches which led to it were of pipes square in section, each length of clay pipe being narrowed at one end so as to fit closely into the next. All the open courts were well paved with concrete, which was laid so as to fall to a surface-gully, down which the rain-water passed, first through a clay pipe, then into the main stone drain, and so into a series of cisterns, where it was stored for use during a siege.

Much manual skill and great variety of tools were used by the masons who worked the stones for this building. Pointed hammers were used for the rough work, and chisels for the ashlar stone: the large thresholds of the various doorways were cut with a saw, with which emery must have been used, as its marks show that each stroke of the saw cut a considerable depth into the stone. Hollow drills set with some kind of hard jewel were also used here: in many of the drill-holes used to fix the pegs or dowels of the wood-work above, the stone stump of the core still exists, showing that a tubular, and not a solid, drill was used. Some of the large quoins or angle blocks were quarried thus—four drill-holes were sunk at the four corners of the future block, and then saw-cuts were made from hole to hole.

This use of tubular jewelled drills, which has recently been introduced with such effect into modern methods of engineering, dates from a very early period. As Mr. Flinders Petrie has pointed out, jewelled drills, both solid and tubular, were used in Egypt as early as 4000 years before Christ, especially in the working of the very refractory granites, basalts, and porphyries, which no unaided metal tools could possibly have cut. That jewels *fixed* in the rim of the metal tube were used, and not merely loose corundum or emery-powder, is shown by the fact that the scratch from a single projecting jewel can often be traced continuously round the spiral markings on the insides of the drill-holes.

It is not, however, only the mere technical details of the workmanship of this Tirynthian palace that bear strong witness to its early date, but also the methods of construction—the walls of sun-baked bricks set on a footing of stuccoed rubble, the use of wood instead of stone for the columns, and the magnificence of the walls lined with plates of bronze, *repoussé* and gilt.

Finally, nothing can be clearer than the evidence supplied by the semi-Oriental style of the wall-paintings, and the distinctly archaic character of the delicately sculptured alabaster frieze, studded with gem-like pieces of blue *κύανος*—exactly as was once the case with the central row of spirals in the well-known architrave from the doorway of the "Treasury of Atreus" in the British Museum, the remote antiquity of which is disputed by no one. In fact the methods of execution, the system of its construction, and the style of its decoration all combine to show that we owe to Dr. Schliemann and Dr. Dörpfeld the discovery of an almost new phase of prehistoric Greek art.

J. H. M.

ON VARIATIONS OF THE CLIMATE IN THE COURSE OF TIME[1]

If we examine the meteorological charts of Norway we observe at once what a great influence the sea and the mountains exercise over the climate in various parts. Nearly all the climatological lines run more or less with the shape of the coast, so that we encounter far greater variation when proceeding from the centre coastwards

[1] The following is a short abstract from various papers, viz.: "Essay on the Immigration of the Norwegian Flora during Alternating Rainy and Dry Periods" (Christiania, 1876). "Die Theorie der wechselnden kontinentalen und insularen Klimate," in Engler's *Botanische Jahrbücher*, ii. (Leipzig, 1881). "Ueber Wechsellagerung und deren mutmassliche Bedeutung für die Zeitrechnung der Geologie und für die Lehre von der Veränderung der Arten," in *Biologisches Centralblatt*, iii. (Erlangen, 1883). "Ueber die wahrscheinliche Ursache der periodischen Veränderungen in der Stärke der Meeresströmungen" *l.c.* iv. (Erlangen, 1884).

than from south to north. In keeping with the same are the variations of the flora.

The plants of Norway may be divided into certain groups of species, the species belonging to the same group having a somewhat similar extension, whilst each of these groups of species is confined to special climatological conditions, and is only found in those parts where such prevail. The Norwegian flora is in the main monotonous. On the mountains large areas are covered with only a few lichens, mosses, and heather, or copses of dwarf birch, juniper, and willows; lower down the forests are formed of birch, fir, and spruce, and have a monotonous flora, viz., heather and lichen in the fir forests, "blue" berries and a few kinds of moss in the spruce forests, whilst the west coast is covered with heather, and the numerous marshes with a vegetation, poor in species, of a few mosses and Carices.

But in spite of this general monotony of the flora of the mountain wastes, with their grayish-yellow lichens, grayish-green and green copses of willows or dwarf birch, there are certain places, particularly on slaty ground, where a rich vegetation may be found. It consists of small perennial plants some inches in height, and which are particularly distinguished by their copiousness of flowers, which are very large in proportion to the size of the plant, and have very pure and lovely colours. Outside Norway we also encounter these plants in Arctic regions, and the Alpine flora of these slaty tracts is therefore of Arctic character. But not all slate mountains have such a varying flora. The coast climate is, in consequence of the mild winters, when the temperature frequently changes, destructive to these plants, which shoot at a very low degree of heat. It is for this reason that, when we mark those places on the map which have a rich Alpine flora, they lie scattered as oases over the land with great spaces between them, but always sheltered from the sea-winds, *i.e.* on the east or north-east side of the highest mountains and greatest glaciers, which act as barriers against the mild climate of the coast. In these places the botanist may fancy himself transferred to Spitzbergen or North Greenland; he finds the principal plants encountered there, and if we follow the Arctic flora to Spitzbergen we find that here also it shuns the sea, and is most copious in the bottom of the fjords.

In the lower districts, sheltered from the open sea, we find in favourable spots another group of plants which also shun the coast, and which thrive on loose slates and warm limestone cliffs, or in screes of different kinds of rock, under precipitous mountains, facing the sun. These screes are generally full of bare boulders at the bottom, but in the finer debris higher up grows a wreath of green underwood, formed of tender deciduous trees and shrubs, hazel, elm, lime, maple, dog-roses, *Sorbus Aria*, *Prunus avium*, wild apple, &c., as well as a number of highly-scented Labiatæ, several Papilionaceæ, grasses, and a great number of other plants, together forming that part of the Norwegian lowland flora which shuns the open sea-coast, and prefers the fjords and the sunny valleys. But even this flora has a scattered extension. It is richest in the tracts around Christiania, and becomes poorer westwards along the coast, disappearing almost entirely on the coasts of the province of Bergen; but at the bottom of the Sogne and Hardanger, and along the Throndhjem fjords we find the same flora, and that in spite of these parts being entirely separated by enormous mountains.

Near the open sea the flora becomes poorer in species, most of those characteristic of the interior disappearing, whilst their number is not by far made up by those belonging to the coast. Here we shall only name a few of the coast plants, such as the holly, the ivy, and the foxglove, whilst in place of the *Primula veris* of East Norway we have the *Primula acaulis* of the west coast. In the woodless tracts of the coast the heather predominates, and besides the ordinary common one we find two other species. This group of plants belongs exclusively to the south and west coasts, and is hardly found north of the Throndhjem fjord. Most of its species are not found near Christiania, but they reappear in the south of Sweden. Some, however, are in Scandinavia only found on the west coast of Norway, and we must travel to the Faroe Islands, Scotland, England, and Belgium to re-encounter them.

We have thus seen that the Norwegian flora consists of groups of species which make different demands as to climate. If we were to colour a map according to the places where certain groups are most copious, we should at once discover that they had a scattered distribution. We should find the same colour here and there, in smaller or larger patches, but those of the same colour would be separated by great spaces of a different tint.

At one time botanists were satisfied with explaining the distribution of species through soil and climate, but as the study of their appearance proceeded it was discovered that there were great gaps in the extension of many. And these gaps were often so great that scientific men were obliged to resort to explaining the same by maintaining that such species were created in places far apart. But since the doctrine of the origin of species by descent has been accepted, such an explanation must be rejected. There remains, therefore, only two ways in which to explain these things. Either wind, animals, or sea-currents are capable of carrying the seed of plants at once across such large areas that the gaps in the extension can be explained by the means of transport at work at present, and there are even those who still believe that this is the case. In certain instances this explanation is indeed the only one possible, when, for instance, it concerns the flora and fauna of the oceanic islands which have never been connected with the great continents, and still have species more or less related to those of the mainland. But such a sudden migration is very improbable, and may even be dispensed with altogether, as we shall presently show, when it is necessary to explain such gaps in the extension of whole groups of species as those we have pointed out above in the flora of Norway.

We have, besides, another explanation of this problem, first advanced by Mr. Edward Forbes, who maintained, in common with most modern botanists, *that the climatic variations of the past are reflected in the fauna and flora of the present.* He was, we believe, the first *savant* who demonstrated that the Glacial Age has left its distinct mark on the flora of the present day. Arctic species are found on mountains in temperate climates. During the Glacial Age these species grew in the plains at lower latitudes, but as the climate became milder they receded gradually to the far north and the high mountains. In the warm plains they had to give way to the new immigrants, and this is the reason of our discovering hyperborean plants on the mountains of Europe.

If now we were to apply this explanation to the scattered extension of the species in Norway, we must bear in mind that the distances here are smaller, although at times there are several degrees of latitude between the places where the same appear. We must, therefore, see if an acceptable explanation of the extension of the Norwegian flora can be made by means of geology, and if the same be supported by other circumstances.

It is not long since, geologically speaking, that the Scandinavian peninsula was covered with an inland ice, stretching right out to sea, above which only solitary mountain-tops rose, like the "nunataks" in Greenland. It is evident that the majority of the present flora could not then exist in Norway; but the present flora is older than the Glacial Age, which is conclusively proved by specimens from the same being found in coal strata older than that period. Thus yew, fir, and spruce, hazel, willow, &c., have been found in old peat-bogs of England and Switzerland, for instance, which are covered by the bottom moraine of the inland ice. The present Norwegian flora, there-

fore, must have lived in other countries which were free from ice during the Glacial Age, and immigrated to Norway as the climate became milder and the ice receded. This is the reason of Scandinavia having no peculiarly characteristic species, *because the flora has immigrated from outside countries, and the time is so short since it settled in the country that it has not yet had time to produce new species.*

If we may now apply the geological theory of explanation to the flora, we come to the conclusion that the immigration took place during repeated changes in the climate. After several thousands of years with a severer climate which favoured the immigration and extension of northern and eastern species, other thousands of years followed with a milder climate. During this period fresh immigrants came from the south and south-west, compelling the older flora to retreat. In this manner the climate must have changed several times since the Glacial Age, and the distribution of the plants must have changed in accordance therewith. The periods of variation are reflected in the present flora, and it is the former which have led to the great gaps in the extension of coast as well as inland plants. The sunny screes, the slate districts, and the moist coast tracts are asylums where the different floras have found refuge. In the intermediary parts they have been dislodged by the newcomers. But certain species, being indifferent to the variations, extended constantly, at the expense of others, *and this is the reason of the Norwegian flora being so monotonous.*

In order to test the accuracy of this assertion we shall first turn to the peat-bogs and examine their structure. We shall, for comparison's sake, also examine the Danish ones, which are well known from the researches of Prof. Steenstrup.

In the forest and mountain districts of Norway there are innumerable marshes. In the forest districts most of them are now comparatively dry, the heather and wood covering parts of the bog, and on the surface of the latter tiny mossy knolls are often found, in the middle of which stands the old stump of a tree. An examination of the structure of the peat layers—which is easily made with a bore—shows that previous to the present time, when the surface is generally more or less dry, there was a period when the bog was much more watery. Under the present conditions the growth of the peat is arrested, at all events in dry places. But just below the lichen and heather-covered surface we find on boring a pure, unmixed white moss (Sphagnum). It is this moss in particular which has formed the peat in the Norwegian bogs; and in the upper layers—only one or two feet from the surface—flint implements from the Stone Age are often found. At the period this upper layer of Sphagnum was formed the bogs were woodless because they were too watery. We see, therefore, that the peat in these bogs has not grown very much within historical times, and that the layer of stumps of trees, which are found on the surface in the knolls, indicates an arrest of the growth of the peat, the duration of which may probably be measured by many hundreds, perhaps by thousands, of years. It might be argued that the present drier state of the bogs was simply due to the circumstance that the peat had grown so high that the moisture had run off. But this is not an acceptable explanation, because if we bore deeper in the peat we find that the oldest bogs are built of four layers of peat, *and between these stand three layers of stumps, so that these bogs are for the fourth time covered with trees since they began to form.* And as most of the bogs, if not all, are at present drier than they were before, the theory of merely local variations of the moisture is also insufficient to explain the phenomena. It remains, therefore, only to assume *that periods of dry and wet have alternated during ages.* The peat layers generally belong to the latter, and the stump layers speak of drier periods, when the bog was covered with trees.

Of these four layers of peat, which in some places measure upwards of twenty-six feet in thickness, only the two youngest inclose, as far as the researches in Norway go to show, remains of foliferous trees sensitive to cold. And this justifies the assumption that they correspond to the four layers which Steenstrup has shown in the bogs of Denmark, and which appear like geological strata with distinct fossils, viz., the aspen, the fir, the oak, and the black alder. This comparison of the peat layers of Norway and Denmark is further supported by the circumstance that layers of stumps are also found in the Danish bogs, and here, too, they stand between the peat layers of the various periods. They indicate long periods, during which also the Danish bogs were dry and partly covered with forests when the peat ceased to grow. But during these dry times the flora was changed through the immigration of new species, and when a wet time again set in, it was other trees which grew around the bogs, and which spread their boughs, leaves, and fruits over the watery bog, and the remains of which were buried by the growing layers of peat.

In this manner the structure of the peat confirms the conclusion to which the distribution of the flora pointed, and if we take the fossil plants and marine shells to our aid we may explain the gaps in the extension of the species without assuming long transports of seed.

In the freshwater clay of Scania and Seeland, Prof. Nathorst has discovered numerous remnants of Arctic plants. This clay lies *below* the peat. When it was deposited in the cavities of the old bottom moraines of the inland ice, not only the dwarf birch, but even hyperborean plants, such as the Arctic *Salix polaris* and others, flourished in the southernmost parts of Scandinavia: *therefore the Arctic flora was the first which immigrated into Scandinavia.* It entered whilst the climate was very severe; but the climate became milder and more moist: the peat began to form; then the aspen and birch entered, and, later on, under varying conditions of moisture, the fir and the spruce, with the flora of the mountains and forest glens, a series of species which have not yet been mentioned, viz. Mulgedium and Aconitum, many great ferns and grasses, wood-geraniums, and lychnis, &c. But the climate became warmer and warmer; and finally the foliferous trees, more sensitive to cold, entered, viz. the hazel, the lime, the ash, the oak, the maple, and a number of others from warmer regions. In the province of Bohus quantities of stones of sweet cherries are found in many places, in peat, where this tree is now extinct; and in the Norwegian peat-bogs hazel-nuts are very frequent in a certain layer, not only in the interior of the great coniferous forests, where not a single hazel-tree is found, but even in the heathery, woodless coast-lands. It will, therefore, be seen that the hazel and the sweet cherry were then very plentiful, and from this we may justly conclude that the trees, and shrubs, and herbs which thrive in their company were also once far more plentiful than at present. *It is this flora which has found an asylum in the above-mentioned screes.*

Following the period when Southern Norway was covered with foliferous forests to a far greater extent than now came a warm and moist one, in which the peat again began to grow. At that time the coast oak (*Quercus sessiliflora*) was far more frequent than at present, judging by the evidence of the peat-bogs, and at that time, the shell deposits inform us (as shown by Prof. M. Sars), the present marine animals of the west coast were found in the Christiania fjord. *And there is every reason to assume that the present flora of the west coast immigrated thither at that period from the south of Sweden along the Christiania fjord to the west coast.*

New changes again set in, with new immigrants, and finally came the present age with its comparatively dry climate. But all these events are prehistoric, as is shown by the stone implements lying in the uppermost peat layer, close under the surface.

Thus, the remains of plants and animals in clay, peat, and shell deposits inform us *that the gaps in the extension of the species in Norway may be explained by the varying events of times long gone by.*

Since the Glacial Age the relation between sea and land in Norway has changed. Formerly the sea was in some places upwards of 600 feet higher than at present.[1]

The clay at that time deposited on the sea-bottom, and the shell deposits formed near the shore, contain, as Profs. M. Sars and Kjerulf have taught us, remains of Arctic animals even in the southernmost parts of the country. There is a difference of opinion between *savants* whether this alteration of the shore-line is due to a rising of the land or the sinking of the sea, or to both. There is further some dispute about the manner in which the level became altered, some maintaining that it took place suddenly at intervals, whilst others believe that it is the result of a gradual and continuous process. The marks left by the sea seem at first glance to corroborate the first of these theories. Thus, in the lower parts of our valleys we find along the river-courses terraces of sand, pebbles, and clay, one behind and above the other right up to the highest old shore-line. The terraces, of which Kjerulf, pre-eminently amongst others, has given us particulars, have an even surface and a steep declivity outwards against the mouth of the valley. They contain sometimes remains of sea animals. Under a higher level of the sea the river carried down sand and gravel to its mouth, just as in the present day banks and bars are formed at the estuary of our rivers. And the terraces seem to indicate that the changes in the level were broken by periods of rest. During the latter the river had time to form a bank, which rose comparatively rapidly; the next period of rest gave occasion to the formation of another terrace, and so on. But this theory has to combat many obstacles, because the terraces lie often, as Prof. Sexe has shown, even in valleys situated near each other, *at different elevations*. The professor is of opinion that step-like terraces may be formed even under a gradual and steady rising, if the carrying-power of the river is subjected to changes. Our theory may therefore probably also be applicable for explaining the terraces, because, if long periods with milder climate have alternated with others whose climate was more severe, it is evident that the volume of water, and thus the carrying-power of the current, may have altered. Perhaps the rivers have at certain times carried down floating ice, at others not, and the thaw in the spring must have increased the carrying-power. We can thus understand why the corresponding terraces in valleys near each other do not always lie at the same elevation. Their rivers differ in size, and when the carrying-power diminishes a big river will retain the strength to form a terrace longer than a small one.

Besides these terraces, which are particularly conspicuous in the short steep valleys on the west coast of Norway, and on account of their regularity must excite the admiration of every one who sees them, there are other equally striking marks of the old sea-levels, viz. the so-called "Strandlinjer"—shore-lines—which are known chiefly through the researches of Prof. Mohn and Dr. Karl Pettersen.

When travelling through the fjords and sounds, particularly in Northern Norway, one sees here and there horizontal lines drawn along the mountain-sides, sometimes several hundred feet above the sea. They are not always equally marked, but appear often remarkably clear; sometimes they look like roads or railway-lines. They are always horizontal, or nearly so, and must, therefore, be remains of an old sea-shore. Often two parallel lines are seen running one above the other in the same place; and on closer inspection it will be discovered that they are hollowed out of the rock itself. They have a surface sometimes many feet broad, and are bounded behind by a more or less steep mountain-wall, forming thus horizontal incisions in the same. The shore-lines have also been brought to prove that the rising was broken by periods of rest, during which the sea had time to hollow out the rock; but I am of opinion *that they could be formed, too, under a gradual rising, if the climate be subjected to periodical changes.* The shore lines belong to the northern parts of the country and the deep fjords, where the winter cold is more severe, and they are only found in districts where there is a tide. They seem to have been blasted out by the influence of the cold. At high tide the sea-water fills the holes and fissures in the rock, and when the tide recedes it is left in the same. In severe winters the water will freeze, and thus burst the rock. During the rising of the land, shore-lines will be broken out in this manner, as long as the erosion is able to keep pace with the rising. When the climate becomes milder, a time will come when the erosion is unable to continue. Then the shore-lines will be lifted up above the level of the sea, and out of the reach of the blasting influence of the water. If next, after thousands of years, when the land has perhaps risen fifty or a hundred feet, a period follows with a severer climate, a new shore-line is formed below the former.

The shell-banks, too (*i.e.* deposits of shells of marine animals living in shallow water near the shore) lie, as Kjerulf has shown, in the Christiania fjord at different levels, the oldest at heights of from 540 to 350 feet, and the youngest between 200 and 50 feet above the present level of the sea. But between 350 and 200 feet none has been found. In the neighbouring Swedish province of Bohus they are found at all elevations, even between 350 and 200 feet, and it must therefore be assumed that local causes, as, for instance, the ice formation in the more closed Christiania fjord, destroyed the shell-banks when they reached the shore-line, at a period when the land lay 350 to 200 feet lower in relation to the sea than at present. According to the evidence of the peat-bogs, there is reason to believe that this part of the rising occurred under a more severe climate.

It is therefore seen that all the facts which have been advanced in order to prove that the rising was broken by periods of rest may be easily explained, *if we assume that the land rose gradually and steadily under periods alternating with milder and severer climates.*

The University, Christiania A. BLYTT

(*To be continued.*)

[1] The depth of the peat in the parts which were formerly below the sea increases with the height above its surface, because the formation of the peat commenced long before the lowest-lying parts had risen above the surface. From the remains of plants found in the various peat layers we may therefore learn how the Norwegian flora was composed during the various phases of the rising of the land.

HYPERTRICHOSIS

I THINK all naturalists, and anthropologists in particular, will be interested in the cases of human hypertrichosis now on view at the Egyptian Hall, Piccadilly. I myself spent two hours with them on Saturday last.

This family of hairy people have been at the Court of Burmah for four generations. Crawfurd saw Mahphoon, the old woman now exhibited, an infant in 1827; the family was described by Col. Yule in his narrative of a Mission to the Court of Ava in 1855.

It is singular that the hypertrichosis of Mahphoon's grandparent should be continued not only to herself but to her son, Moung Phoset, also exhibited, inasmuch as one of the parents has always been an ordinary comparatively hairless Burman, so far as the face and body are concerned.

Mahphoon is now an old blind woman, but very lively, full of fun, and an inveterate chewer of betel; her face

and ears are entirely covered with hair, particularly thick on the nose. Her son, Moung Phoset, is more hairy on the face and ears than his mother—probably her locks are somewhat thinned by age—his forehead is densely clothed with hair, which, when combed over his face, entirely hides his features, the hair being 12½ inches in length; he parts it over the eyebrows and passes it behind his ears; it is also very long on the nose, and being parted in the middle and falling over the cheeks gives his face a most remarkable resemblance to that of a Skye terrier. The suggestion was so strong on my mind that I could scarcely divest myself of the canine idea.

The whole of his body is clothed with soft hair some inches in length, but I am informed that he has usually had this cut from time to time, so that its natural length is not apparent. The hair of Moung Phoset and of his mother Mahphoon is very soft and wavy, of a brown colour, and utterly unlike the coarse black hair of the ordinary Burman.

Capt. Paperno, who obtained them, and has been fifteen years in Burmah, informs me that the dentition of all these hairy people has been imperfect, whilst their less hairy brethren and sisters have had perfect teeth.

I have examined a cast of Moung Phoset's mouth. In the upper jaw he has but two canines and two large incisors, in the lower jaw two canines and four small incisors; the premolar and molar teeth are quite absent.

A nephew of Mahphoon, who is exhibited with them, has the appearance of an ordinary Burman only.

I believe that it is owing to the enterprise of Mr. Farini that we are enabled to see this singular family in London.

They are both far more hairy than Krao, who was exhibited in London some time since, and is now at Paris in good health; she was obtained from a district east of Burmah, and north of Siam; the features of the Burman family are so obscured by hair that I could not ascertain whether there was any resemblance to those of Krao, nor even whether they were Mongoloid.

Moung Phoset has been well educated, writes fluently in the Burman character and language, and possesses considerable power in the delineation of objects; like many Burmans he is tattooed from below the waist to above the knees

I have seen a photograph of a brother of Mahphoon now dead; he was quite as hairy as his sister, but the peculiarity did not, I understand, extend to the whole of the family.

J. JENNER WEIR

Chirbury, Beckenham, Kent

NOTES

PREPARATIONS are being made by Parisian men of science for the celebration of the 100th anniversary of the birth of M. Chevreul, on August 31 next.

A REGULATION as old as the French Academy of Sciences has just been broken through in Paris. Women have hitherto been excluded from the sittings of the Academy, but at the meeting of the 28th ult. the interdiction was raised in favour of Mdlle. Sophie Kowlewska, Professor of Mathematics at the University of Stockholm, and daughter of the eminent palæontologist. Admiral Jurien de la Gravière, who presided, welcomed her in graceful terms, and said that her presence should be a cause of pride and pleasure, not only to the mathematicians present, but to the whole Academy. As she entered, the whole of the members rose to salute her. She took her place between Gen. Faye and M. Chevreul.

ACCORDING to official decree, the Tokio University and the Imperial College of Engineering having been amalgamated into the Imperial University of Japan, they now cease to exist. The new University comprises five colleges or sections: (1) Law; (2) Medicine; (3) Engineering; (4) Literature; (5) Science. Each of these, as well as the whole institution, is placed under a Japanese director. The director of the Science College is Prof. Dairoku Kikuchi, a Cambridge Wrangler, and the same gentleman is acting for the present as head of the Engineering College also. The large and splendid buildings erected for the Engineering College—the finest pile of European edifices in Japan—will, it is said, be used in future as a school for the children of nobles.

HERR FENNEMA, a mining engineer at Buitenzorg, in Java, has made some observations on the recent volcanic eruptions in that island which are of interest as setting at rest a matter on which some doubt has existed. On the authority of Junghuhn, the general belief has been that in historic times all the volcanoes of Java (and of Sumatra it may be added) had thrown out solid matter only, and never those streams of lava which are so characteristic of most eruptions. But a careful examination of Smeru and Lemongau during the catastrophe of April last year shows that this notion must be abandoned as incorrect. The former is not only the highest but also the steepest in Java. From 700 to 1400 metres the slope is about 6°, up to 2100 it is 20°, and from 2100 to 3671 metres it is more than 30°. For a considerable way from the summit the striking cone consists wholly of the detritus thrown out regularly by the almost uninterrupted activity of the crater. Up to April 1885 the existence of torrents of lava was unknown. On the 12th-13th of that month a stream appeared on the south-eastern side, and forced the residents on the plantations lower down to fly. The stream increased for several days, until it reached a height on the mountain-side of about 2100 metres from the level of the sea. The loss of life was due to the avalanche of stones sent down the steep sides of the mountain by the stream. Similarly, at the same time, Lemongau threw out a lava stream, but there was a curious difference between this and the one issuing from Smeru—the latter was andesitic in its character, while the former was basaltic.

WE have received from Mr. Henry Farrar, 6, Hanway Street, W., photographs, seven in number, selected from a very extensive collection taken by a native of India, Lala Deen Diyal. One consists of the whole view of the rapids of Chichai waterfall, near Reira, which are 400 feet deep; another, a river view at Indore. The photographs themselves are exquisite; in looking at some of them one might imagine one's self in the tropics surrounded by the wonderful vegetation of that region. The tone of them is very fine, especially in the one "Channel below the Keuli waterfall, near Reira," the velvety appearance of the vegetation on the hillsides is in strong contrast with the sharp and clear detail of the white and waterworn stones in the river bed. To the various lovers of nature as well as students of art and archæology a possibility of getting quite perfect photographs of the natural and artistic wealth of India at a low price should be very welcome.

IT is stated that the explorations for coal conducted by Dr. Warth in the Salt Range in the Punjab have proved so satisfactory that the Government is now arranging for the practical working of the seams. Dr. Warth estimates that over one million tons are underlying the plateau at Dundote. The coal is not of the first quality. It contains iron pyrites and is very friable, but it is believed that it will be very useful for the North-Western railways.

WE have received several communications relating to the letter signed "P." in NATURE for May 27, p. 76, on "Male Animals and their Progeny." Mr. Arthur Nicols has noticed several times a common cock marshalling a brood of chicks,

picking up food for them, calling them together on the approach of danger, and even "brooding" them at night. A case was communicated to him a few years since by Dr. James Gale, in which a turkey cock incubated six fowl's eggs during the whole period, successfully producing three chickens, and continuing to treat them with all the care of a hen. The hen turkey with which he had been mated was unfortunate in her brood, and this circumstance appears to have impelled him to take possession of the fowl's eggs. Besides the fact that in a considerable number of species the male not only takes his turn at incubation, but continues, equally with the female, to feed the young after they have left the nest, we have the case of the male emu, who performs the task of incubation alone. The male ostrich, too, as observed in a semi-domestic state, undertakes a large, and sometimes it would seem the entire, share of nidification. When the pair of Apterix in the Zoological Gardens nested, the male alone sat assiduously during fifteen weeks on the two eggs, which, however, proved infertile. A correspondent from Melksham also records a case in which a bantam cock brought up a brood of chickens, the mother having died when they were two days old. Mr. Hyde Clarke quotes instances of similar care bestowed by male dogs and cats in Turkey on the young.

The curious case of the emu is described in a letter from Mr. Alfred Bennett, who had an opportunity of watching the habits of this bird, which was, during several seasons, successfully bred by his father in Surrey. The hen bird, says Mr. Bennett, begins to lay about the end of October or beginning of November, and as each brood consists of twenty eggs or more, laid at intervals of two days, the process takes about six weeks. Before it is completed, the cock bird begins to sit. The eggs laid subsequently are deposited by the hen by the side of her mate, who puts out his foot and draws them under him. As soon as the eggs begin to hatch it is necessary to isolate the hen, as she fights furiously with her mate, and would to all appearance kill the chicks if she were allowed to get at them. The whole of the tending of the young is performed by the male bird.

An opah, or king-fish (*Zeus luna*), which is an exceedingly rare fish, was recently captured off the Shetland Islands and brought to the Colonial and Indian Exhibition for inspection. The specimen, which is in perfect condition, measures about 5 feet in length, and weighs 160 pounds. The colours of the sides and back are dark green intermingled with gold and purple, while the irides are red. The opah seems to possess peculiar migratory propensities, being found at various parts, even in Eastern seas. The habits of this fish seem to be little known, but Mr. W. August Carter, of the Colonial and Indian Exhibition, states that, according to inquiries and investigations he has made, the opah varies its diet according to the locality it inhabits, and that when visiting the British Islands it feeds chiefly upon herrings and cuttlefish.

In one of the Courts of the Colonial and Indian Exhibition is a very fine raven, presented by the Maclaine of Lochbuie. This bird, on account of its sagacity, creates much amusement and interest amongst visitors. On being fed it partakes of so much of the food as it requires, then hides the remainder in certain parts of its habitat beneath pieces of paper and other articles that happen to be about. As many as four hiding-places are made use of by the raven for storing its food, which it exhumes when desirous of feeding. It is a curious fact that the raven only resorts to such stratagems when being watched by the public, at other times this sagacious bird consumes its meals in their entirety at one time.

The additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus*) from India, presented by Mr. Albert Thorne; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. S. R. Hicks; a Prairie Wolf (*Canis latrans* ♀) from Winnipeg, presented by Mr. Gerald F. Talbot; a Common Fox (*Canis vulpes*), British, presented by Mr. A. Browning Priestley; a Brown Bear (*Ursus arctos*) from Asia, presented by Capt. Asher Smith; a Stein-bok Antelope (*Neotragus tragulus*) from South Africa, presented by Mr. W. J. Robertson; two Violaceous Night Herons (*Nycticorax violaceus*) from South America, presented by Dr. A. Boon, F.R.C.S.; a Mona Monkey (*Cercopithecus mona*) from West Africa, a Grey Squirrel (*Sciurus cinereus*) from North America, a Greater White-crested Cockatoo (*Cacatua cristata*) from Moluccas, deposited; six —— Sousliks (*Spermophilus* ——), five American Flying Squirrels (*Sciuropterus volucella*) from North America, two Glass Snakes (*Pseudopus pallasi*) from Dalmatia, purchased; two Mule Deer (*Cariacus macrotis* ♀ ♀), a Yak (*Poephagus grunniens* ♀), four Long-fronted Gerbilles (*Gerbillus longifrons*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

Method of Correcting for Differential Refraction in Declination.—Mr. McNeill, of the College of New Jersey, Princeton, has published in the *Astronomische Nachrichten*, No. 2735, a method of correcting micrometer observations for refraction which was devised originally for the diagonal-square micrometer, but is applicable also to the ring micrometer and others of the same class. In this method the correction to the difference of declination is not determined separately, but the true difference is directly determined, the corrections being applied to the logarithms in the course of the computation. Mr. McNeill shows that if we apply the number given by

$$M\kappa(\tan^2\zeta\sin^2 q + 1)$$

to the logarithm of the half chord traversed by the star, and the corresponding number deduced from

$$M\kappa(\tan^2\zeta\cos^2 q + 1)$$

to the logarithm of the apparent distance, measured on a circle of declination, from the point of reference in the micrometer, the result obtained will be the true distance corrected for refraction. In the above expressions M is the modulus of the common system of logarithms, κ the constant of differential refraction, ζ the true zenith distance, and q the parallactic angle. It is then only necessary to tabulate the expression

$$M\kappa\{\tan^2\zeta\cos^2(p - q) + 1\}$$

with arguments $p - q$ and ζ, adding subsidiary tables giving barometer and thermometer factors, in order to obtain the quantities required (by making p alternately $= 90°$ and $= 0°$) to correct the micrometer observations for differential refraction in declination. This Mr. McNeill has done, and his tables will doubtless be of much use to observers using the class of micrometer to which the method is applicable.

New Minor Planet.—A new minor planet, No. 259, was discovered by Prof. C. H. F. Peters, Clinton, New York, on June 28. Minor planet No. 253 has been named Mathilde.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 JULY 11–17

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on July 11

Sun rises, 3h. 58m.; souths, 12h. 5m. 12·6s.; sets, 20h. 12m.; decl. on meridian, 22° 6′ N.: Sidereal Time at Sunset, 15h. 30m.

Moon (three days after First Quarter) rises, 15h. 49m.; souths, 20h. 38m.; sets, 1h. 22m.*; decl. on meridian, 15° 14′ S.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	6 17	13 51	21 25	17 0 N.
Venus	1 32	9 30	17 28	20 42 N.
Mars	11 8	17 2	22 56	1 59 S.
Jupiter	10 31	16 42	22 53	1 22 N.
Saturn	3 30	11 39	19 48	22 25 N.

* Indicates that the setting is that of the following morning.

Occultation of Star by the Moon (visible at Greenwich)

July	Star	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image
			h. m.	h. m.	° °
17 ...	B.A.C. 7097 ...	6 ...	3 44 ...	4 55 .	130 323

Variable Stars

Star	R.A.	Decl.			
	h. m.	° ′		h. m.	
U Cephei	0 52·2 ...	81 16 N. ...	July 13,	23 52	*m*
U Libræ .. .	14 34·0 ...	17 10 S. ..	,, 15,		*M*
δ Libræ	14 54·9 ...	8 4 S. ...	,, 17,	22 14	*m*
U Coronæ	15 13·6 ...	32 4 N. ...	,, 15,	0 40	*m*
U Ophiuchi	17 10·8 ...	1 20 N. ...	,, 12,	1 26	*m*
		and at intervals of		20 8	
X Sagittarii	17 40·4 ...	27 47 S. ...	July 17,	2 0	*M*
W Sagittarii ...	17 57·8 ...	29 35 S. ...	,, 16,	0 0	*M*
U Sagittarii	18 25·2 ...	19 12 S. ...	,, 16,	2 0	*m*
R Lyræ	18 51·9 ...	43 48 N. ...	,, 13,		*M*
T Sagittarii	19 9·7 ..	17 10 S. ...	,, 12,		*M*
R Sagittarii	19 10·0 .	19 30 S. ...	,, 12,		*M*
δ Cephei	22 24·9 ..	57 50 N. ...	,, 15,	3 0	*M*

M signifies maximum ; *m* minimum.

GEOGRAPHICAL NOTES

THE report published by Lieut. von Nimptsch, of the German army, gives some very interesting details of the journey he made with Herr Wolff, a traveller in the service of the Congo Free State, and which has resulted in the discovery of a river likely to be of material value to traders with the Congo. The Congo, in its course from the south-east, makes a very wide bend to the north, and then descends again to the Atlantic, a very large tract of country being embraced in this curve. Within this is the River Kassai, which Lieut. von Nimptsch regards as being "of even greater importance to commerce than the Congo itself." Describing their journey he says that, as far as Luebu, the Kassai flows through wide plains, well adapted for cultivation and pasturage, and forests of palms and gutta-percha trees. There are many villages on the banks, and the travellers met with great civility in all of them save one, the inhabitants of which fled at their approach. "One tribe," adds Lieut. von Nimptsch, "was remarkable for its joviality. The natives accompanied the steamer in their canoes, and when we could, organised dances and songs in our honour." There is a great deal of ivory all along the Kassai, and large pieces of the finest quality were readily given in exchange for empty boxes and tins. They discovered several affluents of the Kassai, and they calculated that they were navigable for a distance of 250 miles. "But the most important affluent," the report goes on to say, "is that which Herr Wolff explored in the steamer *Vorwarts* during the months of February and March. He ascended this stream to a distance of 430 leagues from its mouth, and one of its northern affluents brought him to within a week's march of Nyangwe. He might have gone still further had his steamer not met with an accident, for there are no cataracts in this river. All this network of navigable water, extending over more than 3000 miles, is most admirable, and in future it will be possible to travel eastward from the Atlantic, reaching Nyangwe and then Lake Tangyuteka by leaving the Congo at the mouth of the Kassai, without being obliged to ascend the whole of the former stream, thus avoiding the Stanley Falls."

A TELEGRAM from Zanzibar, of the 30th ult., states that Dr. Fischer had returned there. He has not succeeded in rescuing Herr Junker, the African traveller, who, when last heard of, was in the region north of Uganda.

A VERY interesting discussion which took place at the St. Petersburg Society of Naturalists after the reading of a paper by Prof. Beketoff on the South Russian steppes as compared with those of Hungary and Spain is now summed up in the *Memoirs* of the Society (vol. xxv. 2). The Russian steppes between the Pruth and Don, although belonging to the great "steppe region" of Grisebach, differ, however, from the remainder of the region inasmuch as they support agriculture without irrigation. They are akin, in this relation, to the Hungarian *puzhtas*. Being comparatively well watered, they belong more to Europe than to Asia, while those beyond the Don and the Volga bear a truly Asiatic character. As to the *disiertos* of Spain, they are more akin to the deserts of Africa than to the steppes of either Central Asia or Europe ; they have, however, some likeness to those of Transcaucasia. As to the causes of the want of forests in the Russian steppes, Prof. Beketoff explained it by the circumstance that, being covered with salt-clays, after the emergence from the sea, they were, first, inappropriate to the growth of forests. As the surface, however, lost by and by its salt and became covered with grasses, masses of ruminants were attracted into the region, and these ruminants prevented the appearance of trees, destroying them as soon as they appeared ; the climate being most unfavourable for the spreading of forests, the ruminants were also an important factor in the prevention of their appearance. The American buffaloes are an instance of the same influence. Dr. Woeikof fully confirmed the view taken by Prof. Beketoff, but pointed out that the burning of the steppes by man played also a most important part in the prevention of the appearance of forests. In America he was told of several instances where the trees began to grow as soon as the burning of prairies was stopped. Cattle are surely a great enemy of appearing forests. The very dry season of 1857 partly destroyed the cattle in Texas, and partly compelled to send it away to the mountains, and immediately the *Mesquita* began to spread in the prairies. It had time to take root before the cattle were brought back, and now it grows freely. The same has been seen on the *llanos* of Venezuela. The continuous wars and requisitions have led to a notable diminution of cattle, and now we do not find the boundless steppes of former times ; there are at least bosquets of trees. Mr. Jonas supposes that this change has even slightly modified the climate. Prof. Sovyetoff supported the same views, pointing out that cattle are an enemy not only of forests, but also of the grass covering of the steppes. He mentioned an instance of a large estate of 800,000 acres of virgin steppes in Taurida, where nearly half a million of sheep are grazing. The grass vegetation on these steppes has become strikingly poor, so that the cattle-owners calculate that for each sheep they must have 4·6 acres of grazing-land, 21·6 acres for each head of horned cattle, and 27 to 32 acres for each horse. The black-earth soil, when continually trampled on by the sheep, hardens as well as a clay soi would harden ; the soil is thus no more aërated, and becomes unable to support a rich grass vegetation.

THE *New York Times* announces that Lieut. Schwatka, the Arctic explorer, has accepted a commission from that paper to explore the southern coast of Alaska and to attempt an ascent of Mount St. Elias, the highest peak on the North American continent. Mr. William Libbey, Professor of Geography at Princeton College, has undertaken the charge of the scientific portion of the expedition, which left Port Townsend on the 14th inst.

THE three papers contained in the current number of the *Proceedings* of the Royal Geographical Society are of exceptional value and interest. Mr. James W. Wells describes the physical geography of Brazil in its broad features. He shows that the idea fostered by most maps that Brazil is a very mountainous country is wholly erroneous, and that it is mainly a vast plateau, excavated into numerous valleys by denudations, with relatively few purely mountain chains. As shown by the map accompanying the paper, the four main physical features of the country are (1) the vast, low-lying, flat plains of the Amazons, and the flat, grassy plains of the Paraguay ; (2) the elevated highlands that extend over the greater part of the empire ; (3) the higher lands constituting the watersheds of the principal rivers ; and (4) the groups of mountain ranges consisting of primitive rocks of purely upheaved strata. Mr. Wells then takes the three great hydrographic sections of Brazil, and treats of each in turn. Mr. Hosie describes one of the many journeys which he made through South-Western China while residing as agent at Chungking, the particular journey selected being one which carried him over new ground. A map which is appended shows the vast area covered by Mr. Hosie in his various journeys throughout Sze-chuan, Yunnan, and Kweichow provinces, and the very interesting observations on trade, present and prospective, in these regions show that his commercial duties have not been forgotten in the ardour of exploration. Mr. Bourne writes a paper on Diego Garcia, the principal of the Chagos Islands, which have recently received much attention on account of their position near the Red Sea route to Australia. The writer visited this remote spot to study the fauna and flora, and to make a collection of the corals of this part of the Indian Ocean.

THE SUN AND STARS[1]

VIII.

FOR the purpose of this lecture I have ventured to make a revision of the classification which has been suggested by Dr. Vogel. I should tell you, with reference to this question of classification, that Rutherford started it; then the German physicist, Prof. Zöllner, recommended a certain line of arrangement which practically had been adopted by Father Secchi. I afterwards saw grounds for saying that that line of arrangement, or sequence, was apparently a very just one, because it seemed, from some considerations I brought forward, that it really arranges the stars in the order in which the various phenomena would be produced in the atmosphere of any one of them; that is to say, that it was a true evolutionary line starting from the conditions of highest temperature. Others have followed in the same track since, including Dr. Vogel; but, so far as I can make out, any credit which is due to the existing arrangement is due to Father Secchi and to Prof. Zollner.

I will give you the arrangement, which I think will perhaps bring the facts in the most clear way before you.

We have, then, the first class of stars with broad absorption-lines and very few of them, and a remarkable absence of general absorption at the blue end of the spectrum. Next we have a second class, in which the lines are more numerous, and they are thinner. In this class come our sun, Arcturus, Aldebaran, and Capella.

Then we pass from absorption-lines altogether, and in the third class we have stars with flutings, of which the darkest part and sharpest edge of the fluting lie towards the violet part of the spectrum. Of these stars we have α Herculis and α Orionis as examples.

Then we have another set of fluted stars in which the opposite holds good. The darkest part and sharpest edge of the fluting are to the right, towards the red end of each fluting. And the stars of this class are faint.

In those four classes we nearly exhaust all those forty or fifty millions of stars in the heavens which shine, and which we can study by means of a telescope.

Afterwards we come to stars with bright lines, or the fifth class; and this we must divide into two—A and B.

In sub-class A the bright lines are always lines of hydrogen, such as we have in the chromosphere of the sun. Many of these stars, as we shall see by and by, which are characterised by such a spectrum as this are variable stars; not all.

In sub-class B the lines are not lines of hydrogen, and I may say that up to the present moment the origin of these lines is not known. There are, I think, at the present moment about half a dozen stars known with spectra of this character.

So much, then, for a general view. We have four classes of stars determined by absorption—two, line absorption; two, fluting absorption; first, broad lines; second, thin lines; first, flutings with the sharp dark edge to the left; then, flutings with the sharp dark edge to the right. Then in the last class we leave absorption-lines altogether and get to bright lines, and we get two sub-classes—those which obviously contain incandescent hydrogen, and those which as obviously contain something else.

Just a word or two on each of these two classes.

The stars with dark thick lines can be best shown by this diagram, which I owe to the kindness of Dr. Huggins. You

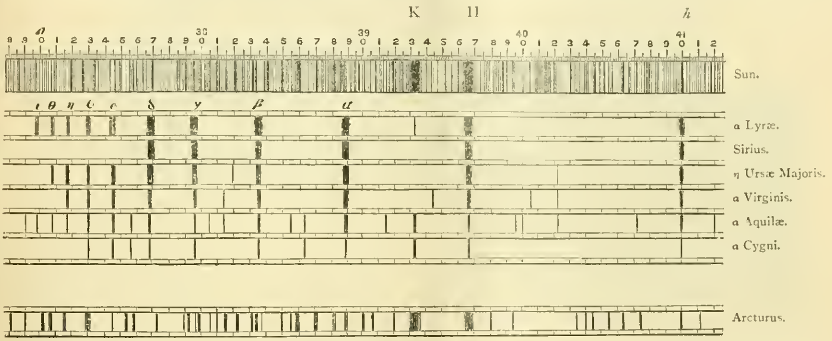


FIG. 22.—Stellar spectra (Huggins). In this diagram the spectrum of the sun is given at the top so that the spectra of the stars can be compared with it. The spectra of the stars are reduced from photographs, and the order of arrangement has been determined by the gradual thinning of the chief lines.

will see the difference between the thick absorption-lines and the thin ones, and you will remember that although stars may have the absorption-lines of identical wave-lengths, the thickness of these lines in the different stars may vary from one star to the other. Then we have the solar spectrum, the thickest lines of which are H and K, already referred to, in the ultra-violet portion.

The remarkable thing about the stars of the first class is that in some of them H is seen alone. In others H is seen with K thin, and in others H is seen with K almost as thick as itself. These lines are supposed to be due to the absorption of calcium. When calcium was studied in the laboratory a good many years ago, it was found that, at the temperature of the electric arc, the important brilliant line of calcium—the line which outshone all the others—was in the blue part of the sprectrum, and that the two lines which are most important, the two broadest lines, in the solar spectrum, are hardly seen at all in the spectrum unless the temperature of a very powerful induction coil be employed. Under these circumstances one may get the same relative importance given to the lines H and K in the violet which one gets with regard to the line in the blue as seen ordinarily, but only when the most tremendous means available to us are taken to secure what we consider to be the highest temperature.

On that ground it was prophesied that if the spectra of stars were ever photographed, probably some might be found hot enough to deal with those bright lines, H and K, in exactly the same way that the electric spark did; that is to say, that as in our laboratories we can get at a high temperature H and K obviously more brilliant than the blue line, whereas at low temperatures H and K are not seen at all, so we may anticipate similar results in the stars; if we can get stars very much hotter than any electric spark which we can obtain here, we might get H and K in different proportions, or each seen alone.

Now you see that prophecy has been fulfilled in this respect—that there are stars in which we get H alone without K, and we get different proportions of K added, as you can get different proportions of milk and sugar in a cup of tea.

Nor is that all. I am bound to tell you one other very curious fact. Since it was obvious when these stars were photographed that we were really photographing the result of an increased temperature; another prophecy was hazarded, and that was, that when, during an eclipse, the very brightest portion of the sun's atmosphere should be photographed in the ultra-violet and violet the spectrum would probably be very closely represented by the spectrum of these hottest stars. These photographs by

[1] A Course of Lectures to Working Men delivered by J. Norman Lockyer, F.R.S., at the Museum of Practical Geology. Revised from shorthand notes. Continued from p. 207.

Dr. Huggins of course only include the violet and the ultra-violet.

This is the place to tell you that in the eclipse of 1882 most of these lines which you see in the spectrum of Sirius and *α* Lyræ—lines which are entirely cloaked in the ordinary spectrum of the one, and in most of the other stars—was actually photographed in the hottest part of the sun's atmosphere during that eclipse; so that you see that there were two prophecies with regard to this set of lines, both of which were fulfilled.

Now, when in science a working hypothesis suggests a certain result under certain conditions—enables us to prophecy in fact, and the prophecy comes true (that is essential)—we have a right to believe that the hypothesis may be well founded.

Those, therefore, who hold that these differences are due to temperature, have considered their opinions to be considerably fortified by those two fulfilments of prophecy to which I have referred.

You see in the diagram that although in the upper spectra representing Sirius and *α* Lyræ the lines are very thick, as those particular lines thin out other lines come in; so that in passing down the diagram from the upper horizon to the lower one, we get two conditions of things—one which we leave when we get few lines thick, and another which we reach where we get a very considerable number of lines thin, as many people believe because these substances gradually, by reduction of temperature in the atmosphere of the star, have gone into combination with themselves or something else, and formed other more complex bodies, which give us of course new lines practically at the expense of the old ones. There then we get on that diagram representing a part of Dr. Huggins's magnificent work, the possible explanation of the passage from Class I. to Class II.

A reference to the two classes of absorption-spectra we owe chiefly to the work of Dunér and Vogel may very fitly follow these records of Dr. Huggins's work.

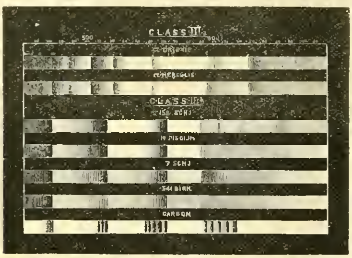


FIG 23.—Dunér's spectra of Class III. compared with carbon.

I did not tell you (there was no necessity for it at the time) that one of the rarest substances apparently in the atmosphere of the sun at the present time is carbon. There is a possible trace of carbon in the sun's atmosphere I think, but the assertion depends upon the existence of a single fluting, so far as observations go, in the ultra-violet part of the spectrum.

Now in these two classes of stars, Class III*a*. and Class III*b*. of Vogel, which I have called Class III. and Class IV. to make things easier, in the former we get a spectrum which at present has not been investigated; and we cannot say to what substances the absorptions in stars (there are hundreds of them mind you) which give you that spectrum is due.

But with regard to the stars of the latter class (and there are hundreds of them) they give us absorptions about which there is no question whatever. There the light of the star, instead of being absorbed by iron vapour, by hydrogen vapour, by calcium vapour, and by nickel, and by cobalt vapours and the like, as in the atmosphere of our sun, is absorbed by carbon vapour, and carbon vapour almost pure and simple, for when you have taken these big flutings out of the spectrum, there is little left—which means that when you get rid of the absorption of carbon in the atmosphere of those stars there is very little absorption left. I mean that the remaining spectrum is very simple.

I have already pointed out that it is fair to say that if our sun were hotter its absorption would more resemble the spectrum of its hottest portion, and there we have the spectra such as Dr. Huggins photographed for us in Class I. and Class II. I have just told you that there we really do get the same lines as in our sun during an eclipse, when we can best get at the spectrum of the hottest portion.

Are we to suppose then that if our sun was very much cooler than it is we should get the spectrum of carbon developed in this enormous way? That is a question which at present it is not possible to answer. It is quite probable, but then if that be so, you will see two things: first, that the carbon, *if it exists as such* in such enormous quantities in the sun at the present moment, must be so far outside the region of high temperature that it cannot absorb in that manner. The second point is that it cannot be that particular substance which gives us the continuous spectrum in the photosphere, because if it were we should be certain, I think, to get more indications of bright carbon vapour, both in the spectrum of sunspots and in the spectra of prominences than we do, so that although these stellar spectra may set us thinking about the sun, they are rather more important to us at the present moment for telling us what possibly is not rather than for what probably is.

With regard to the stars with bright lines, the only point that I need make about them now is that it is most important that every endeavour should be made to determine the origin of those bright lines which, as I have already pointed out to you, are not coincident with the lines of hydrogen. But I would rather say what I have to say on that subject in connection with the next part of the comparison which we are making.

In the spectra which have already been indicated to you nothing has been said about change of star-light except at long periods. It has been hinted that possibly a star which at one stage gives you a spectrum of very thick lines, may at other stages undergo changes which will make it a star of the second class, in which we have a greater number of thinner lines and so on.

Here the question of stellar evolution is suggested. On this subject I cannot enter, but a few general remarks may be made. We may say that we now know that comets are clouds of stones, and experiments, to which I will refer again later on, have been made which suggest that if nebulæ are of like nature the differences between cometary and nebular spectra may be explained by differences of temperature, that of the nebula being higher than that of the comet. Now comets ordinarily, *i.e.* when coolest, give us the spectrum of carbon, but when the temperature is increased, as it was in the case of the comet of 1882, sodium and iron are added. Imagine a comet with a nucleus the light of which is absorbed by ordinary cometary vapours, and we shall have the spectrum of a star of the fourth class.

On the nebular hypothesis, supposing, as seen above, that we started with ordinary cometary materials, then, on the beginning of a central condensation which in time is to become a star, as Kant and Laplace suggested, such central condensation should then give us a star of the fourth class. As the energy of condensation increased and the temperature got higher, the spectra would change through the third and second classes, till ultimately, when the temperature was highest, the first class spectrum would be reached. On the slackening down of the temperature of the now formed star, the spectra of the second, third, and fourth classes would then be reproduced, but, of course, now in the direct order.

Nothing so far has been said about changes which instead of taking millions and perhaps billions of years can be undergone in a few days, or weeks, or months.

Careful observations in the heavens have shown a great many years ago that a large number of stars are subject to a considerable change in their brilliancy. The most important work which has been recently done in what we may call the philosophy of variable stars we owe to the diligence of Prof. Pickering of Harvard Observatory in the United States. He has proposed a classification of variable stars, and for the purpose of this lecture I have just to make a slight alteration in his classification, as I did in the other one. First I will tell you broadly what the classification is, and then I will describe as briefly as may be some of the more important details which are of the greatest moment and interest. There is a whole mine of interest here which of course I cannot touch in the time at my disposal.

The first class of these variable stars then may fitly form what are called temporary or new stars—stars which as we have had evidence during the last few years—in 1866 and 1876, and at the end of last year, suddenly burst into visibility in the heavens as if they were new creations, last for a certain time,

and then die away. These stars have bright lines in their spectra.

The second class gives us those bodies which, although form do not appear and disappear with any suddenness comparable to that, yet indicate that there is something very extraordinary going on in them. They also belong to our fifth class of stellar spectra. They have bright lines as well as absorption-lines. These bright lines, however, only last for a short time; but bright lines there are.

Next we get stars not so interesting from the large point of view, in which we get considerable changes in their luminosity extending over very long periods, but their spectrum apparently does not change to any great extent. At least, no change of the spectra of these stars has yet been recorded.

After these, in Class IV. we get small irregular changes, and in fact, Dr. Gould—and there is no greater authority than he—says that every star in the heavens undergoes some slight change in its light at some time or other, but at all events those stars in Class IV. have undergone sufficient change to find themselves recorded among suspected variables, while the change actually has been so irregular that one has really practically not known to what class to assign them; and therefore they have a class of their own.

The next class of variables I will, on Dr. Pickering's authority, define as eclipsed stars: that is to say, in this class the change of light does not come from anything in the star itself, but from something that is happening outside it. What is happening you will see by and by.

Now with regard to our first class—the new stars. The accompanying diagram will give an idea of what has been recorded with regard to them. The information which the diagram affords will also give a pretty fair comparison between these variables and the other classes.

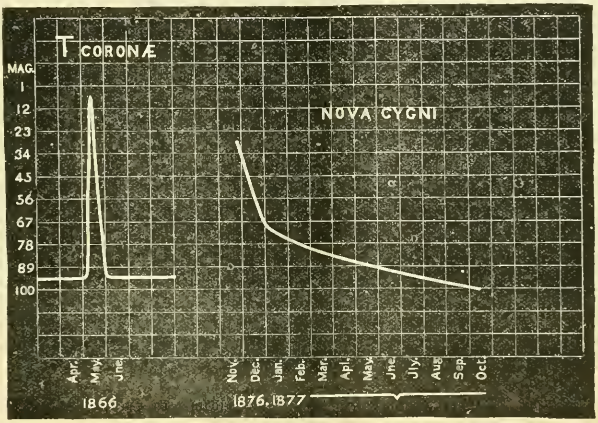


FIG. 24.—Light curves of T Coronæ and Nova Cygni.

In the year 1866 there was a star which had been chronicled for many years as a star between the ninth and tenth magnitudes; for this reason till 1866 its light curve is shown as a straight line. But suddenly, at the beginning of May 1866, this star suddenly burst up into a star of very nearly the first magnitude—between the first and the second. Many observations, as you may imagine, were made on it, and among them Dr. Huggins turned the spectroscope to it, and it was found that the difference between the star when it was between the first and second magnitude, and when it was between the ninth and tenth magnitude, was that in its spectrum when it was most brightly shining we got the spectrum of incandescent hydrogen. We had, in fact, the spectrum of the chromosphere of the sun. It was called "a world on fire." But you know that even the sun is not a world on fire. If it were, and if it were made of the best Welsh coal, we are told that it would last only a few thousand years. But at all events, whatever happened, there was an immense quantity of hydrogen suddenly rendered incandescent, which radiated its light to us.

Almost as suddenly this star went down again, and by the end of the month it had become a ninth or tenth magnitude star, and went about its ordinary business just as if the incident had never happened to it.

FIG. 25.—Cornu's spectrum of Nova Cygni.

Take the next star in 1876, ten years afterwards. It was called a new star—Nova in Cygnus. The point about this one is that it began suddenly as a star of between the third and the fourth magnitudes. It had had no former history. It had never been mapped. It did not visibly rise to the position of a third or fourth magnitude star from a lower level as the other one had done, but it burst out suddenly. Note the difference in its subsequent history. Its light curve, instead of going suddenly down as the one in Corona did in 1866, goes down gently, and takes nearly a year to get to the tenth magnitude. When it got to the tenth magnitude what happened to it? It gave the spectrum of a nebula. It had ceased to be a star. An interesting point is to inquire—unfortunately we shall never now know—whether or not that mass of matter did not exist as a nebula before 1876.

I have stated that, following close upon the publication of Dr. Vogel's paper on the new star, another paper announced the fact

that the new star had put on the appearance presented ordinarily by the so-called planetary nebulæ.

Of all the lines chronicled by Cornu and Vogel during its stellar stage, only one remained, that, namely, which the latter observer showed to be constantly increasing in brightness while all the rest were waning, and which, moreover, was coincident in position in the spectrum with that observed in the majority of the nebulæ.

The observations of such rare phenomena as the so-called new stars are of such vast importance, and will no doubt ultimately provide us with a clue to so many others of a different order, that we may well congratulate ourselves that this Nova was so well watched, and that there is such perfect completeness and unity in the chain of recorded facts.

It should have been perfectly clear to those who thought about such matters that the word star in such a case is a misnomer

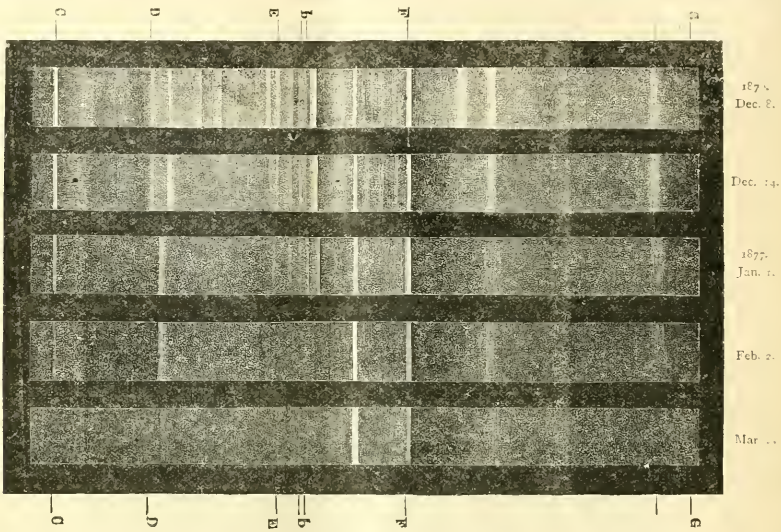


FIG. 26.—Vogel's spectrum of Nova Cygni.

from a scientific point of view, although no word would be better to describe it in its popular aspect. The word is a misnomer for this reason. If any star, properly so called, were to become "a world on fire," were to "burst into flames," or in less poetical language, were to be driven either into a condition of incandescence absolutely, or to have its incandescence increased, there can be little doubt that thousands or millions of years would be necessary for the reduction of its light to the original intensity.

Mr. Croll has shown that if the incandescence observed came for instance from the collision of two stars, each of them half the mass of the sun, moving directly towards each other with a velocity of 476 miles per second, light and heat would be produced which would cover the present rate of the sun's radiation for a period of 50,000,000 years.

A very different state of affairs this from that which must have taken place in any of the Novas from the time of Tycho to our own, and the more extreme the difference the less can we be having to deal with anything like a star properly so called.

The very rapid reduction of light in the case of the new star in Cygnus was so striking that I at once wrote to Mr. Hind to ask if any change of place was observable, because it seemed obvious that if the body which thus put on so suddenly the chromospheric spectrum were single, *it might only weigh a few tons or even hundredweights*, and being so small might be very near us. No motion, however, was perceptible, and Dr. Ball has since stated that he could detect no parallax.

We seem driven, then, from the idea that these phenomena are produced by the incandescence of large masses of matter, because if they were so produced, the running down of brilliancy would be exceedingly slow. J. NORMAN LOCKYER

(*To be continued.*)

FLAME CONTACT, A NEW DEPARTURE IN WATER HEATING[1]

IT is my intention to prove to you on theoretical grounds, and also by experimental demonstration, in such a manner as will admit of no possible doubt, that the present accepted system of water heating, by gaseous or other fuel, is a very imperfect means for an end, and is, both in theory and practice, essentially faulty. My statements may appear bold, but I come prepared to prove them in a manner which I think none of you will question, as the matter admits of the simplest demonstration. I will, in the first place, boil a specified quantity of water in a flat-bottomed vessel of copper; the time required to boil this you will be able to take for yourselves, as the result will be visible by the discharge of a strong jet of steam from the boiler. I will then take another copper boiler of the same form, but with only one-half the surface to give up its heat to the water, and will in this vessel boil the same quantity of water with the same burner in a little over one-half the time, thus about doubling the efficiency of the burner, and increasing the effective duty of the heating surface fourfold, by getting almost double the work from one-half the surface.

The subject is a comparatively new one, and my experiments are far from complete on all points, but they are sufficiently so to prove my case fully. As no doubt you are all aware, it is not possible to obtain flame contact with any cold, or comparatively cold, surface. This is readily proved by placing a vessel of water with a perfectly flat bottom over an atmospheric gas-burner: if the eye is placed on a level with the bottom of the vessel a clear space will be seen between it and the flame. I cannot show this space on a lecture-table to an audience, but I can prove its existence by pasting a paper label on the bottom

[1] A Paper read by Thomas Fletcher, F.C.S., at the Gas Institute Meeting, London, June 9.

of one of the boilers, and exposing this to the direct impact of a powerful burner during the time the water is being boiled, and you will see that it comes out perfectly clean and uncoloured. Now it is well known that paper becomes charred at a temperature of about 400° F., and the fact that my test-paper is not charred proves that it has not been exposed to this temperature, the flame being, in fact, extinguished by the cooling power of the water in the vessel. I need hardly remind you that the speed with which convected or conducted heat is absorbed by any body is in direct ratio to the difference between its own temperature and that of the source of heat in absolute contact with it; and therefore, as the source of the heat taken up by the vessel is nothing but unburnt gases, at a temperature below 400° F., the rate of absorption cannot, under any circumstances, be great, and the usual practice is to compensate for this inefficiency by an enormous extension of surface in contact with the water, which extension I will prove to you is quite unnecessary. You will see I have here a copper vessel with a number of solid copper rods depending from the lower surface; each rod passes through into the water space and is flattened into a broad head, which gives up its heat rapidly to the water. My theory can be stated in a few words: The lower ends of the rods, not being in close communication with the water, can, and do attain, a temperature sufficiently high to admit of direct flame contact, and as their efficiency, like that of the water surface, depends on the difference between their own temperature and that of the source of heat in absolute contact with them, we must, if my theory is correct, obtain a far greater duty from them. I do not wish you to take anything for granted, and although the surface of the rods, being vertical, can only be calculated for evaporating power at one-half that of a horizontal surface, as is usual in boiler practice, my margin of increased duty is so great that I can afford to ignore this, and to take the whole at what its value would be as horizontal surface, and still obtain a duty 50 per cent. greater from a surface which is the same in area as the flat-bottomed vessel on the fire side, but having only one-third the surface area in contact with the water. I do not, of course, profess to obtain more heat from the fuel than it contains, but simply to utilise that heat to the fullest possible extent by the use of heating surfaces, beyond comparison smaller than what have been considered necessary, and to prove not only that the heating surfaces can be concentrated in a very small area, but also that its efficiency can be greatly increased by preventing close water contact, and so permitting combustion in complete contact with a part of the heating surface. I will now boil 40 ounces of water in this flat-bottomed copper vessel, and, as you will see, sharp boiling begins in 3 minutes 15 seconds from the time the gas is lighted. The small quantity of steam evolved before this time is of no importance, being caused partly by the air driven off from the water and partly from local boiling at the edges of the vessel owing to imperfect circulation. On the bottom of this vessel is pasted a paper label which you will see is untouched by the flame owing to the fact that no flame can exist in contact with a cold surface.

It may be thought that, owing to the rapid conducting power of copper, the paper cannot get hot enough to char. This is quite a mistake, as I will show you by a very curious experiment. I will hold a small plate of copper in the flame for a few seconds, and will then hold it against the paper. You will see that, although the copper must of necessity be at a temperature not exceeding that of the flame, it readily chars the paper. We can, by a modification of this experiment, measure the depth of the flameless space, as the copper, if placed against the paper before it has time to be previously heated, will, if not thicker than 1/40 inch, never become hot enough to discolour the paper, showing that the flame and source of heat must be below the level of a plate of metal this thickness.

In repeating this experiment I must caution you to use flour paste, not gum, which is liable to swell and force the paper past the limit of the flameless space, and also to allow the paste to dry before applying the flame, as the steam formed by the wet paste is liable also to lift the paper away and force it into the flame. I will now take this vessel, which has only one-half the surface in contact with the water, the lower half being covered with copper rods, 3/16 inch diameter, ½-inch centres apart, and 1½ inch long, and you will see that with the same burner as before, under precisely the same conditions, sharp boiling takes place in 1 minute 50 seconds, being only 13 seconds more than half the time required to produce the same result with the same quantity of water as in the previous experiment. Although the water surface in contact with the source of heat is only one-half that of the first vessel, and the burner is the same, we can see the difference not only in the time required to boil the 40 ounces of water, but also in the much greater force and volume of steam evolved when boiling does occur. With reference to the form and proportions of the conducting rods, these can only be obtained by direct experiment in each case for each distinct purpose. The conducting power of a metallic rod is limited, and the higher the temperature of the source of heat, the shorter will the rods need to be, so as to insure the free ends being below a red heat, and so prevent oxidation and wasting. There are also other reasons which limit the proportions of the rods, such as liability to choke with dirt and difficulty of cleaning, and also risk of mechanical injury in such cases as ordinary kettles or pans; all these requirements need to be met by different forms and strengths of rods to insure permanent service, and, as you will see further on, by substituting in some cases a different form and type of heat conductor. To prove my theory as to the greater efficiency of the surface of the rods in contact with the flame as against that in direct contact with the water, I have another smaller vessel which, including the rods, has the same total surface in contact with the flame, but only one-third the water surface as compared with the first experiment. Using again the same quantity of water and the same burner we get sharp boiling in 2 minutes 10 seconds, being an increase of duty of 50 per cent., with the same surface exposed to the flame. The rods in the last experiment form two-thirds of the total heating surface, and if we take, as I think for some careful experiments we may safely do, one-half the length of the rods to be at a temperature which will admit of direct flame contact, we have here the extraordinary result that flame contact with one-third of the heating surface increases the total fuel duty on a limited area 50 per cent. This really means that the area in contact with flame is something like six times as efficient as the other. In laboratory experiments it is necessary not only to get your result, but to prove your result is correct, and the proof of the theory admits of ready demonstration in your own laboratories, although it is unfit for a lecture experiment, at all events in the only form I have tested it. If you will take two ordinary metal ladles for melting lead, cover the lower part of one of these with the projecting rods or studs and leave the other plain, you will find on melting a specified quantity of metal in each that the difference in duty between the two is very small. The slight increase may be fully accounted for by the difference in the available heating surface reducing the amount of waste heat passing away, and this proves that flame-contact, and therefore quick absorption of heat, takes place on plain surfaces as soon as these are above a certain temperature, which, in a metal ladle, very soon occurs. What the temperature is which admits of flame-contact I have, as yet, not been able to test thoroughly, and it will need some consideration how the determination of this is to be correctly made; at the same time it is a question in physics which should be capable of being answered.

Let us now take the other side of the question. If the efficiency of a surface depends on flame contact, there must of course be flame, or at least gases of an extremely high temperature, and we therefore cannot expect this extraordinary increase of efficiency in any part of our boiler except where flame exists, and if these projectors are placed in a boiler, anywhere except in contact with flame, their efficiency must be reduced to that of ordinary heating surface. They are, of course, useful, but only in the same way as ordinary flue surface. When we come to boilers for raising steam, which have to stand high pressures, we come to other difficulties of a very serious nature, which require special provision to overcome them. To put such rods as I have referred to in a boiler-plate necessitates the plate being drilled all over with holes, causing a dangerous source of weakness, as the rods cannot be used as stays; further than this, they would render really efficient examination a matter of extreme difficulty, and would be liable to give rise to frequent and almost incurable leakages; but there is, fortunately, a very simple way to overcome this difficulty. I have found that rods or points, such as I have described, are not necessary, and that the same results can be obtained by webs or angle-ribs rolled in the plates. My experiments in this direction are not complete, and at present they tend to the conclusion that circular webs, which would be of the greatest efficiency in strengthening the flues, are not so efficient for heating as webs running lengthwise with the flue, and in a line with the direction of the flame. This point is one which I am at present engaged in testing with experimental

boilers of the Cornish and Lancashire types, and as, with gas, we have a fuel which renders every assistance to the experimenter, it will not take long to prove the comparative results obtained by the two different forms of web. Those of you who have steam-boilers will, no doubt, know the great liability to cracking at the rivet-holes in those parts where the plates are double. This cracking, so far as my own limited experience goes, being usually, if not always, on the fire side, where the end of the plate is not in direct contact with the water—where it is, in fact, under the conditions of one of the proposed webs —I think we may safely come to the conclusion that this cracking is caused by the great comparative expansion and contraction of the edge of the plate in contact with the fire; and it will probably be found that if the plates are covered with webs the whole of the surface of the plates will be kept at a higher and more uniform temperature, and the tendency to cracks at the rivet-holes will be reduced. This is a question not entirely of theory, but needs to be tested in actual practice.

There is another point of importance in boilers of the locomotive class, and those in which a very high temperature is kept in the fire-box, and this is the necessity of determining by direct experiment the speed with which heat can safely be conducted to the water without causing the evolution of steam to be so rapid as to prevent the water remaining in contact with the plates, and also whether the steam will or will not carry mechanically with it so much water as to make it objectionably wet, and cause priming and loss of work by water being carried into the cylinders. I have observed in the open boilers I use that when sufficient heat is applied to evaporate 1 cubic foot of water per hour from 1 square foot of boiler surface, the bulk of the water in the vessel is about doubled, and that the water holds permanently in suspension a bulk of steam equal to itself. I have, as yet, not had sufficient experience to say anything positively as to the formation or adhesion of scale on such surfaces as I refer to, but the whole of my experimental boilers have up to the present remained bright and clean on the water surface, being distinctly cleaner than the boiler used with ordinary flat surfaces. It is, I believe, generally acknowledged that quick heating and rapid circulation prevents to some extent the formation of hard scale, and this is in perfect accord with the results of my experiments. The experiments which I have shown you I think demonstrate beyond all question that the steaming-power of boilers in limited spaces, such as our sea-going ships, can be greatly increased; and when we consider how valuable space is on board ship, the matter is one worthy of serious study and experiment. It may be well to mention that some applications of this theory are already patented.

I will now show you as a matter of interest in the application of coal gas as a fuel how quickly a small quantity of water can be boiled by a kettle constructed on the principle I have described, and to make the experiment a practical one I will use a heavy and strongly-made copper kettle which weighs 6½ lbs., and will hold when full one gallon. In this kettle I will boil a pint of water, and, as you see, rapid boiling takes place in 50 seconds. The same result could be attained in a light and specially-made kettle in 30 seconds, but the experiment would not be a fair practical one, as the vessel used would not be fit for hard daily service, and I have therefore limited myself to what can be done in actual daily work rather than laboratory results, which, however interesting they may be, would not be a fair example of the apparatus in actual use at present.

THE CRATERS OF MOKUAWEOWEO, ON MAUNA LOA[1]

DURING last year I was engaged for many months in surveying lands on Mauna Hualalai and Mauna Loa, in Hawaii, and in that way had an opportunity of making investigations of craters and lava flows that may be of interest to those studying volcanic phenomena.

It would seem that, as the best histories are those written long after the events which they record, when all the reports of eyewitnesses can be carefully examined, so the best descriptions of volcanic action may be obtained long after eruptions, by carefully investigating the records indelibly inscribed in the rocks.

The ascent of Mauna Loa is so seldom made that a brief account of my excursions may be interesting.

[1] By J. M. Alexander, from the *Hawaiian Commercial Advertiser* of October 1885.

On September 1, 1885, I set out in company with Mr. J. S. Emerson, of the Hawaiian Government Survey, to ascend that mountain from the table land east of Hualalai, along the south side of the lava-flow of 1859, which, as many will remember, was visited by a party from Oahu College. We were provided with mules for riding and pack-donkeys, and accompanied by several natives, including a so called guide, who lost himself and delayed us over a day in searching for him.

Our route led first through a narrow belt of forest, consisting of mamane, ohia, and sandalwood trees; then through a scanty vegetation of ohelos and the beautiful *Cyathodes Tameiameiæ*, and at last beyond the limits of vegetation, without a vestige even of moss or lichen, over a wonderful and awful billowy waste of "pahoehoe" lava, traversed by tracts of "aa" and deep chasms.

At about two-thirds of the distance towards the summit we passed the rugged crater hill from which the outbreak of 1859 had issued, and here our path was strewed with pumice and "Pele's hair" from that eruption. There was an enormous quantity of lava poured forth from the small fissure of this crater, forming a stream from half a mile to two miles wide, and reaching nearly thirty miles to the ocean at Kiholo. Lower down I counted eighteen species of ferns and a dozen kinds of phenogamous plants already growing on this flow. In this vicinity the caverns contained many carcasses of wild goats. In one further south I counted eighty of their skeletons and decaying bodies. They had probably leaped in for shelter, and had been unable to leap out.

When near the summit our guide warned us to descend, because of an approaching storm; but Mr. Emerson and I, anxious to accomplish the object of our journey, set out without him through the driving rain that soon turned into hail and then into snow, marking our route with flags so that we might be able to find our way back. In a short time we reached the brink of the vast crater of Mokuaweoweo, filled with fog and surrounded by frightful precipices. Along this brink were numerous deep fissures filled with ice and water, the beginning of cleavage for avalanches into the crater. Here, and for a quarter of a mile below, we observed many rocks of a different kind from the surface lavas, solid, flinty fragments of the foundation walls, weighing from fifty pounds to a ton, which had formerly fallen down upon the crater floor and had afterwards been hurled out during eruptions. I noticed similar rocks around the summit craters of Hualalai. It would be unsafe to approach the crater at this place during eruptions, when such brickbats were flying.

We returned to our camp about noon, and sent the poor animals, which had stood all night in the icy wind tied to jagged rocks, in the care of the guide down the mountain; and with the help of one native, with much difficulty, carried a tent and supplies to the summit.

At evening the fog lifted and gave us a glimpse of the craters. Immediately below us lay the central crater, surrounded by almost perpendicular walls, with a pahoehoe floor streaked with grey sulphur cracks, from hundreds of which there issued columns of steam, and with a still smoking cone in the south end. Beyond this central crater on the south rose a high plateau, and beyond this plateau still further south we saw an opening into another crater small and deep. In the opposite direction, north of the central crater, appeared another higher crater like an upper plateau, from which a torrent of lava had once poured into the central crater, and north of this again another crater, like a still higher plateau, from which also lava had flowed south.

Thus it was evident, as appeared more clearly by subsequent investigation, that Mokuaweoweo is not simply one crater, but a series of four or five craters, the walls of which have broken down, so that they have flowed into each other.

The crater of Haleakala, on Maui, was probably formed in a similar manner out of several ancient craters which have broken into each other. These vast chasms may well be called calderas, as has been recommended by Captain Dutton. On Hualalai there is a series of craters having the same relative position as those of Mokuaweoweo, and crowded so close together as to be almost broken into one. On the older mountains, like that of West Maui, such congeries of craters have evidently formed the starting-points for deep valleys, which the rain torrents, leaping down their lofty walls, have torn out through concentric layers of lava to the sea. Just before sunset we saw the splendid phenomenon of the "Spectre of the Brocken" (Hookuaka),

our shadows on the mist, encircl . with rainbows, over the black inferno.

We erected a survey signal for determining the location and height of the summit, and also of an important land boundary in the crater, viz. the corner where the four lands of Keauhou, Kahuku, Kapapala, and Kaohe meet, which is at the cone in the central crater. We then descended the mountain, carrying more weight than was agreeable, until we were met by our natives bringing up our mules, for which we had signalled by fires. On the way down a violent thunderstorm was raging below us, while we were above in clear air. On my next trip up this mountain I found a tree on the slope below completely rent to splinters, and parts of it thrown several rods, by the lightning of this storm.

During the next month I ascended the mountain again, this time carrying an excellent engineer's transit. As I had no guide, I marked most of the way up by strips of cloth fastened to rocks to find the way back; and taught by our former experience, I took a donkey-load of fuel, as well as a load of grass for making a spherical survey signal, which served me several nights as a bed. When about half-way up the mountain, one of our pack-donkeys broke into a lava cave, and slid downwards nearly out of sight. It was extricated with great difficulty by a direct upward lift with ropes. I then sent one of my men down the mountain with the donkeys, retaining the other man with me. The first night on the summit was uncomfortable enough for us, with a storm from the north. At midnight we observed with a lighted candle that the roof of the tent was a-sparkle with icicles, and on touching it found it frozen stiff as a bullock's hide. In the morning we found a beautiful sheet of snow an inch thick over the tent and over all the ghastly blackness of the rocks. Every morning of our stay upon the mountain we found the water frozen in our kettles, and hoar-frost on the rocks.

In the clear frosty air I was able with my transit to take the bearings of a dozen survey signals on the slopes and summit of Hualalai.

The new spherical signal which I had erected was afterwards accurately determined by observations from more than twenty stations on Mauna Kea, Hualalai, and in South Kona, and thus a trigonometrical station was at last located on the very summit of Mauna Loa.

On the second day I descended from the west brink of the crater down the track of a high avalanche of rocks upon the second plateau, and again from this plateau by the path of another avalanche into the central crater, stepping cautiously down upon the black floor of the crater, lest it should break under our weight. We found this caution unnecessary, for much of the crater bottom proved to be the most solid kind of pahoehoe.

Here we stood as on the congealed surface of a tossing sea that had dashed its fiery surf thirty feet up on the surrounding walls. We travelled directly south for the cone, the boundary corner, which I was to locate, erecting two flags about 2500 feet apart for the ends of our base-line. In some places, where there appeared to have been violent action, the lava broke under our feet, letting us down into caverns. In some large tracts the pahoehoe was covered with pumice, indicating the violence of the former surging and tossing of the lava, for pumice and other light lavas seem to be the froth and foam of the fiercest eruptions. Just before reaching the cone we came to a deeper basin, twenty or more feet below the rest of the crater bottom and about 400 feet wide, covered with the most friable lava, swollen upwards as though raised by air-bubbles, and this basin extended in a lava flow to the north-east along the side of the crater.

Probably this was the place of the last eruption, and of most of the eruptions of this central crater. We found the cone to be composed of pumice and friable lava still hot and smoking, and very difficult to ascend, but we succeeded in climbing to its top, 140 feet high, and in setting up a flag there for the boundary corner. We then descended between the east and west peaks of this cone over huge rocks and deep chasms.

From the fact that this cone is represented on Mr. J. M. Lydgate's map of 1874, I conclude that it has been of long continuance, probably composed of the cinders of successive eruptions, and that the deep basin to the windward of it, like Halemaumau in Kilauea, has continued many years, and is situated at the great central volcanic throat of the mountain.

I then returned to the second plateau to the north, and thence clambered out to the east of Mokuaweoweo by the extremely interesting route of a former cataract of lava from the summit into the crater, the swift downfall of which had turned its lava almost into pumice, and the black, shining spray of which lay spattered on the surrounding rocks.

Further south I observed the course of two other cataracts, which had poured directly into the central crater. At the summit I found the deep fissure from which the outbreak had come that caused these cataracts, and ascertained that it had also poured an immense stream north upon the first plateau and thence south to the central crater. Crossing from this place to the north over the first plateau I suddenly came to a frightful circular crater in the bed of the plateau, apparently 600 feet deep and 1000 feet wide, with a cone in its centre still smoking. We were obliged to hurry with exhausting speed over rough lava in order to reach our tent before night.

The next day we took the transit to the stations in the crater, and the next we surveyed with it along the western brink to the extreme south end, where we looked down into the south crater, which is about 800 feet deep and 2500 feet wide. The length of the whole chasm, or "caldera," I have ascertained to be about 10,000 feet, the greatest breadth 9000 feet, and the greatest depth 800 feet. The area is three and six-tenths square miles. A map of these craters has been sent to the Government Survey Office.

On the south-west side, near the junction of the central crater with the south plateau, I found that there had been another eruption, from fissures that were still smoking, and that this eruption had poured an immense stream southward towards Kahuku, and had also poured cataracts into the south crater from all sides.

I had everywhere observed that there had been great flows from the summit brink down the mountain, and had wondered at the thought of the vast chasm having filled up and overflowed its brim.

This, however, turned out to be an incorrect view. The flows have not been from the lowest parts of the brim, but from some of the highest, which could not have been the case in an overflow.

The walls of the craters are largely composed of loose, old, weather-beaten rocks, and large tracts of the plateau are composed of old pahoehoe that has not been overflowed for ages, which would not be the case if the craters filled and overflowed.

These outbreaks from fissures around the rim indicate that the lava has rather poured into the crater than out of it; and that it has poured from such fissures in vast streams down the mountain-side. What enormous quantities of lava may flow from such small fissures is illustrated by the flow of 1859. The question arises, How has the lava risen high enough to pour in extensive eruptions through these fissures, almost a thousand feet above the bottom of the crater, without rising in the crater and overflowing it? The same question has often been asked in respect to the rise of liquid lava to the summit of Mauna Loa without overflowing the open crater of Kilauea, 10,000 feet below.

We have seen that it is not because the lava in Mokuaweoweo is lighter than that in Kilauea that it rises so much higher. In fact, it is as solid there as in Kilauea. The explanation has occurred to me that molten lavas rise the higher the smaller the conduits in which they rise from their subterranean reservoirs.

An illustration is afforded by the "spouting horns" on the sea-coast, where the ocean, rushing into caverns of rock, drives columns of water through small openings to the height of forty or fifty feet above high-water mark. We see another illustration in water conveyed in pipes, which jets the higher the smaller the orifice.

However violent the subterranean pressure may be, Kilauea does not overflow, but only rages the more fiercely, because its passage from the chambers below is so large. But through the vast mountain of Mauna Loa there is no doubt a constricted conduit leading upward; and there must be still smaller conduits to the fissures on the summit rim. On this theory, the molten lava rises higher through Mauna Loa than in Kilauea, because Mauna Loa has the smaller throat.

It is therefore by no means certain that there is no subterranean connection between the two volcanoes.

Another vexed question, of which several solutions have been proposed, is the mode of formation of the two strongly contrasted forms of lava known as "pahoehoe" and "aa." The former term is applied to tracts of comparatively smooth and uniform lava, as though it had cooled while flowing quietly; the latter to tracts of broken lava, as though it had cooled when tossing like an ocean in a storm, and had then been broken up by earth-

quakes. As Mr. Brigham states, "No words can convey an idea of its horrible roughness and hardness."

My own belief is that "aa" has been formed simply by obstructions breaking the quiet flow of molten lava. Every observer has noticed that "pahoehoe" contains ducts and air-chambers, having an upper crust contorted into the shape of the waves and ripples of the flowing lava. The liquid lava has evidently flowed in these ducts and chambers, and at last flowing out has left them empty with glazed interior surfaces. In like manner torrents of lava have poured through caverns down the mountains to the sea, and flowing out have left the innumerable caves, smooth and shining within, to be found all over the island. Now, when there are obstructions on the earth's surface or meeting flows, this system of ducts is broken up, and fragments of lava are carried along on the surface, piling up higher than the adjacent "pahoehoe," like ice-packs in rivers, and sometimes rolling immense boulders twenty and thirty feet high, which now stand on the "aa" with the drip glistening over them. This theory is confirmed by the fact that "aa" is always higher than the adjoining "pahoehoe," and also by the fact, which I especially noticed in the flow of 1859, that wherever there are open spaces in lava flows (kipukas), the old lava under the flow is found to be "pahoehoe" under "pahoehoe" and "aa" under "aa."

While surveying the region I was extremely interested in the arrangement of the craters; and now having determined the situation of more than fifty of them on Mauna Loa, Hualalai, and Mauna Kea, I have ascertained that there is a method in their arrangement. They are not arranged relatively to the mountain on which they are situated, but relatively to the points of the compass. There seems to have been a series of nearly parallel fissures through which these craters have risen, in lines running from S. 40° E. to S. 60° E. There are a few arranged in lines running N. 50° E.

It has been remarked by Mr. W. T. Brigham that, while the general trend of the Hawaiian group and of the major axis of each island is N. 60° W., there is no crater on the islands whose major axis is parallel to this line. "On the contrary," he continues, "a very interesting parallelism is observed among all the craters, and invariably the longest diameter is north and south." It would be more correct to say that the major axes of the great craters are generally at right angles to the general axis of the group, *i.e.* about N. 30° E. Haleakala and the ancient Kipahulu caldera appear to take the other direction, but the statement is certainly true of the great calderas of Kilauea and Mokuaweoweo, which have other points of resemblance.

Thus in both the highest walls are on the western side, and in both the action is working towards the south-west, as is indicated by the fact that the north-east craters are nearly filled up, while the deepest and active craters are in the south-west end of the caldera.

It has been shown by Prof. Dana and other geologists that the principal mountain-ranges of the globe, as well as the main coast-lines and chains of islands, take the two directions just mentioned, "which are in general tangential to the Arctic and Antarctic circles." Thus it appears that the laws in accordance with which the volcanic forces are now operating in these islands are the same as those by which all the grand features of our world have been established, and possibly related to the laws of crystallisation which pervade the mineral kingdom; and thus we perceive a unity in the processes of the globe.

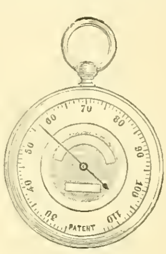
In conclusion, I would remark that to my mind the most plausible theory to account for volcanic action is that of Mallet, that the contraction of the earth's crust continually going on under the power of gravitation causes as much internal heat as would be required to cause a similar expansion. Prof. Dana has remarked that "the fact is well established that motion in the earth's rocks has been a powerful source of heat," and that the annual crushing of not over one-sixth of a cubic mile of rocks in the earth would cause all the volcanic phenomena of the world. This theory has the beauty of attributing all these phenomena to a single cause, and of thus suggesting the thought of the one great Power above the inexplicable forces of gravitation, who continues all the forces of the universe.

IMMISCH'S THERMOMETER

THIS instrument depends for its action upon the opening and closing of a minute volute Bourdon tube, which for this purpose is filled with expansive liquid and hermetically sealed. One end of the tube is fixed, and the free end is brought into contact with the short arm of a lever, the long arm of which forms a rack gearing with a pinion which carries the pointer. The position of the tube with regard to the short lever-arm is such that for ordinary purposes the divisions on the dial are equal, while for clinical use the scale is an increasing one, in order that near blood-heat the divisions become wider to permi of a fraction of a degree being read off accurately.

The success which these instruments meet with is owing principally to their sensitiveness, accuracy, and non-liability to get broken. If they should meet with an accident they can be easily repaired.

The appellation "metallic" does not seem to be a happy one for these thermometers, as they are likely to be confounded with the unsuccessful attempts which have been made to produce instruments for similar purposes by means of bi-metallic laminæ. The defects of the latter are the extremely small *vis viva* avail-



able for the work of multiplying the small motion of the laminæ, and the liability to not return to precisely the same point after being subjected to extremes of temperature. In this latter respect there is a double security with the instrument which is the subject of this notice. The tube is in itself a very flexible spring, the motion of which does not overreach the limits of perfect elasticity, and its position at any given time is determined by the volume of the liquid, which, of course, remains always a constant quantity whatever the volume may be. As the tube is absolutely full, it must of necessity always accommodate itself to the volume and correctly indicate the temperature.

As regards accuracy, we are informed that upwards of 500 have already been tested at Kew—we have ourselves seen the certificates of the last group of two dozen clinical ones, and they give the remarkable results of perfect accuracy at 66 per cent. of the points tested, and of no error greater than $0°{\cdot}2$ at any point on any one of the twenty-four thermometers.

SCIENTIFIC SERIALS

Rendiconti del Reale Istituto Lombardo, May 13.—On the theory of waves, by Prof. E. Beltrami. The author presents some considerations which place in a clearer light the process by which F. Neumann deduces the laws of Fresnel from the fundamental equations of elasticity.—Dynamics of moving systems which preserve their mutual affinity, by Prof. C. Formenti.

Rivista Scientifico-Industriale, May 31.—Maximum and relative humidity of the atmosphere, by Prof. Paolo Cantoni. Hygrometric tables of mean annual moisture, recorded at thirty meteorological stations in various parts of Italy, show that the average of maximum and relative humidity increases from north to south, from elevated to low-lying stations, and from inland to maritime districts.—On the persistence of the mathematical figure of the earth throughout the geological epochs, and on the constitution of the terrestrial crust, by Prof. Annibale Riccò. A summary is given of M. H. Faye's views on this subject already published in the *Comptes rendus* of the French Academy (March 22 and April 5, 1886), the author concluding that the mathematical figure of the globe, as represented by the surface of oceans, has not been perceptibly modified by the geological forces associated with the cooling process.—On the permanent magnetism of steel at various temperatures, by Prof. Poloni. It is shown that at the temperature of 180° C. the well-known law of magnetic distribution in steel bars no longer holds good when

the bars have been magnetised without being first subjected to great variations of temperature and kept free from telluric action by being held in a vertical position.

SOCIETIES AND ACADEMIES

LONDON

Royal Microscopical Society, June 9.—Rev. Dr. Dallinger, F.R.S., President, in the chair.—Mr. G. F. Dowdeswell described a preparation of the microbe of rabies in the spinal cord of a rabid dog, which he exhibited × 400. — Prof. F. Jeffrey Bell exhibited a specimen (received from Prof. McIntosh) of a very young starfish, in a stage so early as to show clearly the knob-like portions of the larval organ. Prof. McIntosh has been giving some of his knowledge and skill to fishing observations, which had been rendered possible by the facilities afforded by an enlightened Fishery Board in Scotland.—Mr. F. R. Cheshire exhibited a device for the better examination of Bacteria in culture tubes, the cylindrical form of the tube so distorting the appearance of the contents that it was almost impossible to make any observations upon them under the microscope. The first plan adopted was that of placing the tube in a trough of water and then looking at it through the front of the trough. This was found to diminish the aberration very much, but it did not get rid of it altogether, and was, therefore, only available under very low powers. Water having a refractive index of about 1·333 and alcohol of about 1·374, by adding water to alcohol a mixture having a refractive index of anything between the two could be obtained according to the proportions used. Gelatine has a refractive index rather higher than that of water, and the interposition of a cylinder of glass added something to this. The trough which he employed had a front of rather thin glass, the bottom being sloped in such a way as to cause a tube placed in the trough to lie always near to the front. The tube to be examined was placed in the trough with some water, and then alcohol was added until the proper density was arrived at, and by this means it was quite possible to use a $\frac{1}{8}$-inch objective effectively.—Prof. Bell, at the request of the President, gave an account of what he regarded as the most extraordinary biological fact brought to light during the last twenty-five years—that of a third eye at the top of the head of certain lizards.—Mr. Crisp called attention to a new lamp for the microscope which had been sent for exhibition by Mr. Curtis, and which was so cheap and simple that it seemed likely to become the lamp of the future. It was founded on the lamp originally devised by Mr. Nelson.—Mr. A. Brachet's communication suggesting the use of a hyperbolic lens for the field-lens of the eye-piece was read. Mr. Brachet claimed that thereby the diaphragms in the eye-piece and objective could be dispensed with, and the image much improved.—Dr. Crookshank read a paper on photo-micrography, which was illustrated by the exhibition of a large number of prints, negatives, &c. Mr. Glaisher, President of the Photographic Society, said he had examined Dr. Crookshank's exhibits, and thought they were certainly very beautiful productions. He had for many years taken a great interest in the subject of photography, and had looked to it with hopes which had been more nearly fulfilled than ever before by the specimens before them. He had heard the paper with great pleasure, and could only express his admiration of it, believing as he did that it held out great promise for the future.—Mr. F. Enock exhibited sketches of some of his slides, the various parts being numbered and named and accompanied by a short explanation. It is intended to issue sketches of all the mouth organs of British bees and other interesting insects.

Mineralogical Society, June 22.—Mr. L. Fletcher, President, in the chair.—Mr. Andrew Taylor was elected a Member.—The following papers were read:—C. O. Trechmaine, Ph.D., on barytes from Addiewell, West Calder, N.B.—Prof. E. Kinch, on plattnerite.—F. H. Butler, M.A., on dufrénite.—R. H. Solly, on anglesite from Portugal; and on apatite from Cornwall.—Mr. R. Simpson (visitor) exhibited a very large rolled crystal topaz from Tasmania.—Several interesting specimens were re-exhibited by the President, Mr. Rudler, and others.

PARIS

Academy of Sciences, June 28.—M. Jurien de la Gravière, President, in the chair.—On the theory of minima surfaces, by M. G. Darboux. The results hitherto arrived at in the study of minima surfaces lead naturally to the inquiry here instituted regarding the determination of all minima algebraic surfaces contained in a given algebraic curve, or, more generally, to determine all the minima algebraic surfaces inscribed in a given algebraic curve.—On the subject of certain circumstances presented by the movement of the hydro-extractor, by M. de Jonquières. The author deals with the normal case (omitted by Poinsot), in which the movement of precession is complicated and rendered irregular by movements of nutation.—On a process by means of which the oscillations of an absolutely free pendulum may be mechanically counted, by M. M. Deprez. The principle is described of an apparatus not yet constructed, which is intended to record the number of vibrations without exercising any mechanical influence on the pendulum. Without this condition the results would be worthless, as the vibrations, instead of being effected under the influence of gravitation alone, would be affected by the action of a force of unknown magnitude. The problem is solved by the aid of optics, light being the only agent which exercises no mechanical action on the bodies exposed to its influence.—On the persistence of voluntary movements in bony fishes after removal of the cerebral lobes, by M. Vulpian. The author's experiments with carp fully confirm Steiner's recent conclusions regarding the persistence of the voluntary movements in fishes thus operated upon. They also show that the faculty of sight is unaffected by the operation, as already proved by the author in 1864.—On the normal metronome, by M. Saint-Saens. Owing to the defective character of this instrument, it is found to be of little practical service to musicians. Hence the Academy is urged to supply a normal metronome mathematically regulated which, before being issued to the public, should be tested and stamped like all diapasons, weights, and measures. The matter was referred by the President to the Section for Mechanics and Physics.—On the extension of a theorem of Clebsch relating to curves of the fourth degree, by Prof. Sylvester.—A fresh series of experiments on the automatic action of the regulating apparatus constructed at the Aubois sluice, by M. A. de Caligny.—On the fluorescence formerly attributed to yttria, by M. Lecoq de Boisbaudran. By recognising the complex character of yttria and announcing the existence of new elements characterised by fluorescent bands at first attributed to yttria (NATURE, June 17, pp. 160–62), the author considers that Mr. Crookes has implicitly adopted the opinion always held by him regarding the true character of these bands. But from the fresh experiments here described it is pointed out that further interesting studies will have to be made in order thoroughly to elucidate the subject.—Remarks accompanying the presentation of a work entitled "Cosmogonic Hypotheses: an Inquiry into the Modern Scientific Theories on the Origin of Worlds, with a Translation of Kant's 'Theory of the Heavens,'" by M. Wolf. In writing this work the author's object has been to show that the theory of Laplace, completed by the labours of M. Roche and other *savants*, still answers best to the conditions required of a cosmogonic hypothesis. He claims to have met all the objections urged against it, and especially that of M. Faye regarding the pretended necessity of a retrograde rotation of the planets.—Report on M. Poincaré's memoir entitled "Influence of the Moon and Sun on the Northern Trade-Winds," by the Commissioners, MM. d'Abbadie and Mascart. With certain reservations this memoir is recommended to the favourable consideration of the Academy. It shows that there is some truth in the popular opinion respecting the influence of the moon on the weather, but that this influence should be referred not to the new, but to the waning phases of the moon, while account should also be taken of the antagonistic influence of the sun.—Action of an electric current on anhydrous hydrofluoric acid, by M. H. Moissan.—On the flow of gases in the case of a permanent *régime*, by M. Hugoniot. It is shown that M. Hirn's experiments in no way contradict either the kinetic theory or the laws of hydrodynamics, and, so far from refuting, actually confirm the well-known formula of Weisbach or Zeuner.—On the condensation of vapours, by M. P. Duhem.—On the coefficient of self-induction in the Gramme machine (three illustrations), by M. Ledeboer.—On the spectra of didymium and samarium, by M. Eug. Demarçay. Some fresh results are described, which the author has obtained from the study of the photographed absorption-spectra of various products of the fractionation of didymium and samarium.—On a new double iodide of copper and ammonia, by M. A. Saglier. The process is explained by which the author has obtained this

new compound, whose formula is $2NH_3Cu_2I_2$, as shown by the following figures :—

	Found			Theory
Copper	24·66	24·61	...	24·84
Iodine	66·03	65·91	...	66·27
Ammonia	8·58	8·66	...	8·88

—On the synthesis of an inactive terpilenol, by MM. G. Bouchardat and J. Lafont.—Action of anhydrous baryta on methylic alcohol, by M. de Forcrand. From the author's experiments it follows that whenever the solution of baryta takes place in methylic alcohol in the presence of a trace of water, which it is very difficult to avoid, the resulting compound should be $C_2H_4O_2,BaO,H_2O_2$.—Action of heat on the acetones, by MM. P. Barbier and L. Roux. The paper deals fully with the mode of decomposition which these substances undergo when subjected to the influence of red heat.—Decomposition of pilocarpine, by MM. E. Hardy and G. Calmels.—Researches on the development of beetroot, by M. Aimé Girard. Here the author studies more especially the tap-root and radicules, concluding that the saccharine matter is formed, not in the underground, but exclusively in the overground parts of the plant.—On the functions of the ovoid gland, of Tiedemann's bodies, and Poli's vesicles in the Asteridæ, by M. Cuénot.—On the conjunctions of the ciliated Infusoria (*Colpidium colpoda*, *Paramecium aurelia*, and *Euplo'es patella*), by M. E. Maupas.—On the classification of the Thaliaceæ and some other groups of Ascidians, by M. F. Lahille.—Note on the *Amphistegina* of Porto Grande, St. Vincent Island, by M. de Folin.—On the functions of the cephalic fossettes in the Nemertæ, by M. Remy Saint-Loup.—Researches relative to the influence of the nerves on the production of lymph, by M. Serge Lewachew.—On the anatomic constitution of the Ascidians attached to the rare American plant *Heliamphora nutans*, Benth., By M. Ed. Heckel.—On the presence of a line of erratic boulders stranded on the coast of Normandy, by M. Ch. Vélain.—On the eruption of Etna during the months of May and June, by M. H. Silvestri. The discharge during twenty days of activity has been approximately estimated at 66,000,000 cubic metres.

BERLIN

Physiological Society, May 28.—Dr. Virchow made a report of his investigations into the capillaries of the vitreous body and their environment. The vitreous body, which must no longer be regarded as a tissue, but as an organ, showed different structural relations among the different groups of animals, and, in the case of fishes and the frog, was distinguished by its strong bounding cuticle, on which the capillaries formed an object of interesting examination. In regard to the structure of the capillaries the speaker had come to the conviction that they consisted of a fundamental membrane which was occupied with cells. The environment of the capillaries formed lymph-spaces, which had not yet, however, manifested themselves as standing in continuous connection with one another. On the cuticles inclosing the lymph-spaces lay cells displaying a great multiplicity in form and arrangement among the different kinds that had been examined.—Prof. Munk attacked the position taken up at the last sitting of the Society by Prof. Christiani respecting the possibility of seeing after excision of the greater brain. He challenged his opponent to show to the Society or the Association of Naturalists for this year a rabbit that was able to see after the removal of the greater brain.—Dr. Benda exhibited a series of preparations of the central nervous system which were coloured in accordance with the hæmatoxyline method as modified by him. There were in particular three advantages distinguishing his hæmatoxyline colouring from that of Weigert's: (1) the axial cylinders of the nerve fibres in the brain came out more distinctly, and their connection with the ganglia cells was directly demonstrated. (2) The structure of the ganglia came out more distinctly. In the case of those ganglia which remained clear after the hæmatoxyline colouring, there appeared with great constancy in the fibrous framework, dark concretions, which might perhaps be interpreted as a special structure, though the speaker was not yet prepared to decisively maintain that assumption as fact. (3) With still more reservation would he present the third result, which came to light in a particular structure of the medullary sheath. On the transverse section radiate drawings were seen to proceed from the axis cylinder towards the neurilemma. These markings ramified, and perhaps formed the protoplasmatic scaffold within which was deposited the fluid nerve-medulla. The speaker next described more minutely his method of proceeding—hardening with picrin acid, washing out with alcohol, laying in paraffin, treating with a sulphate of iron, colouring with hæmatoxyline, washing out with solution of alum or with a diluted acid. In conclusion Dr. Benda gave a theory of hæmatoxyline colouring, which ranged itself close in order with the colouring with logwood customary in technics. In both cases the colouring-matter was applied as lac, the tissue being first saturated with the mordant, and then impregnated with the colouring-matter, which formed in the tissue lacs insoluble in water and alcohol, and only in part capable of being resolved through washing out with the mordants or with acids. Or the colouring-matter might be used in the way of ink, which formed precipitates with the tissues.

BOOKS AND PAMPHLETS RECEIVED

"Annual Report of the Smithsonian Institution for the Year 1884" (Washington).—"The Gothic of Ulfilas," by T. le M. Dowse (Taylor and Francis).—"Recherches pour établir ses Rapports avec la Côte de France," by Prince A. de Monaco (Gauthier-Villars, Paris).—"Monthly and Yearly Means, Extremes and Sums for the Years 1883, 1884, 1885" (Tokio).—"Aus dem Archiv der Deutschen Seewarte," 7 Jahrgang, 1884 (Hamburg).—"Reichenbachia, Orchids Illustrated and Described," part 1, May, by F. Sander (Sander and Co, St. Albans).—"Encyklopædie der Naturwissenschaften," Erste Abth. 45, 46. und 47, Lief.; Zweite Abth. 34, 35, und 36, Lief. (Trewendt, Breslau).—"Mémoires du Comité Géologique, St. Petersburg," vol. ii. No. 3.—"Bulletins du Comité Géologique, St. Petersburg," v. Nos. 1 to 6.—"Bibliothèque Géologique de la Russie," i., 1885.—"Physiological Laboratory Notes," by S. W. Holman (Cushing, Boston).—"Annotated Catalogue of the Published Writings of Chas. A. White," by J. B. Marcow (Washington).

CONTENTS

THURSDAY, JULY 15, 1886

GEOLOGY OF TURKESTAN[1]

II.

Turkestan: a Geological and Orographical Description based upon Data collected during the Journeys of 1874 *to* 1880. By J. V. Moushketoff. Pp. 714. With Map and Engravings. Russian. (St. Petersburg, 1886.)

THE view taken by M. Moushketoff and other modern explorers of the region, as to the Aral-Caspian basin having consisted of several large lakes, or rather seas, connected together by outlets, is, in our opinion, the only one which can adequately explain the ulterior changes undergone by the basin during historical time; and it is also fully in accordance with the orographical configuration of the region. These outlets have gradually dried up, and it is probable that the Aibughir and the Balkhan outlets both existed during the historical period.

The basin of the Aral and Sary-kamysh was long maintained by the inflow of water received by the Amu and the Sir. There may have been a period when both joined together before entering the great lake; but later on, the Amu entered the Sary-kamysh, or western part of the double lake; while the Sir flowed into its eastern or Aral part. As the Amu, undermining the Sultan-uiz-dagh hills, gradually moved further east, and finally, finding its way through this range, began to flow into the Aral, the Sary-kamysh lakes, deprived of its water, dried up much more rapidly. The Aral basin, in the meantime, may have increased in size.

As to the Uzboy, which was considered by the earlier explorers as a former bed of the Amu-daria, M. Moushketoff, in accordance with the majority of the more recent explorers, considers it a marine outlet which connected both the great lakes; and the absolute want of any river-deposits and the wide extension of Caspian shells up the Uzboy serve to confirm this view. The drying up of the Aral goes on now very rapidly. The disappearance of the Aibughir gulf; the conversion of the Sary-cheganak gulf into a mere lake; as also that of the Kamyshly-bash, which the Kirghizes remember to have been connected with Lake Aral; together with the numerous facts mentioned by MM. Severtzoff, Borschoff, Meyendorf, Maksheeff, and Schultz, are well known. The observations of MM. Kaulbars and Dorandt furnish most valuable data of the same kind for other lakes of the region: the Sary-kamysh, now 50 feet below the level of the Caspian, covered a surface of no less than 4400 square miles. And we may add that these facts are not isolated ones, but that the same rapid drying up is going on throughout Western Asia: it is the characteristic of the geological epoch in which we now live.

Many most interesting pages are devoted by M. Moushketoff to wind-agencies and to moving sands. His observations on dunes and analogous sand-hills, accompanied by several drawings, will assuredly be most welcome to geologists. M. Moushketoff distinguishes between two different kinds of sand-hills: the dunes, arising on the shores of lakes, and the *barkhans*. Wind is a powerful agency in the formation of both. Recent meteorological observations have shown that north and north-east winds are much more prevalent than any others in the Turan region. On the lower Amu-daria and at Tashkend they are from 50 to 60 per cent. As to the rains they are so scanty that throughout the year their aggregate amount hardly reaches 69 millimetres at Petro-Alexandrovsk, and 73 at Nukus. The evaporation, as appears from M. Stelling's work, is exceedingly great. Thus, while at Kishineff, for instance, the annual evaporation exceeds the annual amount of rain by only one-fifth, it is five times greater than the amount of rain at Astrakhan, three times greater at Tashkend, twenty-seven times at Nukus, and thirty-six times at Petro-Alexandrovsk. The yearly amount of rain being represented by a column 69 millimetres high at Petro-Alexandrovsk, the evaporation is so great that a column of water 2320 millimetres high would be evaporated every year; at Nukus the respective figures are 71 and 1928 millimetres. These climatic conditions would suffice, in M. Moushketoff's opinion, to explain the geographical distribution of the moving sands which appear more especially to the south of Lake Aral.

Now, among these moving sands two different kinds of moving hills should be distinguished; the *dunes* and the *barkhans*. The former are indebted for their origin to the combined action of water and wind; they are disposed in long waves along the shores of the lakes or rivers—these last (the river-dunes) being local and never reaching more than 10 or 15 feet in height. The marine dunes, attaining as much as 50 feet—not more—have lengths reaching to about 700 yards. They arose in consequence of the retiring of the Aral Sea and the prevailing winds. These dunes have the most varied directions, according to the local direction of the former shore-line.

As to the *barkhans* they are indebted for their origin to the agency of the wind alone. They can arise only under certain climatic conditions, and may appear covering any geological formation, like the moving sands o the Ili River, which are due to the destruction of massive crystalline rocks, or the sands of the Sahara, which are a result of the disintegration of basaltic rocks. Their outer shape is quite characteristic, being always that of a crescent, or, to use Middendorff's comparison, it resembles the hoof of a horse. Sometimes two, three, or four *barkhans* are connected together, and then they appear like a succession of crescent-shaped conical hills, connected by their respective horns. Their height is usually from 30 to 40 feet; there are, however, much smaller ones, and a few reach as much as 80 and even 100 feet. The sand of which they consist varies according to the nature of the rocks to whose disintegration they are due; the angles of inclination of their slopes also vary between 30° and 40° on the side turned to the wind, and between 6° and 16° on the opposite side.

On the whole, on seeing these hills, one would refuse, according to M. Moushketoff, to recognise in them a formation due to the sole agency of the wind, but one must witness a storm in the desert to recognise its full force. Still, during a very strong storm, M. Moushketoff did not see the wind moving particles of sand more than 1 to 2 millimetres in diameter.

[1] Continued from p. 119.

We should very much like to go further into an analysis of the interesting observations on the loess, scattered through M. Moushketoff's "Turkestan"; but can only mention that the loess which is widely spread over the region, both on the outskirts of the Tian-Shan and in the neighbouring lowlands, is always accompanied by what the author describes as a "conglomerate," and which is most probably some kind of more or less modified glacial deposit. Both are inseparable, and the loess invariably covers the "conglomerate" when they are met together Of course, the loess extends further in the lowlands, and the "conglomerate" in the hilly tracts. Sometimes there are layers of loess amidst the "conglomerate." As to the loess itself, although mostly quite typical, it sometimes appears stratified to a certain extent; but it does not differ at all from the unstratified loess. M. Moushketoff accepts Richthofen's theory as to the eolic origin of loess; but he does not deny that water spreading over a wide surface at the issue of small depressions of the ground, gives the same typical loess as that which may be considered eolic in its origin.

We ought to notice also a special question discussed at length by M. Moushketoff, namely, his thorough researches, made in company with Prof. Beck, on the nephrite (jade) of which the stone on the grave of Timur at Samarkand is made, as to its chemical composition, micro-structure (represented on a coloured plate), and also the different places where nephrite is found throughout the world. But we must merely commend these interesting researches to the attention of mineralogists.

As may be seen from the foregoing notice, the work of M. Moushketoff is an acquisition of the first importance for all those interested in the geography and geology of Turkestan. The chapters containing the descriptive part of the work will be, for a long time to come, an especially valuable source of varied and reliable information.

P. K.

CHEMISTRY FOR THE GOLD-FIELDS

Chemistry for the Gold-Fields: including Lectures on the Non-Metallic Elements, Metallurgy, and the Testing and Assaying of Metals, Metallic Ores, and other Minerals, by the Test-tube, the Blow-pipe, and the Crucible. By James G. Black, M.A., D.Sc., Professor of Chemistry, Metallurgy, and Assaying in the University of Otago, and Otago School of Mines. 8vo, pp. 569. (Dunedin, 1885.)

THE title "Chemistry for the Gold-Fields" the author justifies by stating in his preface that in writing this book he had three objects in view:—

"First.—To put into the hands of miners and prospectors a guide to enable them to identify, by simple tests and cheap appliances, the valuable ores when they find them.

"Second.—To provide a manual in chemistry, metallurgy, analysis, and assaying for the 'Schools of Mines' which are now being established on the gold-fields of the colony.

"Third.—To provide for his own students in the chemistry, metallurgy, and assaying classes in the University of Otago, a text-book in these subjects introductory to the larger treatises"

The book includes an elementary treatise on the chemistry of the various elements, and on this portion of the book it is scarcely necessary to dwell, as it is claimed that "the feature of the book" is to deal with "such subjects as have a direct reference to the mineral resources" of New Zealand, and "the extraction of the metals from their ores." We propose, therefore, to confine our remarks to that portion of the work which relates more especially to the detection of minerals, the methods for assaying them, and their metallurgical treatment. The ores of each metal are described, their chief physical characteristics being stated, as well as the ordinary blow-pipe tests, and this latter portion of the subject is made more useful by an appendix on the use of the blow-pipe by A. Montgomery, M.A., the brevity of which is greatly to be regretted.

To the metallurgy of zinc the author devotes little more than three pages, nearly half of which is devoted to the abandoned English crucible process. The Belgian process is briefly described, and in half-a-dozen lines the Silesian process is touched upon. With regard to this latter description the author remarks that "various modifications of this process have now, it is said, been adopted in many of the larger smelting works." This remark could with justice have been appended to many of the descriptions of other processes given by the author. In the metallurgy of lead the use of iron for the decomposition of the silicate is not mentioned, and the description of lead-refining is very incomplete, as also is that of the process for the de-silverisation of lead by the aid of zinc; the use of steam for the de-zincification of the lead is not given. In the description of the Welsh process of copper smelting the coarse metal slag is stated to be a ferric silicate—ferric silicates are, as such, rarely, if ever, produced in metallurgical processes. In describing the refining of copper the author gives equations to show that the reduction of the cuprous oxide on poling is due to the products of the dry distillation of the green wood employed; the action of the anthracite spread over the molten metal is not referred to. The electrolytic refining of copper is not mentioned, and electrolytic processes generally, which would be so important in a country like New Zealand, are ignored.

In describing the Ziervogel process the author remarks, p. 344: "When copper pyrites containing silver is roasted, under certain conditions, the iron and copper may be converted into insoluble oxides, while the silver is converted into sulphate of silver which dissolves in water. The presence of mercury promotes this reaction." This at least suggests that mercury should be charged into the roasting furnace. Again, p. 348, Ziervogel's "process is now carried on on a large scale at Freiberg, in Saxony," the fact being that it has long been abandoned there, except as a very minor incident of a portion of the process. The process of pan-amalgamation, as described by the author, is inaccurate. In describing the methods employed for the production of steel, the Siemens "ore and pig" process is not mentioned, and the basic Bessemer process is only referred to by the sentence: "By a recent invention, however, whereby the converter is partly lined with lime, it is said that sulphur and phosphorus are also removed in the Bessemer process."

The author suggests "a rapid process for distinguishing galena from zinc blende, grey antimony ore, and the other mineral sulphides for which it is sometimes mis-

taken," which involves solution, evaporation, &c., for lead sulphate, filtration, and submitting the solution to ordinary chemical tests. This is surely not a method adapted to the use of "miners and prospectors."

With regard to assaying, in the case of copper ores not one of the ordinary methods of assay is given, and the ordinary method for assaying silver ores finds a place in an addendum to the volume. The whole book affords additional evidence of the prevalence of the belief in the fallacy that a chemist must of necessity be acquainted with a subject so dependent on his own, yet so widely differing from it, as metallurgy.

OUR BOOK SHELF

Microbes, Ferments, and Moulds. By E. L. Trouessart. "International Scientific Series." (London: Kegan Paul, Trench, and Co., 1886.)

THIS book, which aims at the instruction in microbes not so much of the medical and scientific as of the general public, is a fairly accurate exposition of the present state of our knowledge of the morphological and physiological characters of moulds and bacteria.

The chapters on fungi and moulds, of the various ferments and yeasts, and their chemistry, are the best parts of the book. Those on bacteria, septic and pathogenic, are less commendable, since they contain a good many dogmatic statements not accepted by bacteriologists. The chapter on laboratory research and culture of microbes is imperfect in its account of the now generally employed methods of cultivation on solid nutritive media.

One of the most conspicuous deficiencies of the book in the eyes of the scientific reader is the one-sided account given by the author of many of the discoveries made in bacteriology, since the works of French authors form as it were the basis of the author's account. It is certainly a novel proposition that "the science of microbes is essentially a French science."

The book is well illustrated, and written in a clear and concise manner.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Luminous Clouds

THE clouds described by D. J. Rowan, on p. 192 in your issue of the 1st inst., seem to have been of the same kind as were described in several letters in NATURE last summer; they were seen by myself in Bavaria. I saw these extraordinary clouds again this year, on the 28th of May, at Freshwater Bay, Isle of Wight, and on the 23rd of June at Bideford. They were seen by A. C. Dixon at Sunderland on the 2nd, 3rd, 13th, 16th, 22nd, and 23rd of June, and on the latter date were very striking. A description of them on the same date, written by E. Greenhow, appeared in the *Newcastle Chronicle*, as seen near Earsdon in Northumberland, erroneously describing them as a kind of aurora. On that night the display at Bideford was comparatively slight: at 10.18 p.m. the upper limit of the clouds distinctly visible was five-eighths of the way from the horizon to γ Andromedæ, and I presume that that was the limit to which the sun was shining upon them; though with field-glasses I could see them very faintly rather higher up.

I never saw them before last summer, and they are quite different from the iridescent clouds that have created such interest the last two winters, resembling them only in their height and brilliancy. If they require a name I hope the word *boreales*, as proposed by Mr. Rowan, will not be adopted; for they appear in the north only because the sun lies in that direction, and if they occurred at any other time of the year, or in any place much further south than this country, their direction would necessarily be different. On all the occasions which I have seen these clouds they have exhibited a very fine structure like cirrus. The colours of the clouds appear to be due to the same cause as the colours of the sky, for they generally correspond with these at similar altitudes, the upper visible portion of the sheet of clouds being green or bluish, and the lower portion a dull yellow, becoming more orange towards the horizon.

Sunderland, July 8 T. W. BACKHOUSE

Re Immisch's Thermometer

IN your article, p. 234, referring to this pretty little instrument, you refer to the appellation "metallic" as not a happy one in describing it. This I pointed out to the maker some time ago, and termed it an *avitreous* thermometer, as glass plays no part in its construction beyond that of a protector to the dial. The certificates of verification are printed with the instrument so designated, and probably the erroneous term will soon drop out of use. I must also crave permission to correct a misprint in your correspondent's statement with regard to the number of avitreous thermometers verified here up to the present date: for 500 read 300. G. M. WHIPPLE,

Superintendent Kew Observatory

Kew Observatory, July 10

Kirby and Spence's "Introduction to Entomology"

WITH reference to a just complaint made by "R. M." in his article contained in NATURE for July 1 (p. 190) about the want of good indexes to books, and specially to the early editions of Kirby and Spence's "Introduction to Entomology," may I venture to inform him that should an index to the latter book be desired by "R. M." or any other reader of NATURE, they have only to apply to "E. E. J.," Camerton Court, Bath, to obtain one *gratis*. I found the book so perfectly useless for want of one, that I made one some years ago, a copy of which was accepted by the British Museum authorities, and is now included in their Catalogue. I have a good many copies on hand, which I am always glad to give away on application.

E. E. JARRETT

11, Holles Street, London, W., July 8

ON VARIATIONS OF THE CLIMATE IN THE COURSE OF TIME[1]

II.

IF such a periodical variation in the climate does take place, we should be able to trace it in the older formations, as we cannot assume that it first began to operate in the most recent geological age. We must, therefore, try to discover if such variation can be traced in the earlier times.

During the melting of the Norwegian inland ice it left here and there moraines, and on the map drawn by Kjerulf they are seen to stretch in lines more or less continuously across large parts of Southern Norway. On both sides of the Christiania fjord the outside lines, the so-called "Raer," stretch like gigantic ramparts from Moss and Horten south-east and south-west many miles wide through Smaalenene and far into Sweden, and, on the other side of the fjord, through the province of Jarlsberg and Laurvig to Jomfruland outside Kragerö. And behind this outside line of moraines others follow in more or less broken but distinct continuity, one behind the other, through all Southern Norway. These lines show that the

[1] The following is a short abstract from various papers, viz.: "Essay on the Immigration of the Norwegian Flora during Alternating Rainy and Dry Periods" (Christiania, 1876). "Die Theorie der wechselnden kontinentalen und insularen Klimate," in Engler's *Botanische Jahrbücher*, ii. (Leipzig, 1881). "Ueber Wechsellagerung und deren mutmassliche Bedeutung für die Zeitrechnung der Geologie und für die Lehre von der Veränderung der Arten," in *Biologisches Centralblatt*, iii. (Erlangen, 1883). "Ueber die wahrscheinliche Ursache der periodischen Veränderungen in der Stärke der Meeresströmungen" *l.c.* iv. (Erlangen, 1884) Continued from p. 222.

ce did not recede gradually, because it would not then have left behind such great ramparts, but the sand and the gravel would have been spread more evenly. During the melting, however, its edge remained at times stationary, or advanced perhaps a little. At each such event a row of moraines was formed, and as the same are found in large tracts of the country, they cannot be attributed to local circumstances, but we have to assume *that periodical variations of climate were the cause of the manner in which the ice receded.*

We found in the peat-bogs alternately layers of different kinds, peat alternating with remains of forests several times, and we saw how this was easiest explained by periods of change in the climate. But these alternating layers are not peculiar to the peat alone, but found in all stratified formations, loose as well as solid, whether deposited in fresh or salt water, or on land, in all the strata from the Laurentian gneiss to the loose deposits of the present age. Take a geological structure from any age, alternating layers will be found everywhere. Sand alternates with gravel, sandstone with conglomerate, clay with sand, slate with sand or sandstone, marl with clay, chalk with marl, and so on. The layers vary in thickness, from several yards to less than an inch.

The solid rock withers away by the action of air and water in heat and cold; it partly crumbles away mechanically and partly changes chemically. The products of the erosion are carried by wind or running water as dust, in dissolved or original state, and deposited in places more or less remote from those where they were produced. The foaming mountain stream often carries great stones in its course, and the softer the wind and the weaker the current the finer is the matter deposited. When the current becomes weak the gravel sinks first, then the sand, then the clay, and, finally, the chemically-dissolved lime by the animal life in the water. When we, therefore, have a change of beds of different composition through all geological ages, as those mentioned above, it must be due to the circumstance that the speed of the depositing stream was always varying—now increasing, now decreasing.

The *Challenger* Expedition has taught us that all the stratified rocks which geologists hitherto have known must have been formed comparatively near the shore, even if deep-sea formations. They are all of quite a different nature from the strata in the abysses of the great oceans. From this it follows that the variations in the rainfall might have had some influence on the nature of the strata in the known geological formations, since they were formed comparatively near land and are the result of the erosion of the solid rock. A weak river is unable to carry debris far out to sea, but a strong one is capable of supplying the sea-currents with deposits over great areas. When, therefore, the rivers alternately increased and decreased, the sand, clay, and gravel were carried now a greater, now a less distance, into the sea, and thereby the variations of the layers were produced.

It is, however, not the intention to assert that all alternations of layers are due to that long climatic period. When the stratification goes on quickly, and the supply of matter is plentiful, rapid local changes may produce an alternation of strata. In the Norwegian marl-clay, formed during the melting of the inland ice, alternating thin layers of sand and clay are found, varying in colour, sometimes only a quarter of an inch in thickness or even less. These variations must be ascribed to changes during brief spaces of time, and cannot be referred to the long climatic periods. But, of course, such layers are only formed in the immediate vicinity of the coast, and during the constant advance and retrogression of the latter, which may be traced through all geological ages, such shore-formations were most exposed to destruction. They were frequently lifted above the sea, and were more exposed to the destructive agencies—air and currents—than those formed in deeper waters further from the shore. For this reason these quickly-formed layers have at all times been more exposed than others to destruction, and we must, for that reason, conclude that most of the beds which constitute the geological stratified deposits were formed somewhat further from the shore, and that, consequently, the time of their formation was longer. From the thickness of the layer alone it is impossible to form an idea of the time it has taken to form, because in the time a layer in one place upwards of several yards in thickness has been forming, only an inch has formed in another, whilst in a third place in the same time the formation has ceased, or older layers even carried away. But we have a means whereby we may ascertain the time it has taken to form a layer, viz. the study of the remains of the flora and fauna found in the same. The most frequent species have, *ceteris paribus*, the most chance of being preserved. When, therefore, we find that fossils, as is often the case, vary from stratum to stratum, we must assume that this proves that great changes took place in the fauna and the flora during the formation of each stratum. What was stated above with regard to the variations in the peat-bogs of remains of plants from layer to layer *may be applied to variations of strata through all ages.* The examination of the fossils in the strata teach us respect for Time. The fossils vary quickly even in strata of small thickness. In one stratum we find remains of distinct animals and plants, and in the one above—although, perhaps, only an inch above it—we find others quite different. A thin stratum of a couple of inches is sometimes distinguished by peculiar animals and plants, so that the stratum may be recognised over large areas by the aid of the same. When two strata of different nature alternate, it is generally found *that one kind of stratum contains certain fossils, and that those of the others are quite different.* The theory of periodical variations of the climate *explains all this.* Because if the sea-currents varied in strength, the temperature of the water, and consequently the aquatic fauna and flora, must have changed too; with a higher temperature of the sea the moisture of the air and the rainfall must have increased, *and thus a periodical change of the sea-currents would have the effect of causing variations of the strata.* It is exactly such strata of varying nature, and varying forms of fauna and flora, which would build the geological strata of the earth.

We have seen how this theory explains a number of various well-known puzzles to scientific men, viz. the scattered extension of species of plants and animals; the formation of the terraces of shell-banks and shore-lines; the rows in which moraines appear; and, finally, the alternation of peat-layers and various geological strata. It only remains now *to find a natural cause for such a periodical variation of the climate*, but before doing this it is necessary clearly to understand what the theory demands.

It does not require great changes; all the facts on which it is founded may be explained by comparatively small variations in the extremes of temperature and rainfall. No very great variation is required in order that the holly and similar coast-plants should be able to grow by the Christiania fjord, as the theory assumes it once did; because the holly, which cannot stand the winter cold at Christiania (lat. 60° N.), has for many years been successfully cultivated in the open air at Horten, only half a degree further south on the same fjord. And along the coast plants of Oriental origin have, during the last thousands of years, spread from the Christiania and Throndhjem fjords right out to the open shores of Jæderen and Fosen, the former in lat. 58°–59° and the latter in lat. 63°–64° N., and there would hardly be required a very great change to enable them to grow also in the intervening district, the province of Bergen, which would again make their extension continuous.

Whether the surface of a bog becomes covered with forest or not, whether the peat grows or not, whether during the rising the erosion is strong enough to hollow out the shore-line, or the carrying power of the river is great enough for the formation of terraces, whether the edge of the inland ice recedes or advances, whether a deposit of clay or marl is to be found in a certain place near the shore, or whether chalk only is left—*may entirely depend on small variations in the climate, as the conditions will alter as soon as a certain point is reached.* The periodical changes dealt with here were therefore not great; but as they acted simultaneously, and in the same direction, over whole climatic areas, it must be generally-acting forces which caused the same, and not variations in local conditions.

The theory advanced here proves thus that the climate is at all times subjected to periodical changes, the duration of which may be measured in thousands of years, and which act in the same direction within the same climatic area, which for one period are not important, but which, as the alternation of the strata is often remarkably regular, *seem to return after the lapse of a fixed cycle of years.*

It is obvious that periodical changes in the strength of the ocean currents will cause corresponding changes in the climate of the adjacent continents. Thus, for instance, if the warm North Atlantic current, to which North Europe owes its climate, which is mild compared with its latitude, should increase in strength, the climate there would doubtless become still milder. Our shell-banks show that such changes in the temperature of the sea have accompanied climatic variations. We are, therefore, compelled to ask, What is the force which causes this warm sea-current to flow northwards, and may we assume that there is some natural cause effecting periodical changes in the intensity of this force? The question being one as to a climatic period, we must examine the great laws which govern the climate. We must, of course, leave all temporary disturbances of the air out of consideration, and only pay attention to the great and simple laws which are revealed by the synoptic charts of the average distribution of the aërial pressure at various seasons. These charts show us:—in the summer a low pressure over the heated continents, but generally a higher one over the cool oceans; and in the winter a higher pressure over the cold continents, and a lower one over the oceans, which are warmer.

In order to understand this varied distribution of pressure, we shall imagine an atmosphere which everywhere has the same degree of heat and the same height. The warmer the air the more it expands, so that the height of the atmosphere will change if the temperature rises or falls. If we further assume that the air cools or becomes more quickly heated in some places than others, the equilibrium will be disturbed. Over cold areas the height of the atmosphere will decrease. The surface of the atmosphere should thus become uneven, and consequently, in the upper strata of the atmosphere air must flow from the warm regions into the cold ones, so that equilibrium be maintained. For this reason a greater mass of air will lie over cold regions, which have, therefore, a higher atmospheric pressure. But at the surface of the earth, too, the equilibrium will be disturbed, as a higher atmospheric pressure will drive the air from the cold to the warm regions. As long as the temperature of the air varies, movements will be created by the disturbed equilibrium, during which, therefore, air will flow from the cold to the warm regions along the surface of the earth, and *vice versâ* in the upper part of the atmosphere. In winter as well as summer the disturbances of the equilibrium of the atmosphere will proceed from the continents, because the latter are heated and cooled more intensely than the oceans. Over the ice-covered interior of Greenland the sun in the summer cannot create any low pressure, because all its heat is consumed in melting the snow. Even in the summer comparatively cold air and high pressure prevails over Greenland, and this is probably the cause of the atmosphere in the North Atlantic differing from the above-mentioned law, inasmuch as this ocean has a low pressure even in summer. This low pressure, which lies generally near Iceland, is, however, more marked in winter.

The air, according to the law of Buys Ballot, moves *against* the low pressures, so that in the Northern Hemisphere one has the low pressure a little in front to the left when turning the back to the wind. That is but a natural consequence of the rotation of the earth's axis. At lower latitudes this action is more intense. Air, flowing from lower to higher latitudes, retains for a time its original speed of rotation, and will thereby deviate in the direction of the rotation of the earth's axis, *i.e.* towards the east. And *vice versâ* when the air flows from higher to lower latitudes. In this manner southerly winds become south-westerly, and northerly ones north-easterly. In fact, the low atmospheric pressure at Iceland draws the south-west winds up the North Atlantic, and as the cause prevails all the year round, the consequence is that south-west winds blow in this sea summer as well as winter.

The opinion held by Croll, Zöppritz, &c., that winds are the chief cause of sea-currents, is now generally accepted by *savants.* The winds set the surface of the sea in motion, and by frictional resistance the movement is conveyed to lower depths. It depends on the force and the duration of the wind how deep the action will have effect. The main current runs in the direction of the prevailing wind, and its speed is dependent on the average speed of the surface. Winds of short duration are only capable of changing the direction of the current on the surface, but through the *predominance* of such winds through thousands of years, *great currents are created.* Their strength may vary, *but their direction is independent of temporary changes of the wind.* For the upper system of currents, which alone affects the climate, and which reaches to a depth of a couple of hundred fathoms (Mohn), the average direction and force of the wind during the last great epoch are determinal.

Such a great stream is the warm North Atlantic current. It softens the winter even at high latitudes. As the surface imparts heat to the air, the heat lost is replaced from lower depths, and as long as there is a store of heat below the sea will always yield heat to the air.

The mild climate of Norway is, therefore, dependent on this warm current. It runs predominantly in a north-easterly direction, and thus it must, in consequence of the general laws for currents and winds, have run through untold ages, or as long as sea and land have been divided as at present.

We will now see if the force which guides this current is periodically changeable. As we know, the orbit described by the earth round the sun is not circular but elliptical, so that the distance between the two bodies varies according to the seasons; when there is winter in the Northern Hemisphere the earth is nearest to the sun, and the nearer the earth approaches the sun the quicker it travels, so that the winter in the north is shorter than the summer. The difference is five days. In the Southern Hemisphere, on the other hand, the winter is five days longer than the summer. But these relations change through the precession of the equinoxes, the period having a mean duration of 21,000 years. Thus, 10,500 years ago the conditions were the reverse of what they are at present, and the same will be the case 10,500 years hence. The winter at the Northern Hemisphere will then fall when the sun is furthest from the earth, and last longer than the summer, and in the Southern Hemisphere the conditions will be the reverse.

But the orbit of the earth is also subjected to periodical changes, inasmuch as it differs more from the circular

sometimes than at others. The further it deviates from it the greater becomes the difference between the length of winter and summer, and the difference may even amount to more than thirty days every year. The length of winter and summer varies therefore in the course of 10,500 years, and the difference increases the more the earth's orbit deviates from the circular. During the 10,500 years in which the winter is longer than the summer there will be several thousand more winter days than summer ones, and in the second half-cycle there will be as many thousand less. Even at present, when the orbit deviates but little from the circular, the excess of winter or summer days for each half-cycle is more than 50,000, and when the deviation is greatest it amounts to nearly 220,000 days, or some 600 years.

As the cooling of the continents contributes to preserve the low atmospherical pressure over the oceans, and thus directs the prevailing winds and currents at sea, the winds thus directed, as, for instance, the south-west winds of the Atlantic, must be stronger in winter than in summer. *And this is indeed the case.* The weather conditions differ in summer and winter. Of course south-westerly winds blow predominantly in the North Atlantic and West Europe all the year, *but they predominate more in the winter.* According to Prof. Mohn, their force in the North Atlantic is about three times as great in the winter as in the summer, and similar conditions prevail in the Pacific Ocean. In the southern temperate seas north-west winds, which correspond to south-west ones with us, are equally predominant when there is winter in that hemisphere. It will therefore be seen that the forces which promote the warm sea-currents in our latitude *are most active in the winter.* And the same is the case in the Southern Hemisphere, so that it must be said that the winter favours these currents, whether it falls when the sun is nearest, as with us, or when it is most distant, as in the Southern Hemisphere. From Prof. Zöppritz's studies of the currents it appears that the wind exercises an influence upon the strength of them even long after it has ceased to blow. The action of the winds is summed up through centuries, *and the total recorded in the sea-currents.*

As we know that the wind conditions vary at different seasons, and that the effect of the wind does not cease as soon as it is discontinued, but leaves traces in the sea-currents for a long time after ; so that, in fact, the strength of the current is dependent on the average force of the wind during last great ages—it can hardly be a matter of indifference whether these thousands of days fall as a surplus to winter or summer in the 10,500 yearly half-cycle. When they fall in the winter, the south-west winds must be more predominant than others ; and, correspondingly, when they fall to the summer, weaker. It seems, therefore, reasonable that the currents must increase or decrease as the equinoctial line moves round. When the winter falls in aphelion our warm currents will increase, and when the reverse is the case they will decrease. We should, therefore, now in the Northern Atlantic have a weaker current, and in North-Western Europe less rain and a greater difference between winter and summer heat, *and this is exactly what the theory demands.*

In regions with different weather conditions the case will be different. For instance, in the eastern part of North America north-west winds are more predominant in the winter and south-west ones in the summer. Winter, in aphelion, would here increase the north-west wind, and one might conclude that these parts under such conditions would perhaps thereby obtain a more severe climate, so that it seems evident that variations in the climate will not simultaneously move in the same direction everywhere in the Northern (or Southern) Hemisphere.

From calculations we have elsewhere demonstrated that the varying length of the season alone during the precession of the equinoxes will cause an increase or decrease in the force of the current of several per cent. of the total. And these figures are doubtless below the true ones, but space does not here permit of developing them. We may, therefore, with a high amount of probability conclude *that the precession of the equinoxes causes periodical variations of the climate which are great enough to explain all the facts on which the theory for these periodical variations is based.*

But the eccentricity of the earth's orbit changes so rapidly that in two consecutive half-cycles it is not as a rule the same. Therefore variations in the strength of sea-currents, and consequently also those in the climate in one half-cycle will not be quite balanced in the next, and it might even be possible that greater and more lasting variations of the climate might be caused by the same agencies.

A. BLYTT

The University, Christiania

VEGETABLE PRODUCTS AT THE COLONIAL AND INDIAN EXHIBITION

IN passing through the various courts of the Colonial and Indian Exhibition the prevailing natural resources of each colony are apparent even to the most unobservant, for while the riches of some countries are to be found chiefly in their vegetable products, the wealth of another is in its mineral resources, and of another in its animals.

Regarding the vegetable products, as might be supposed, some of the most interesting objects from a scientific point of view are those which have the least attraction for the general public, such, for instance, as the large and varied collection from the Straits Settlements, or the interesting exhibits from British North Borneo. Amongst the exhibits from the former possessions are various samples of damar, the botanical origin of which is but imperfectly known ; thus, for instance, are specimens of damar sesa, a fossil resin from Larut, Perak, damar meta kuching, or cat's-eye damar, damar renkong, and others. Another fossil resin new to us is called incense or gum Benjamin. Under the name of buah saga are shown some seeds of an *Adenanthera*, probably those of *A. pavonina*, a seed of which is the unit in the Malay jeweller's weight, equal to 4·33 grains troy. The seeds are also eaten by the natives. The tree is found in India, China, and the Philippines. In India the wood, which is of a red colour, hard, and close-grained, is known as red sandal-wood, and is used as a red dye, as well as for cabinet-making and building purposes. On account of their bright red colour the seeds are used as necklaces. Naturally in countries where the bamboo is abundant we should expect to find numerous illustrations of its uses, and various articles of domestic utility, as well as for other applications besides that of ornament, are shown, some of which are very ingenious, such as a trap called grôgoh, used for catching river fish ; it is somewhat of the shape of an eel-pot, and the body of the trap is made of a single piece of bamboo-stem of about 2 inches diameter, and from 14 to 18 inches long. It is split longitudinally for the greater part of its length into fine strips, these are distended to a wide mouth at the top some 6 or 8 inches diameter, tapering to the point from which they spring, where they form the natural stem. By the addition of other fine strips of bamboo fastened round at short intervals a complete funnel-shaped basket or eel-pot is made, the lower or tubular end of which is formed by the hollow bamboo-stem. The ready way in which the natives adapt natural productions is seen in a very simple spinning-top, which is composed of a flattened acorn of the type of *Quercus placentaria*, through the centre of which a piece of wood is driven. In this division also are some very varied sets of betel-chewing appliances as used by the Malays, including the scissor-like implements used for cutting the betel-nuts ; many of these sets are in deftly-worked brass, while others are in more costly metals.

The collection from British North Borneo has many interesting exhibits, notably some remarkably fine specimens of gutta-percha and india-rubber a magnificent plank of the Sumatra or Bornean camphor-tree (*Dryobalanops aromatica*), the crystallised camphor of which is found deposited in cracks and fissures in the wood, occurring sometimes in very large masses; it is largely used by the Chinese, who prefer it to the ordinary camphor of commerce which is produced in their own country. Bornean tobacco is also a prominent object here, and is exhibited both in bundles of cured leaves as well as in cut form. A favourable report has been obtained of this tobacco, and it has been valued above the average of Sumatra tobacco, for which indeed it has been mistaken even by experts.

In the Hong Kong Court the varied uses to which bamboos and rattans are put are largely represented; the difference, however, in the character of the work to which the stems of these two classes of plants are applied is manifest at a glance, for while the rigid stems of the *Bambusas* are used for the rougher or coarser work, those of the pliable species of *Calamus* form the materials from which the finer basket-work, screens, &c., are manipulated. Various examples of the baker's art in the form of biscuits are shown by the Hong Kong and China Bakery Company, Limited, and it is stated, as an illustration of the capabilities of this bakery, that it can turn out 15,000 pounds of ship biscuits or 10,000 pounds of bread per day.

The British Guiana collection almost adjoins that of Hong Kong. Here, as might be expected from the extent of the forests of the colony and the abundance of large hard-wooded trees, timber takes a prominent place, and some magnificent specimens of the best known woods, such as mora (*Dimorphandra Mora*), greenheart (*Nectandra Rodiœi*), wallaba (*Eperua falcata*), and other well-known and useful timbers are exhibited. The heartwood of these timbers is described as "almost everlasting, the beams of old houses being good for over a hundred years in the most unfavourable circumstances of a tropical climate infested with wood-ants and other vermin." Specimens of tibiserie fibre and hammocks made from it are here exhibited. This fibre, which is obtained from the young leaves of the Eta palm (*Mauritia flexuosa*), is of wonderful strength and tenacity, from it the natives make their strongest and most durable cords and hammocks. It is very easily obtained and in any quantity, and if better known in Europe might become a valuable article of commerce. A fine collection of medicinal and tanning barks are here shown, but unfortunately, like the woods from this colony, comparatively few have other than native names. In the catalogue of exhibits it is stated that "the medicinal barks are very varied; a few are well known, but the majority, having never received the attention of chemists or physicians, are as yet untried, but may possibly be found worthy a place in the Materia Medica. Fair quantities are exhibited, and will be distributed to qualified persons who will undertake to report on their qualities. Most of them are common in the colony, and can be easily procured."

It is scarcely correct to say that the medicinal barks of British Guiana have never received the attention of chemists or physicians, for at the close of the International Exhibition of 1862 some twenty different medicinal barks of the colony were experimented upon and their effects tried in various cases by Mr. Charles Hunter, who was some time House Surgeon to St. George's Hospital. The results of his experiments were embodied in a pamphlet, and published at the time by Messrs. Churchill and Sons of New Burlington Street, but we never heard that any of them came into use in this country, and it is to be hoped that better results may be obtained from the present collections.

JOHN R. JACKSON

WHAT IS A GLACIER?[1]

GLACIERS have become so well known from the graphic descriptions of Carpenter, Forbes, Agassiz, Tyndall, and other explorers, that it seems unnecessary at this time to do more than call attention to a few of their more characteristic features by way of an introduction to what I have written concerning those now existing in the United States.

The formation of glaciers in any region depends primarily on the fact that the amount of snow precipitated during a term of years exceeds the amount dissipated by melting and evaporation. In this manner snow-banks of broad extent are formed, the lower portions of which become compacted into ice. The change from snow to ice is known to result from pressure, and as ice is mobile under pressure, either by reason of its inherent plasticity or as a result of regelation, the weight of this mass tends to change its form, and it thus acquires motion, which takes the direction of least resistance.

The essential characteristic of glaciers seems to be that they result from the consolidation of snow in regions of secular accumulation, *i.e.* above the snow-line, and flow to regions of dissipation, *i.e.* below the snow-line. From these primary conditions result a multitude of secondary phenomena.

For convenience of reference we will divide glaciers into *alpine* and *continental;* not that the two classes are always distinct and separable, but for the reason that typical examples of each are well characterised and capable of specific description. Variations occur in each class which may suggest minor subdivisions.

The glaciers with which we are most familiar belong to the class that have their archetype in the mountains of Switzerland, and occur about high peaks, usually in amphitheatres or *cirques* at the heads of high-grade valleys. The snow that accumulates on high mountains, especially in temperate latitudes, is frequently not completely melted during the summer, and thus tends to increase indefinitely. The *névé* of a glacier is such a snow-field. The gorge or valley leading from every alpine amphitheatre furnishes an avenue of escape for the consolidated *névé*-snow, which is forced out through the opening, and flows for a greater or less distance as a stream of ice. Such in brief is the genesis of an alpine glacier. Every glacier of this class is divided into a *névé*, or snow-region, above, and an icy portion below. The line of demarcation is the *snow-line*. As compact ice occurs also beneath the *névé* from which it is formed, this division of a glacier into two portions applies only to the surface. The division line, moreover, shifts with the seasons; at times, perhaps for many years together, the true glacier ice may be concealed by a snowy covering. The *névé* is composed of granular snow, white or grayish-white in colour, and comparatively free from dirt and stones; below the snow-line the glacier is formed of both porous and compact ice, and is usually concealed more or less completely with rock-debris. From a distance these two divisions are frequently distinctly shown by contrast in colour. The stones and dirt that fall on the *névé* sink more or less deeply into the snow and become buried beneath the next addition, and as the *névé* becomes consolidated and acquires glacial motion, this debris is carried along in its mass. But the region below the *névé* being one in which loss exceeds supply, the snow and ice are melted, and the foreign bodies formerly held in the mass become concentrated at the surface, and are then carried along as moraines. Thus in the *névé* region the tendency is to bury foreign objects, and in the glacier proper to concentrate them at the surface.

All the debris carried by glaciers may be designated in general terms as *morainal* material, but when arranged

[1] From Memoir on "Existing Glaciers of the United States," by Israel C. Russell. Reprinted from the Fifth Annual Report of the Director of the U.S. Geological Survey.

in definite ways it receives specific names. When distributed along the margin of an ice-stream it forms *lateral* moraines. Two glaciers uniting, the right lateral moraine of one combines with the left lateral moraine of the other to form a *medial* moraine at the line of contact, the ice-streams flowing on side by side as a single compound-glacier. The debris carried to the extremity of a glacier and deposited about its foot is known as a *terminal* or *frontal* moraine.

In flowing through a valley ice is subjected to stress, which causes it to fracture and form open fissures termed *crevasses*. When a glacier passes over a steep ascent it becomes broken by a great number of fissures, and not infrequently falls to the base of an escarpment in detached blocks, forming an ice cascade, but heals its scars and flows on as a solid mass below. The fissures formed when a glacier passes over an inequality in its bed are commonly transverse to the direction of flow, but may take other courses, depending on the nature of the obstruction, change of slope, &c. Marginal crevasses, resulting from the friction of the ice-stream against its banks and the consequent more rapid flow of the central portion, usually leave the shore at a moderate angle and tend up-stream.

Glacier ice has been found to exhibit a definite structure, known as lamination, or as ribboned or banded structure, produced by the alternation of thin plates or strata of compact bluish ice with others more porous. As shown by Tyndall's experiments, this arrangement is the result of pressure, and is analogous to slaty cleavage.

Owing to unequal melting, the surface of a glacier is usually extremely irregular, the parts protected by moraines standing in higher relief than the clearer portions. Still further diversity is formed by boulders perched on columns of ice, which they have protected from melting as the general surface wasted away. These are termed *glacial tables*. At other times the ice bristles with a multitude of acicular pyramids, or is melted into holes and ice-wells, each having a stone or mass of dirt at the bottom.

The melting of the surface of a glacier gives rise to many rivulets and brooks, which course over it in channels of ice, frequently plunging into yawning crevasses, and finally joining the sub-glacial stream that issues from beneath every glacier. These glacier-born streams are always heavy with comminuted rock, ground fine by the moving ice.

Such in brief are the principal characteristics of alpine glaciers.

At the present time continental glaciers are confined to the arctic and antarctic regions, and have been less thoroughly explored than the alpine forms common in more temperate latitudes. Glaciers of this class are characterised by their broad extent and by not being confined by definite walls; their *névés* are large, frequently covering nearly the entire glacier, and their surfaces are free from boulders and debris, for the reason that they are regions of accumulation, and also because mountains seldom rise above them. Owing to inequalities in the country over which these great fields pass, they are not infrequently broken by crevasses; and, as on smaller glaciers, the melting of the surface gives origin to numerous streams, frequently of large size, which become ponded and form lakes in basins of ice, or plunge into open fissures and disappear in the body of the glacier. Existing continental glaciers are believed in all cases to flow from the interior towards the coast, and hence may be considered as acquiring motion in all directions from a centre of accumulation. When alpine glaciers increase sufficiently to cover an entire mountain-range and form a confluent ice-sheet, they approach and may pass into the continental type. It is not impossible that a mountains range of very modest dimensions might give origin to a quaquaversal glacier of vast proportions. It is perhaps not out of place to suggest in this connection that the glaciers which formerly covered the New England State and Canada were of this character.

In framing a definition of a glacier it is evident that we must include both alpine and continental types, and also embrace the secondary phenomena that are commonly present. A glacier is an ice-body originating from the consolidation of snow in regions where the secular accumulation exceeds the dissipation by melting and evaporation, *i.e.* above the snow-line, and flowing to regions where loss exceeds supply, *i.e.* below the snow-line. Accompanying these primary conditions many secondary phenomena, dependent upon environment, as crevasses, moraines, lamination, dirt-bands, glacier-tables, ice-pyramids, &c., may or may not be present. Thus, glaciers even of large size may exist without moraines; in such an instance glacier-tables, ice-pyramids, ice-wells, &c., would be absent. We may conceive of a glacier flowing through a channel so even that it would not be broken by crevasses, but such instances must be extremely rare. The most common of the numerous secondary features seems to be the laminated structure of glacial ice, but even this is not always distinguishable in ice-bodies that are unquestionably true glaciers.

Although the definition we have presented may assist in understanding the nature of a glacier, yet it is manifestly open to objections. If we consider the snow-line as defining the limit between the *névé* and the glacier proper, it is evident that there must be numerous exceptions to the rule. As before remarked, during certain years, and in many instances for a term of years, the snow-line is much lower than at other times, and may completely conceal the glacier beneath. Again, an ice-stream may terminate in the sea and be broken up and form icebergs before the differentiation into *névé* and glacier proper has been reached.

From all that has been determined concerning the nature of glaciers it is evident that they form one of the transition stages in the history of the snow that falls in certain regions, and like genera and species in the organic kingdom cannot be limited by hard-and-fast lines, but may be classified by comparison with typical examples. From the snow, hail, and frozen mists of a mountain-top are formed the accumulations of granular ice-snow which we call a *névé*. By pressure and alternate melting and freezing, the *névé* passes into compact ice, which acquires motion and is termed a glacier; but the plane of separation is indefinite, and one merges into the other by insensible gradations.

The morainal material carried by glaciers is deposited when melting takes place, and frequently forms characteristic accumulations that still retain the name of moraines. The debris along the border of an ice-stream is frequently left as ridges or irregular terraces on the sides of a valley, marking the former height of the ice-flood. At various stages in the retreat of the ice the lateral moraines are united by terminal moraines which cross the former bed of the glacier in irregular but usually crescent-shaped piles, between which the valley bottom is usually deeply filled with unassorted debris, and frequently occupied by lakelets. When a glacier is prolonged from the mouth of a valley on a plain, it builds out its lateral moraines perhaps for many miles, and when it retreats these are left as parallel embankments, not infrequently hundreds of feet high and sometimes miles in extent.

The movement of glaciers causes friction, which results, as the study of living glaciers has shown, in the smoothing and scratching of the rocks over which the ice passes. The boulders, pebbles, and sand held in the bottom and sides of the glacier produce smooth and polished surfaces, crossed by scratches and grooves having an exceedingly characteristic appearance, which, when once understood, it is difficult to mistake for the results produced by other

agencies. While the rocks beneath a glacier are being worn and rounded, the stones set in the ice are in turn battered and scratched and often ground down to plane surfaces that are not infrequently polished and covered with glacial striæ.

As a rule, alpine glaciers follow pre-existing drainage valleys, which they enlarge and broaden. As frequently stated, a stream-cut gorge is distinctly V-shaped, but after being occupied by a glacier it is found to have become U-shaped in cross-section.

The records of glacial action looked for by geologists are: deposits of morainal material, which frequently differs from the adjacent country rock, and may occur in an irregular manner or be grouped definitely as lateral and terminal moraines; boulders perched in fortuitous positions, as on steep slopes and hill-tops; smoothly rounded rocky knolls; polished and scratched rock surfaces; rock-basins, &c.

NOTES

It is stated that the forthcoming "Life and Letters of Charles Darwin," by Mr. Francis Darwin, will contain a brief autobiographical fragment.

Mr. Murray announces a new edition of Darwin's work on "The Expression of the Emotions in Man and Animals," with the author's latest corrections.

We learn from the *Times* that Dr. Hermann Abich, the distinguished Austrian naturalist, died at Vienna on the 1st inst. at the advanced age of eighty years. He was born at Berlin on December 11, 1806, and attained the grade of Doctor in the University of that city before he was twenty-five. His first scientific tours were in Sicily and Italy. He then became Professor of Mineralogy at Dorpat, and devoted most of his leisure during his residence in Russia to travels in the Caucasus, Armenia, and Northern Persia. His earliest published work was on Vesuvius and Etna in 1833–34, and his latest seems to have been brought out in 1862 on the Geology of Daghestan. By his own request his remains were removed to Gotha for the purpose of cremation.

With reference to the recent catastrophe by which the King of Bavaria and his physician lost their lives, *Science* notes that Dr. Gudden is a sad loss to science, for he was one of the most noted authorities in the sphere of nervous and mental diseases. He has also been at the head of a laboratory in which investigations of the fine anatomy of the brain, spinal cord, and sense-organs have been carried on. He has given his name to a manner of studying the connections of the nervous system which is as ingenious as it has proved fruitful of results. His method consists in extirpating a sense-organ or other part of an animal when young, and then allowing the animal to grow up. At death the animal is examined, and the fibres which have failed to develop will thus be marked out as the paths of connection between the extirpated sense-organ and the brain-centre. For many years he had been at work on the problem as to the mode of connection between the retina and the brain, but his results are not yet before the public.

According to *Science* the first circular of the local committee at Buffalo of the American Association for the Advancement of Science, announces that the meetings will be held in the recently enlarged high-school building. Reduced rates have been obtained over many of the railroads, most of which allow a return ticket at one-third of the usual fare. The Western Union Telegraph Company will place its lines and district telegraph system at the service of members. The Botanical Club of Buffalo is arranging an excursion and reception for the Botanical Section, and the local Entomological Club is doing the same kind service for the Entomological Section. The address of the local secretary is Dr. Julius Pohlman, Buffalo, N.Y.

As our readers are aware, it has been resolved to mark the memorable event of the attainment, on August 31 next, of his hundreth year by the venerable father of modern science, "Le Doyen des Étudiants," as he loves to call himself—M. Chevreul—by striking a medal in his honour. The execution of this medal has been intrusted to M. Roty, old "pensioner" of the Academy of France, at Rome. Contributions towards the commemorative medal are, of course, not to be limited to France, but will embrace the whole scientific world, which everywhere alike claims the author who extended the bounds of science as its honoured citizen. Subscriptions, which will be received up to the 22nd of this month, may be addressed to the President of the Committee, 8, Rue Guy-de-la-Brosse, Paris. A list of the subscribers will accompany the medal, which is to be presented to M. Chevreul on his centenary day, and if the amount of the subscriptions allows of it, a copy of the medal will be sent to the subscribers.

At the sitting, on June 7 last, of the Academy of Sciences at Paris, M. Halphen delivered an address in praise of the labours of M. Bouquet, his immediate predecessor in the seat he holds in that body. From the foundation of the Academy down to the present time, the duty of eulogising departed members has devolved exclusively on the Perpetual Secretaries at the anniversary meetings. The annual death-rate of members has, however, of late been such that a large number of them were in danger of disappearing from the roll without any formal record of their services. The initiative thus taken by M. Halphen was followed up at the next meeting. This step has, of course, been taken in imitation of the arrangements of the Académie Française, in accordance with which each incoming member is required to eulogise his predecessor at a special meeting, an answer being also given in the name of the Academy by another member appointed for that purpose.

Recent soundings have given the following depths for the different Swiss lakes:—Constance, between Uttwyl and Friedrichshafen, 255 metres; Geneva, between Rivaz and Saint-Giugolphe, 256 metres; and between Lausanne and Evian, 330 metres; Brienne, 261; Thun, 217; Lucerne, between Gérau and Rueteren, 214 metres; Zug, 198; Neuchatel, 153; Wallenstadt, 151; and Zürich, 143 metres.

According to Prof. Heim, of Zürich, the total number of glaciers in the Alps is 1155, of which 249 have a length of more than 7500 metres. Of this number the French Alps contain 144, those of Italy 78, of Switzerland 471, and of Austria 462. The total superficial area of these glaciers is between three and four thousand square kilometres, those of Switzerland amounting to 1839 kilometres. The greatest length is reached by the Aletsch glacier, which is 24 kilometres long. As to thickness, it will be remembered that Agassiz, when measuring a crevasse in the Aar glacier, did not reach the bottom at 260 metres, and that he calculated the depth of the bed of ice at a certain point of this glacier at 460 metres.

We have received the *Bulletin* for the past year of the Society for Indo-Chinese Studies of Saigon. Amongst the papers is one by that indefatigable student, Dr. Tirant, on the odoriferous woods of Cochin China, which, though numerous in variety, are of four principal kinds, the most important being aloes and sandalwood. We have already noticed a series of papers by the same writer on the fishes and reptiles of Cochin China. Another interesting paper deals with the textile plant *Sansevıera* as found in Cochin China.

A Commission composed of MM. Becquerie, Berger, and Mascart, having been appointed to examine the question of the

safety or danger of erecting a tower to the height of 300 metres, as proposed, in the Champ de Mars, on the occasion of the forthcoming Universal Exhibition, has reported that there is no danger in connection with such a structure if special precautions are taken for its non-insulation. The tower acting as a lightning conductor would, on the contrary, they explain, serve to protect the whole of the Champ de Mars from injury by lightning if the rules laid down by the Commission on lightning conductors were applicable in the case of so exceptional an altitude.

THE report for the current year of the Coventry Free Public Library is a very encouraging document. It shows increase in all directions—in the number of borrowers, the number of books issued, and a large increase in the number of volumes owned by the Library. All the excellent work done by the Institution is paid for by a trifling rate producing a little over 500*l.*, supplemented by the assistance of a private book club. The tables appended by the Chairman, Mr. Odell, showing the number of books issued in each class of literature, the monthly totals and averages, the ages of the borrowers, and more especially the occupations of the latter, are very suggestive. In these days, when more than ever we have, politically speaking, to "educate our masters," the record of the work of the Coventry Free Public Library is very gratifying.

WE have received from Tokio a copy of a Japanese scientific journal (apparently the NATURE of Japan), which has already reached its third volume and fifty-sixth number. It is printed throughout in Japanese, being much the same shape as NATURE, and containing forty-eight pages in each number. The issue before us contains a lecture on human parasites, by Prof. Ijima; some remarks on the historical methods of the Chinese school, by Mr. Suyematsu, formerly of Cambridge; the third of a series of lectures on physical geography by Prof. Kotō; a paper on "Some Phenomena I have witnessed," and another on methods of treating pebrine, by a teacher in the Kornaba Agricultural College. The notes are also of a very general character: they refer to "some simple physical experiments"; an alloy that expands with cold; the uses of coffee; refining ores by electricity; the strength of paper; a new sweet compound; animal bone industry; hypnotism; the Universal Meridian and Time Conference, &c. Then follow letters to the editor, and finally a report of a meeting of the Tokio Physico-Mathematical Society. No proof is wanted nowadays of the remarkable scientific progress in Japan; if it were, it would be supplied by the fact that a journal of this high character can live and apparently flourish.

WE have also to acknowledge the receipt from the Imperial Meteorological Observatory of Japan of the "Monthly and Yearly Means, Extremes, and Sums for the Years 1883, 1884, 1885," with an appendix on observations of clouds. There were twenty-seven stations, including four in Yezo and one in Corea.

AN interesting work which has just been published in the *Bulletin* of the United States National Museum (No. xxx. pp. 113-81) is an annotated catalogue of the published writings of Dr. Charles Abrathar White, the distinguished palæontologist to the United States Geological Survey, and the occupant of several other important scientific positions. The editor is Mr. J. B. Marcou, whose object has been to note everything containing any expression of Dr. White's views on scientific subjects, as well as his more elaborate works. The annotations which accompany the catalogue were drawn up mainly from data furnished by the author himself, and all expressions of opinion on geological and palæontological subjects are his own. The catalogue contains in all 151 entries, ranging from articles in scientific periodicals to his contributions on invertebrate palæontogy in the annual reports of the Geological Survey. The whole represents an almost incredible mass of scientific work, performed as it was in a brief quarter of a century, 1860-85.

WE are glad to notice that Dr. White's important work on the Cretaceous invertebrates of Brazil, which were collected by the Imperial Geological Commission under the direction of the late Prof. Hartt, is now in process of publication at Rio de Janeiro by the Brazilian National Museum. It is to appear in the Portuguese and English languages, and will be illustrated by twenty-eight lithographic plates; 214 species in all are published and figured in this work, of which 116 species are diagnosed as new. Four new genera are proposed—three of Gasteropoda, and one of Echinoids. The former are *Orvillia*, *Cylindritella*, and *Cypræactæon*. The latter is *Heteropoda*, the generic diagnosis of which was supplied to the author by Prof. P. de Loriol, of Geneva.

THE following facts exemplify the strong migratory instincts of trout. At the fish culture establishment at Delaford, where the utmost care is taken to isolate the various species of Salmonidæ, a few of the fish occasionally are found in ponds long distances from those in which they were originally located. Considering that each pond is so constructed as to prevent such a contingency, the occurrence is very remarkable, and can only be accounted for in two ways, viz. that the fish either burrow through holes that probably are made by rats and moles, or they jump out of the water and so proceed to the next pond. It is not likely that they are borne thence by birds, as the appearance of the fish on the occasions referred to does not justify such an assumption. It will be interesting to inquire further into the subject with a view of eliciting the real facts of the case.

A FISH Culture Conference is to be held at the Colonial and Indian Exhibition on July 26 at the instigation of the National Fish Culture Association. The Marquis of Exeter will preside, and papers will be read by Mr. J. Willis-Bund, the Rev. C. J. Steward, Mr. Oldham Chambers, and others, upon fresh-water and marine fish. The Conference will commence at 10.30 a.m. and last until 10 p.m.

A PAIR of electric eels (*Gymnotus electricus*) arrived the other day at the Colonial and Indian Exhibition Aquarium from British Guiana, but have since unfortunately died. They were very fine specimens, and measured 4½ feet in length, their normal size being 6 feet. They require a temperature of 75°, and provided the water is maintained at this standard they will live and thrive in captivity. The water should not be too deep, however, and must be kept clean.

FROM the report issued by the Central Meteorological Institute of Sweden for last year it appears that there are thirty-four public stations for observation, and some half-a-dozen private ones, in that country. In addition to these there are nearly 400 places where the fall of rain is registered, and other partial observations made. These observations have been duly published in the Institute's journal, "Monthly *Résumé*" of the weather in Sweden, edited by Dr. H. E. Hamberg, which has now reached its sixth year of publication. In addition to this, the publication of a short climatological description of each country, founded on the observations of the last twenty-six years, has been continued, whilst Dr. Hamberg has added an important contribution in the shape of the work "On the Influence o Forests on the Climate of Sweden." The twenty-second part of a work "Meteorological Observations in Sweden," a *résumé* of the observations made at the public meteorological station, has also appeared, and finally synoptical tables have been

framed of the weather at all stations on any day of 1884, showing the quantity and nature of the rainfall, thunder, fog, dew, frost, transparency of the air, "sun-snake" (a phenomenon chiefly observed in the northern part of Sweden), aurora borealis, &c. Reports on the forming and breaking up of the ice have been received from fifty-eight stations, besides seventy-seven observations of periodical features of animal and vegetable nature.

THE Swedish Academy of Sciences has issued a work entitled "The Correspondence of Carl von Linnæus," containing a record of all the correspondents of this famous naturalist, Swedish as well as foreign, with their addresses, date of birth and death, &c., as well as the date of each letter to and from.

THE first African city lighted by electricity was not Algiers or Cairo, but Kimberley, with forty-two Brush lamps, each of 2000 candle-power. The current is also utilised there for the killing of dogs, a step suggesting the execution of death sentences by the same means, as proposed in America and in France by M. Charson, a member of the Senate.

MR. R. N. CUST, the Secretary to the Royal Asiatic Society, is engaged on a work on the languages of the tribes of Polynesia, including those of Australia.

THE additions to the Zoological Society's Gardens during the past week include a Squirrel Monkey (*Chrysothrix sciurea* ♂) from Guiana, presented by Madam G. Sangiorgi; a Macaque Monkey (*Macacus cynomolgus* ♀) from India, presented by Mr. D. Evans; a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Capt. Pitman; a Common Cormorant (*Phalacrocorax carbo*), British, presented by Mr. O. Moulton Barrett; two Golden Eagles (*Aquila chrysaetus*) from Scotland, a Lined Finch (*Spermophila lineola*) from South America, deposited; two Ostriches (*Struthio camelus* ♂ ♀) from North Africa; a Lear's Macaw (*Ara leari*) from South America; a Lineolated Parrakeet (*Bolborhynchus lineolatus*) from Mexico, purchased; a Bennett's Wallaby (*Halmaturus bennetti* ♀), a Vulpine Phalanger (*Phalangista vulpina* ♂), three Canadian Beavers (*Castor canadensis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE STRASBURG OBSERVATORY.—Herr W. Schur has published, in No. 2736 of the *Astronomische Nachrichten*, a supplementary report on the work done at the Strasburg Observatory during the ten months preceding May of this year, so as to exhibit the state of the instruments and of the computations relating to the observations made with them on the eve of his departure for Göttingen, where he has been appointed Director in the room of the late Prof. Klinkerfues. During the interval to which the report refers, Herr Schur was chiefly occupied with observations of the moon with the altazimuth and of comets with the great refractor, also with the examination of the micrometer-screw of the latter instrument. The meridian-circle has chiefly been employed in the observation of southern stars—amongst others the eighty-three stars of Auwers' Southern Fundamental Catalogue, and certain stars for investigating astronomical refractions. The direct and reflection observations to the end of the preceding year give for the geographical latitude of the meridian-circle, $+48° 35' 0''.11$, which agrees well with a former determination with Repsold's transit, using Horrobow's method, viz. $+48° 35' 0''.23$. In former reports Herr Schur has drawn attention to the discordance between the nadir points determined with observer north and observer south, which, for his observations, amounts to a considerable quantity; in the mean, from a large number of observations, ½ (north–south) being as much as $+0''.50$. This large value agrees both in sign and in magnitude with the quantity determined from observations of zenith stars for similar positions of the observer, viz. $0''.77$, and Herr Schur concludes that his observed zenith distances of stars require a correction of about $-0''.6$. In the case of the other Strasburg observers, the corresponding correction is comparatively insignificant. Herr Schur's successor at Strasburg is Dr. Kobold.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 JULY 18–24

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on July 18

Sun rises, 4h. 6m.; souths, 12h. 5m. 56·5s.; sets, 20h. 6m.; decl. on meridian, 21° 1' N.: Sidereal Time at Sunset, 15h. 52m.

Moon (two days after Full) rises, 20h. 42m.*; souths, 1h. 31m.; sets, 6h. 27m.; decl. on meridian, 14° 5' S.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° '
Mercury	6 41	13 54	21 7	13 21 N.
Venus	1 33	9 38	17 43	21 46 N.
Mars	11 3	16 48	23 33	3 38 S.
Jupiter	10 9	16 18	22 27	0 58 N.
Saturn	3 7	11 15	19 23	22 20 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich)

July	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
19	ϵ^2 Aquarii	5½	3 30	near approach	35 —
19	ϵ^1 Aquarii	6	4 1	near approach	220 —
24	μ Ceti	4	23 32	0 24†	52 267

† Occurs on the following morning.

July	h.	
19	10	Mercury at greatest elongation from the Sun, 27° east.

Variable Stars

Star	R.A. h. m.	Decl. ° '		h. m.	
U Cephei	0 52·2	81 16 N.	July 18,	23 32	*m*
			,, 23,	23 11	*m*
R Piscium	1 24·8	2 18 N.	,, 23,		*M*
S Ursæ Majoris	12 39·0	61 43 N.	,, 24,		*M*
V Bootis	14 25·2	39 22 N.	,, 22,		*M*
U Coronæ	15 13·6	32 4 N.	,, 21,	22 22	*m*
U Ophiuchi	17 10·8	1 20 N.	,, 22,	2 58	*m*
			,, 22,	23 6	*m*
X Sagittarii	17 40·4	27 47 S.	July 24,	2 0	*M*
U Sagittarii	18 25·2	19 12 S.	,, 19,	2 0	*M*
S Vulpeculæ	19 43·7	27 0 N.	,, 24,		*m*
χ Cygni	19 46·2	32 38 N.	,, 18,		*m*
S Delphini	20 37·8	16 41 N.	,, 22,		*M*
δ Cephei	22 24·9	57 50 N.	,, 18,	21 30	*m*

M signifies maximum; *m* minimum.

Meteor Showers

Meteors begin to be somewhat numerous in the latter half of the present month. Amongst the radiants represented are the following:—Near π Andromedæ, R.A. 10°, Decl. 38° N.; near β Cassiopeiæ, R.A. 6°, Decl. 58° N.; near η Draconis, R.A. 245°, Decl. 64° N.; near σ Serpentis, R.A. 266°, Decl. 12° S.; near α Cygni, R.A. 312°, Decl. 46° N.; from Lacerta, R.A. 342°, Decl. 40° N.; and the great *Perseid* shower, maximum August 10, radiant R.A. 45°, Decl. 56° N., begins to furnish individual meteors about this time.

GEOGRAPHICAL NOTES

ACCORDING to the *Colonies and India*, the Secretary of the Victorian branch of the Geographical Society of Australasia has written to the Royal Society of Victoria asking the latter to appoint a committee to confer with that already appointed by the former Society on the question of sending an exploring expedition to the Antarctic regions. It is urged that a conference should take place as soon as possible, and that various scientific associations should be invited to co-operate in sending out one or more expeditions.

THE latest news from the Chitral Mission is that Col. Lockhart is returning to India from Zebah, in Badakshan, leaving Col. Woodthorpe in charge of the party.

IT is stated that Mr. A. R. Colquhoun, who is at present Civil Commissioner at Mogoung, in Upper Burmah, is about to start

on a journey of exploration into Upper Assam, and the regions lying between this and Burmah.

THE latest official information concerning Dr. Wilhelm Junker, the African traveller, comes from Zanzibar. It reports that while he was staying with the King of Unyoro, the latter was attacked and defeated by the King of Uganda. The King of Unyoro and Dr. Junker succeeded, however, in making their escape. Dr. Junker lost all his collections, but saved his journals.

AT the March meeting of the Geographical Society of Stockholm it was decided not to distribute the *Vega* medal—the greatest honour the Society can confer—this year. Only three travellers have as yet received it, viz. Nordenskjold, Pallander, and Stanley. The *Vega* fund was awarded to Dr. F. Svenonius, for explorations in the Lapland highlands during the summer.

A RECENT number of the *Verhandlungen* of the Berlin Geographical Society (Band xiii. No. 5), contains an important paper on Corea, by Dr. Gottsche, who travelled widely over the peninsula on behalf of the Japanese Government. During two journeys he traversed all the eight provinces of the country, and visited 80 of the 350 district towns. The general features are already tolerably well known to English students from Mr. Carles's reports laid before Parliament, and his paper in the *Proceedings* of the Royal Geographical Society, but as Dr. Gottsche is a geologist, and travelled specially for scientific observation, he supplements Mr. Carles's papers in this direction. The climate, he shows by meteorological tables, lies between that of Japan and of North China, while in the North the climate in winter is of almost Siberian rigour. As for the geological formation, granite, gneiss, and crystalline schists play a predominant part. Here and there these are broken by the older volcanic formations, as diabase and quartz porphyry; palæozoic strata occur rarely, and the later sedimentary formations not at all. Active volcanoes do not exist on the mainland, and earthquakes have been unknown within the memory of man. The only metal in which Corea is rich is iron; the belief that gold abounds is a delusion. The fauna is rich, and of much interest, for palæarctic and sub-tropical types meet here. With regard to the flora, unfortunately a large portion of Dr. Gottsches' collection was lost, and the remainder was handed to Dr. Engler of Breslau for examination and report. In conclusion, he says that though Corea may never be popular with the ordinary traveller for pleasure, it will well repay the visitor on scientific objects intent. Prof. Enting, who travelled in the interior of Arabia in 1883-84 on an antiquarian mission, especially in search of inscriptions, gives a long and comprehensive account of the regions through which he went.

THE new number (Heft ii. Bd. 9) of the *Deutsche Geographische Blätter*, the organ of the Geographical Society of Bremen, contains the conclusion of Dr. Oppel's paper on the Congo basin. The present instalment refers to the explorations of past years, the lower, central, and upper Congo region, north and south of the stream, the climate, meteorology, botany, zoology, ethnography, &c. It will thus appear that the paper is an encyclopædic one on the great West African river. The next paper is a continuation of Herr Valdau's account of his journey to the north of the Cameroons, especially around Lake Mbu, and between that and Balundu, and the coast. Herr Steinvorth gives a most interesting account of a little Slav colony or oasis in Hanover called the Hanoverian Wendtland, where the people down to this day have in great part preserved their original speech, customs, and other peculiarities. The writer describes all these in some detail, and comes to the natural conclusion that here we have not a pure Wendt people, but one largely mixed with Germans, and thinks, after discussing the peculiarities of the dialect spoken, that this colony is worthy of the attention of the student of comparative language. This is followed by a general article (which is anonymous) on the new Chilian province of Tarapaca, dealing especially with the silver mines and saltpetre industry. Herr Seelstrang supplies a paper of more direct geographical interest on the region about the source of the Rio Chubut, hitherto one of the least-known parts of the Argentine Republic. The rest of the number (which is of considerable size) is occupied by geographical intelligence, reviews of books, &c.

IN the *Bolletino* of the Italian Geographical Society for May, Signor Sommier describes the excursion which he made with Signor G. Cini to Cape North in January 1885. Some interest attaches to this journey, which is the first made across Lapland and Finland in midwinter for purely scientific purposes. The travellers proceeded by train from Christiania to Throndhjem, and thence by steamer in darkness and storm to Hammerfest and Skarsvaag, in the island of Magerö, the northernmost group of habitations in Europe, and the nearest permanent settlement to Cape North. Here they received a friendly welcome from the local "Landelsmand," and reached the goal of the expedition on foot with much greater ease than had been anticipated. The weather was unusually calm and mild, with a temperature of only $-2°$ C. At some points the evergreen lichens and other growths (*Betula nana*, *Empetrum nigrum*, *Diapensia lapponica*, &c.) were visible through some centimetres of transparent ice clothing the surrounding rocks. The only animals seen, besides the eider and other water-fowl, were the raven, crow, magpie, Arctic fox, and frankoline, the latter (*Lagopus mutus*) everywhere present in large numbers. Several photographs were taken, and after a stay of eleven days in the neighbourhood, during which the glass never fell below $-16°$ C., the travellers returned by water to Hammerfest and Bossekop, at the head of the Alten fjord. Thence the route was continued overland under great hardships—eastwards to Lake Enare, southwards through Kittilä to Haparanda, and round the west side of the Gulf of Bothnia to Sundsvall, whence Stockholm was reached by train. At Karasjok, on the road between Bossekop and Enare, the travellers made the acquaintance of the same Lapp family that visited London last year, and much valuable information was collected on the Lapps, Quans, and northern Finns. This forms the subject of two communications sent by Stephen Sommier to the *Archivio per l'Antropologia e l'Etnologia* (xvi., 1, 1886), and separately printed under the title of "The Lapps and Northern Finns." The account of the trip to Cape North has also been issued in separate form by the Italian Geographical Society (Rome, 1886).

THE LUNAR SURFACE AND ITS TEMPERATURE

A MONOGRAPH by the writer, relating to the temperature of the lunar surface, read before the American Academy of Science, September 1869, contained the following:—"Are we not forced to dissent from Sir John Herschel's opinion that the heat of the moon's surface, when presented to the sun, much exceeds that of boiling water? Raised to such a high temperature, our satellite, with its feeble attraction, could not possibly be without an envelope of gases of some kind. Indeed, nothing but the assumption of extreme cold offers a satisfactory explanation of the absence of any gaseous envelope round a planetary body, which, on account of its near proximity, cannot vary very much from the earth as regards its composition. The supposition that this neighbouring body is devoid of water, dried up and sunburnt, will assuredly prove one of the greatest mistakes ever committed by physicists." This assertion was based on demonstrations showing that the circular walls of the great "ring mountains" on the lunar surface are not, as supposed, composed of "mineral substances originally in a state of fusion." The height and diameter of these walls being recorded in "Der Mond," computations based on the safe assumption that the areas of their transverse sections cannot be less than the square of their height, establishes the important fact that the contents of the wall of, for instance, Tycho, the circumference of which is 160 miles, height 2·94 miles, amounts to $2{\cdot}94 \times 160 = 1382$ cubic miles. The supposed transfer of this enormous mass, in a molten state, a distance of 25 miles from the central vent imagined by Nasmyth, and its exact circular distribution at the stated distance, besides its elevation to a vertical height of nearly 3 miles, involve, I need not point out, numerous physical impossibilities. Other materials and agencies than those supposed to have produced the "ring mountains" must consequently be sought in explanation of their formation. A rigid application of physical and mechanical principles to the solution of the problem proves conclusively that water subjected successively to the action of heat and cold has produced the circular walls of Tycho. The supposition that these stupendous mounds consist of volcanic materials must accordingly be rejected, and the assumption admitted that they are inert glaciers which have become as permanent as granite mountains by the action of perpetual intense cold.

Independently of the foregoing demonstration, the fallacy of the volcanic hypothesis will be comprehended by its advocates on learning that the quantity of lava requisite to form the circular walls of Tycho would cover the entire surface of England and Wales to a depth of 125 feet.[1]

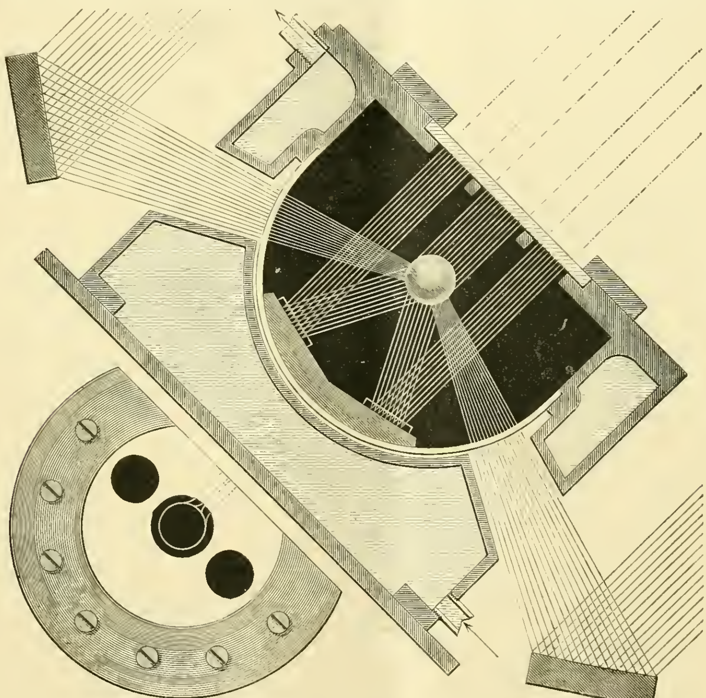
Before proceeding further with our demonstration it will be necessary to establish the maximum temperature which solar radiation is capable of imparting to the lunar surface. This temperature, of course, varies with the distance of the primary and its satellite from the sun. By means of an actinometer the bulb of whose thermometer receives an equal amount of radiant heat on opposite sides, I was enabled to determine with desirable accuracy, sixteen years ago, that, when the earth is in aphelion, solar radiation on the ecliptic imparts a maximum temperature of 67°·2 F., and that the retardation of the radiant energy occasioned by the want of perfect atmospheric diathermancy reaches 0·207. Consequently the temperature produced by solar radiation at the boundary of the terrestrial atmosphere is

$$67 \cdot 2 \times 1 \cdot 207 = 81^\circ \cdot 11 \text{ F.},$$

when the earth is in aphelion. Agreeably to observations during the winter solstice, compared with observations at midsummer, at equal zenith distance, the augmentation of solar intensity when the earth is in perihelion amounts to 5°·84 F.; hence the temperature produced by solar radiation reaches

$$81 \cdot 11 + 5 \cdot 84 = 86^\circ \cdot 95 \text{ F.},$$

when the rays enter our atmosphere during the winter solstice. It should be observed that on theoretical grounds the increase of temperature, when the earth is in perihelion, will be in the



Captain Ericsson's Pyrheliometer.

inverse ratio of the *dispersion of the solar rays*; hence, as the aphelion distance is to the perihelion distance as 218·1 to 210·9, it will be seen that the temperature produced by solar radiation when the earth is in perihelion will be

$$\frac{218 \cdot 1^2 \times 67^\circ \cdot 2}{210 \cdot 9^2} = 71^\circ \cdot 86 \text{ F.}$$

Adding 0·207 for retardation caused by imperfect atmospheric diathermancy, solar intensity during the winter solstice will be

$$71 \cdot 86 \times 1 \cdot 207 = 86^\circ \cdot 73 \text{ F.}$$

Calculation based on *observation*, as before stated, proves that the perihelion temperature is 86°·95, thus showing a trifling discrepancy between theory and observation.

[1] Area of England and Wales, 58,320 square miles; contents of the walls of Tycho, 1382 cubic miles; hence $\frac{1382}{58320} \times 5280 = 125 \cdot 12$ feet.

Adopting 86°·73 as correct, it will be found that the yearly *mean* temperature produced by solar radiation when the rays enter the earth's atmosphere will be

$$\frac{81^\circ \cdot 11 + 86^\circ \cdot 73}{2} = 83^\circ \cdot 92 \text{ F.},$$

while the temperature produced by the sun's radiant heat is only 81°·11 during the summer solstice, as before shown. Hence the temperature of the lunar surface when presented to the sun while the earth is furthest from the luminary can only be augmented 81°·11 F.

The remarkable fact that the moderate heat produced by solar radiation is capable of increasing the temperature of bodies previously heated to a high degree demands consideration in connection with the subject under investigation; also the nature of the device, before referred to, for ascertaining the temperature produced by solar radiation. The accompanying illustration

represents a combination of said device and a pyrheliometer differing materially from Pouillet's instrument, by showing the true intensity of the "fire" in the sun's rays.

The illustration presents a top view and a vertical section of the new instrument through the centre line. The upper part, composed of bronze, is cylindrical with a flat top, the bottom being semispherical, composed of ordinary glass. The top of the cylindrical chamber is provided with three circular perforations covered by a thin crystal carefully ground and polished. A thermometer having a spherical bulb is introduced through the side of the chamber, the bulb being central to the transparent semispherical bottom. A short parabolic reflector, shown in section on the illustration, surrounds the instrument, adjusted so that its focus coincides with the centre of the bulb of the thermometer. The compound cylindrical and spherical chamber is inclosed in a vessel containing water, appropriate openings at top and bottom being provided for maintaining constant circulation during experiments. Efficient means are also provided for exhausting the air from the internal chamber. The instrument is secured to the top of a substantial table which, during experiments, faces the sun at right angles by the intervention of a parallactic mechanism. Movable shades are applied, by means of which the sun's rays may be quickly cut off from, or admitted to, the parabolic reflector; while other shades enable the operator to admit or exclude the solar rays from the circular perforations at the top of the exhausted chamber. It will be readily understood that the parallel lines within the exhausted chamber, shown on the illustration, indicate the course of the solar rays passing through the crystal and the perforations at the top, while the converging radial lines indicate the rays reflected by the parabolic reflector. The upper hemisphere of the thermometric bulb, it will be seen, receives the radiant energy of the sun's rays which pass through the large central perforation; while the lower half of the bulb will be acted upon by the rays passing through the small perforations. These rays are reflected upwards by two inclined circular mirrors attached to the bottom of the exhausted chamber. It should be particularly observed that the areas of these inclined mirrors *together* should exceed the area of the great circle of the bulb of the thermometer sufficiently to make good the loss of radiant energy caused by the imperfect reflection of the said mirrors, and also to make good the loss attending the passage of the solar rays through the crystal. A capacious water cistern, connected by flexible tubes with the external casing of the pyrheliometer, enables the operator to maintain the exhausted chamber at any desirable temperature. Engineers of great experience in the application of heat for the production of motive power and other purposes deny that the temperature of a body can be increased by the application of heat of a lower degree than that of the body whose temperature we desire to augment. The soundness of their reasoning is apparently incontrovertible, yet the temperature of the mercury in the instrument just described raised to 600° F. by means of the parabolic reflector, increases at once when solar heat is admitted through the circular apertures, although the sun's radiant intensity at the time may not reach one-tenth of the stated temperature. It should be mentioned that the trial of this new pyrheliometer has not been concluded, owing to very unfavourable atmospheric conditions since its completion. For our present purpose the great fact established by the illustrated instrument is sufficient, namely that the previous temperature of a body exposed to the sun's radiant heat is immaterial. The augmentation of temperature resulting from exposure to the sun, the pyrheliometer shows, depends upon the intensity of the sun's rays.

Regarding the temperature prevailing during the lunar night, its exact degree is not of vital importance in establishing the glacial hypothesis, since the periodical increment of temperature produced by solar radiation is only a fraction of the permanent loss attending the continuous radiation against space resulting from the absence of a lunar atmosphere; besides, all physicists admit that it is extremely low. Sir John Herschel says of the night temperature of the moon that it is "the keenest severity of frost, far exceeding that of our Polar winters." Proctor says: "A cold far exceeding the intensest ever produced in terrestrial experiments must exist over the whole of the unilluminated hemisphere." The author of "Outlines of Astronomy" has also shown that the temperature of space, against which the moon at all times radiates, is -151° C. ($-239^\circ\!\cdot\!8$ F.), Pouillet's estimate being -142 C. ($-223^\circ\!\cdot\!6$ F.). Adopting the latter degree, and allowing $81^\circ\!\cdot\!1$ for the sun's radiant heat, we establish the fact that the temperature of the lunar surface presented to the sun will be $223^\circ\!\cdot\!6$ less $81^\circ\!\cdot\!1$, or -142° 5 F., when the earth is in aphelion. It will be well to bear in mind that when the earth is in the said position, the sun's rays acting on the moon subtend an angle of 31′ 32″, hence the loss of heat by radiation against space will be diminished only 0·000021 during sunshine. Nor should Herschel's investigation be lost sight of, showing that stellar heat bears the same proportion to solar heat as stellar light to solar light. Stellar heat being thus practically inappreciable, the temperature produced by stellar radiation cannot be far from absolute zero—an assumption in harmony with the views of those who have studied the subject of stellar radiation, and consequently regard Pouillet's and Herschel's estimate of the temperature of space as being much too high.

Having disposed of the question of temperature, let us return to the practical consideration of the glacial hypothesis. The formation of annular glaciers by the joint agency of water and the internal heat of a planetary body devoid of an atmosphere and subjected to extreme cold is readily explained on physical principles. Suppose a sheet of water, or pond, on the moon's surface, covering the same area as the plateau of Tycho, viz. 50 miles diameter and 1960 square miles. Suppose, also, that the internal heat of the moon is capable of maintaining a moderate steam pressure, say 2 lbs. to the square inch, at the surface of the water in the pond. The attraction of the lunar mass being only one sixth of terrestrial attraction, while the moon's surface is freed from any atmospheric pressure, it will be evident that under the foregoing conditions a very powerful ebullition and rapid evaporation will take place, and that a dense column of vapour will rise to a considerable height above the boiling water. It will also be evident that the expansive force within this column at the surface of the water will be so powerful at the stated pressure that the vapour will be forced beyond the confines of the pond in all directions with great velocity. No vertical current, it should be understood, will be produced, since the altitude of the column, after having adjusted itself to the pressure corresponding with the surface temperature of the water, remains stationary, excepting the movement consequent on condensation from above. The particles of vapour forced beyond the confines of the pond, on being exposed to the surrounding cold, caused by unobstructed radiation against space, will of course crystallise rapidly, and in the form of snow fall in equal quantity round the pond, and thereby build up an annular glacier. As the radius of the vaporous column exceeds 25 miles, it will be perceived that, notwithstanding the rapid outward movement, before referred to, some of the snow formed by the vapours rising from the boiling pond will fall into the same, to be melted and re-evaporated.

In connection with the foregoing explanation of the formation of annular glaciers, their exact circular form demands special consideration. An examination of Rutherford's large photograph of the lunar surface shows that, apart from the circular form of the walls, the bottoms of the depressions are in numerous cases smooth, rising slightly towards the centre uniformly all round. The precision observable proves clearly the action of formative power of great magnitude. Referring to what has already been explained regarding the vaporous column of 25 miles radius, calculation shows that a surface temperature exerting the moderate pressure of 2 lbs. to the square inch will produce an amount of mechanical energy almost incalculable. Practical engineers are aware that the steam rising from a surface of water 10 square feet, heated by a very slow fire, is capable of producing an energy of 1 horse-power; consequently a single square mile of the boiling pond will develop 2,780,000 horse-power. This prodigious energy will obviously be exerted *horizontally*, as the weight of the superincumbent column of vapour balances its *expansive* force precisely as the weight of our atmosphere balances its expansive force. But unlike the earth's atmosphere, which is restrained from horizontal movement by its continuance round the globe, the vapour of the column of 50 miles diameter is free to move beyond the confines of the pond. A very powerful horizontal motion, especially of the lower part of the vaporous mass, will thus be promoted, acting in radial lines from the centre, the principal resistance encountered being the friction against the water. Considering that the friction against the surface of the ocean, caused by the gentle trade-wind, is sufficient to produce the Gulf Stream, we need no figures to show the effect on the water in the boiling pond produced by the vaporous mass propelled by an energy of 2 lbs. to the square inch, in radial lines towards its confines. A circular tidal wave of extraordinary power, together with a return under-current towards the centre, will obviously be the result. But agreeably to the laws supposed to govern vortex

motion, these currents cannot be maintained in a radial direction. A rotary motion, rapidly augmenting, will take place, producing a vortex more powerful than any imagined by Descartes. The radial currents of the vaporous column having assumed a spiral course, will rapidly acquire a velocity exceed that of a cyclone. The practical effect of the powerful movement of the vortex, it is reasonable to suppose, will resemble that of a gigantic carving-tool whose thorough efficiency in removing irregularities has been proved by the exact circular outline presented by thousands of lunar formations. The terraces within the "ring mountains" indicated on Beer and Mädler's chart, it may be shown, were produced by evaporation resulting from low temperature and reduced energy after the formation of the main glacier.

There is another feature in the lunar landscape scarcely less remarkable than its circular walls and depressions. In the centre of nearly all of the latter one or more conical hills rise, in some cases several thousand feet high. Has the rotary motion of the boiling vortex any connection with these central cones? A brief explanation will show that the connection is quite intimate. The under-rated estimate that 10 square feet of surface under the action of slow fire is capable of developing one horse-power proves the presence of a dynamic energy exceeding 5,000,000,000 of horse-power at the base of the vaporous column resting on the boiling water of a pond as large as that of Tycho. No part of this power can be exerted vertically, as already explained, on the ground that the weight of the vapour restrains such movement. The great velocity of the vortex resulting from the expenditure of the stated amount of dynamic energy will of course produce corresponding centrifugal force; hence a maelström will be formed capable of draining the central part of the pond, leaving the same dry, unless the water be very deep, in which case the appearance of a dry bottom will be postponed until a certain quantity of water has been transferred to the glacier. It should be observed that the central part of the bottom, freed from water, will also be freed from the surrounding cold by the protection afforded by the vaporous mass. The quantity of snow formed above the centre, at great altitude, will be small, and of course diverged during the fall. Evidently the dry central part, prevented, as shown, from cooling, will soon acquire a high temperature, admitting the formation of a vent for the expulsion of lava, called for as the moon, whose entire dry surface is radiating against space, shrinks rapidly under the forced refrigeration attending glacier-formation. Lava-cones similar to those of terrestrial volcanoes, and central to the circular walls, may thus be formed, the process being favoured by the feebleness of the moon's attraction. The existence of warm springs on the protected central plains is very probable; hence the formation of cones of ice might take place during the last stages of glacier-formation, when those plains no longer receive adequate protection against cold.

In accordance with the views expressed in the monograph read before the American Academy of Science, continued research has confirmed my supposition that the water on the moon bears the same proportion to its mass as the water of the oceans to the terrestrial mass I have consequently calculated the contents of the circular walls of the "ring mountains" measured and delineated by Beer and Mädler, and find that these walls contain 630,000 cubic miles. The opposite hemisphere of the moon being subjected to similar vicissitudes of heat and cold as the one presented to the earth, the contents of the circular walls not seen cannot vary very much from those recorded in "Der Mond"; hence the total will amount to 1,260,000 cubic miles. Allowing for the difference of specific gravity of ice, the stated amount represents 1,159,000 cubic miles of water. But "Der Mond" does not record any of the minor circular walls which, as shown by the large photograph before referred to, cover the entire surface of some parts of the moon. On careful comparison it will be found that the contents of the omitted circular formations is so great that an addition of 50 per cent. to the before-stated amount is called for. An addition of 25 per cent. for the ice-fields, whose extent is indicated by cracks and optical phenomena, is likewise proper. The sum total of water on the moon, therefore, amounts to 2,028,600 cubic miles.

Adopting Herschel's estimate of the moon's comparative mass, viz. 0·011364, and assuming that the oceans of the earth cover 130,000,000 square miles, it will be seen that the estimated quantity of water on the moon corresponds with a mean depth of 7250 feet of the terrestrial oceans.[1] This depth agrees very nearly with the oceanic mean depth established by the soundings for the original Atlantic cable, viz. 7500 feet; but the result of the *Challenger* Expedition points to a much greater depth. This circumstance is by no means conclusive against the supposition that the satellite and the primary are covered with water in relatively equal quantities. The correctness of Sir John Herschel's demonstration proving the tendency of the water on the lunar surface to flow to the hemisphere furthest from the earth must be disproved before we reject the assumption that the quantity of water on the surface of the moon bears the same proportion to its mass as the quantity of water on the earth to the terrestrial mass.

JOHN ERICSSON

[1] $\frac{2028600 \times 5280}{130000000 \times 0·011364} = 7250$ feet mean depth of terrestrial oceans corresponding with water on the moon.

SCIENTIFIC SERIALS

Rendiconti del Reale Istituto Lombardo, May 27.—Determination of the heat of fusion in the alloys of lead, tin, bismuth, and zinc, by Prof. D. Mazzotto. By the cooling process usually adopted for determining the specific heat of liquids, the author finds the point of fusion and the heat of fusion for these various chemical alloys as under:—

	Point of fusion	Heat of fusion
Tin and lead	181°	10°29
Tin and zinc	196	16·20
Tin and bismuth	138	11·065
Bismuth and lead	126	4·744

Two of these coincide and two others differ little from the composition of the chemical alloys as given by Rudberg.—Education and crime in Italy, by S. Amato Amati. In order to ascertain the influence of public instruction on the criminal classes in the Peninsula, the author has compiled a number of comparative tables based on official returns ranging from the year 1871 to 1883 inclusive. For the last three years of this period the results are as under:—

	Criminals	Unlettered	Could read and write	Educated
1881	8693	5511	3031	151
1882	7009	4139	2671	199
1883	6490	3741	2596	153

According to the three last census returns the total percentage of unlettered was as under:—

	Males	Females	Total
1861	65·47	81 52	73·50
1871	60·16	77·18	68·64
1881	53·89	72·93	63·45

—Meteorological observations made at the Brera Observatory, Milan, during the month of May.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 6.—"Further Discussion of the Sun-spot Spectra Observations made at Kensington." By J. Norman Lockyer. Communicated to the Royal Society by the Solar Physics Committee.

I have recently discussed, in a preliminary manner, the lines of several of the chemical elements most widened in the 700 spots observed at Kensington.

The period of observation commences November 1879, and extends to August 1885. It includes, therefore, the sunspot curve from a minimum to a maximum and some distance beyond.

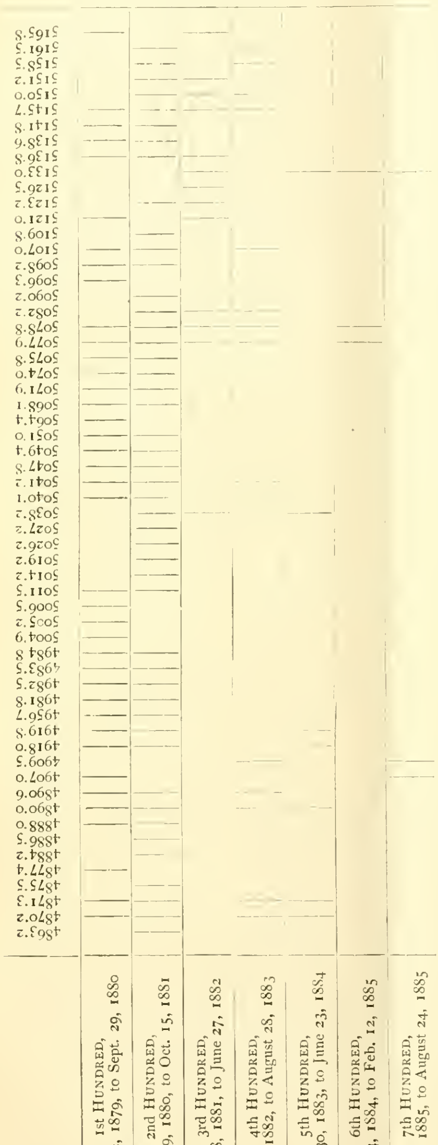
It is perhaps desirable that I should here state the way in which the observations have been made. The work, which has been chiefly done by Messrs. Lawrance and Greening, simply consists of a survey of the two regions F—*b* and *b*—D.

The most widened line in each region—not the widest line, but the *most widened*, is first noted; its wave-length being given in the observation books from Angström's map. Next, the lines which most nearly approach the first one in widening are recorded, and so on till the positions of six lines have been noted, the wave-lengths being given from Ångström's map, for each region.

It is to be observed that these observations are made without any reference whatever to the origin of the lines; that is to say it is no part of the observer's work to see whether there are metallic coincidences or not; this point has only been inquired into in the present reductions, that is, seven months after the

TABLE A.—IRON

Iron Lines observed in Sunspot Spectra at Kensington among the most Widened Lines



last observations now discussed were made. In this way perfect absence of all bias is secured.

It may further be remarked that the number of lines widened throughout a sunspot period is about the same, so that the conditions of observation vary very little from month to month, and from year to year.

It may be further remarked that the absolute uniformity of the results obtained in the case of each of the chemical elements investigated indicates, I think, that the observations have been thoroughly well made; and, as a matter of fact, they are not difficult.

I first give tables (A, B, C) showing that for each of the

TABLE B.—NICKEL

List of most Widened Lines observed at Kensington

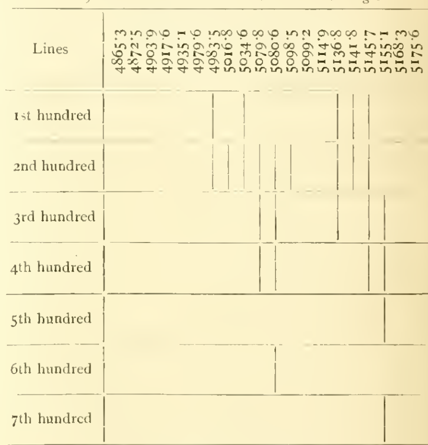
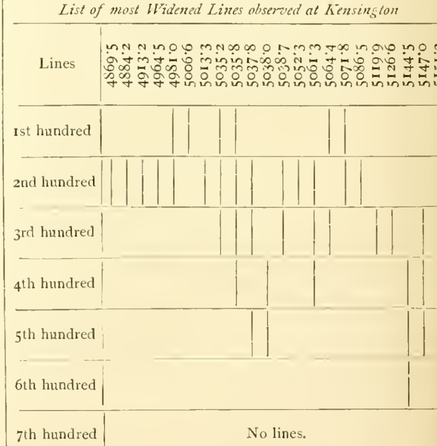


TABLE C.—TITANIUM

List of most Widened Lines observed at Kensington



chemical elements taken—iron, nickel, and titanium—the num- of lines seen in the aggregate in each hundred observations is reduced from minimum to maximum, and that this result holds good for both regions of the spectrum.

I next give another table (D) showing that during the obser-

TABLE D.—*Unknown Widened Lines observed at Kensington*

	1st hundred	2nd hundred	3rd hundred	4th hundred	5th hundred	6th hundred	7th hundred
4865	.	..	...	..	...	1	...
4885	...	..	...	...	...	...	1
4888·3	1	...	...	...	...	...	...
4891·8	..	...	...	...	1	...	...
4910	...	...	...	2	...	...	...
4944	..	...	1	...	...	...	...
5017·2	..	1	...	...	...	...	...
5028·9	..	1	...	...	...	..	...
5030		1	...	...	...	...	...
5034·8	11	...	3	...	...	...	..
5037		...	...	...	...	1	...
5038·9	...	...	1	1	...	...	...
5042	.	..	3	...	...	...	...
5042·3	...	...	4	..	...	...	...
5043	..	1	...	..	..	...	...
5044·6	.	3	...		..	...	...
5061	..	..	2	..	...	...	3
5061·5	..	...	...	..	...	...	2
5062	..	...	..	..	.	...	5
5062·4		...	...	...	..	2	...
5062·8		...	...	3		2	...
5065		...	8	...		...	...
5067		1	...	...		...	...
5069·5	..	1	...	.	.	...	...
5070·8	...	1	...	...		...	...
5077	...	...	.	1	.	...	...
5079·5	...	...	...	..	...	2	...
5080	...	...	...	1		...	...
5081·5	..	...	...	...	.	...	3
5082	.	...	...	...	..	2	..
5083	...	...	...	...		2	...
5083·3	..	1	2	3	...	.	...
5084	.	1	...	...	...	...	3
5084·5	..	...	..	...	...	...	2
5086	..	...	17	..	1	...	...
5086·8	..	1	...	..	..	...	...
5087·7	...	1	...	...	..	..	...
5088·1	...	1	...	...	..	...	..
5088·6	...	1	...	...	...	...	...
5089·0	...	...	...	1	...	...	...
5101	...	...	...	...	..	...	1
5103·5	...	...	...	...	1	..	.
5112·1	...	6	22	4	2	1	..
5115·5	...	...	...	...	..	...	9
5116	3	6	24	3	.	.	...
5116·2	..	...	7	...		...	..
5118	4	...	14	...		..	...
5127	...	...	...	1	.	..	...
5127·5	...	...	1	...	.	.	.
5128·8	...	...	...	...	..	...	1
5129·6	...	17	19	4	...	...	..
5130	...	...	1	..	...	...	...
5132	...	14	21	6	...	...	...
5132·5	...	...	1	...	...	...	...
5132·8	...	...	...	3	...	...	...
5133·5	...	...	1	...	1	3	17
5133·8	...	30	47	43	62	3	27
5134	...	..	...	...	12	41	10
5134·4	...	...	...	...	...	19	...
5135	...	...	...	...	16	36	11
5135·5	...	33	15	...	53	36	20
5135·8	...	...	37	52	13	2	...
5136	...	...	4	...	9	22	27
5136·5	...	...	3	1	...	...	...
5137	...	...	2	2	1	...	...
5137·5	...	...	4	...	72	79	22
5137·8	...	12	35	64	13	10	3
5138	...	...	..	...	1	...	3
5139	...	...	...	...	1	..	1
5139·4	...	1	2	3	...	...	...
5140·4	...	2	...	...	..	...	...
5142·2	13	4	...	1	.	...	...
5142·8	...	21	7	19	2	...	...
5143	...	..	..	...	...	...	20
5143·2	...	..	2	...	...	...	...
5144·2	1	3	..	2	..	...	..
5144·5	..	..	.	.	1	...	..
5145·5	.	..		.	...	1	...
5146	..	..	6	12	.	...	.
5146·5	.	.		2	...	...	.
5148	..	.		...	...	...	1
5148·8	.	..	1	2	..	...	...
5149	2	32	31	36	4	...	35
5149·2	..	...	.	1	...	...	..
5149·5	.	...	.	4	...	...	29
5149·8	...	8	2	8	..	8	...
5150	...	...	.	1	..	...	.
5151·8	...	...	..		1	...	...
5153·8	...	...	..	..	1	...	..
5154	..	...		.	..	...	1
5155·4	...	...		.	1	...	...
5156	1	12	37	74	82	91	95
5156·5	...	...	...	.	8	...	...
5157·2	...	...	...	4	...	...	...
5159	..	...	1	8	13	11	41
5159·5	1	...	31	59	80	86	57
5160	...	...	1	4	...	9	...
5160·4	...	1	...	5	..	...	4
5162	...	...	9	7	61	67	62
5162·2	1	...	23	49	21	30	..
5175	...	...	...	...	...	...	3

vations the lines recorded as most widened near the maximum have not been recorded amongst metallic lines by either Ångström or Thalèn, and that many of them are not among the mapped Fraunhofer lines, though some of them may exist as faint lines in the solar spectrum when the observing conditions are best.

The reduction of the latitudes of the spots is not yet completed.

The result of these observations may be thus briefly stated. As we pass from minimum to maximum, the lines of the chemical elements gradually disappear from among those most widened, their places being taken by lines of which at present we have no terrestrial representatives. Or, to put the result another way—at the minimum period of sunspots when we know the solar atmosphere is quietest and coolest, vapours containing the lines of some of our terrestrial elements are present in sunspots. The vapours, however, which produce the phenomena of sunspots at the sunspot maximum are entirely unfamiliar to us.

The disappearance of the lines of iron, nickel, and titanium, and the appearance of unknown lines as the maximum is reached, is shown by curves in Fig. 1.

The results, in my opinion, amply justify the working hypothesis as to the construction of the solar atmosphere which I published some years ago (*Proc.* Roy. Soc., 1882, p. 291). In the region of the spectrum comprised between 4860 and 5160, I find in the case of iron, to take an instance, that sixty lines were distributed unequally among the spots in 1879 and 1880, many iron lines being visible in every spot. In the last observations, about the maximum, only three iron lines in all are seen among the most widened lines. These three lines also have been visible in four spots only out of the last hundred. The same thing happens with titanium and nickel, and with all the substances for which the reductions are finished.

I am quite content, therefore, to believe that iron, titanium, nickel, and the other substances very nearly as complex as we know them here, descend to the surface of the photosphere, in the downrush that forms a spot at the period of minimum, but that at the maximum, on the contrary, only their finest constituent atoms can reach it. It may also be remarked that these particles which survive the dissociating energies of the lower strata are not the same particles among the constituents of the chemical elements named which give the chromospheric lines recorded by Tacchini, Riccò, and myself.

Having thus found the working hypothesis to which I have referred stand the severe test which the sunspot observations apply to it, I have gone further, and have endeavoured to extend it in two directions.

First. I found that the view to which the hypothesis directly leads, that the metallic prominences are produced by violent explosions due to sudden expansions among the cooler matters brought down to form the spots, when they reach the higher temperature at and below the photosphere level, includes all the facts I know touching spot and prominence formation. Thus, for instance, the close connection between metallic prominences and spots; the entire absence of metallic prominences with rapid motion from any but the spot-zones; the fact that the faculæ always follow the formation of a spot and never precede it; that the faculous matter lags behind the spot as a rule; the existence of veiled spots and minor prominences in regions outside the spot-zones; the general injection of unknown substances into the lower levels of the chromosphere which I first observed in 1871, and which have been regularly recorded by the Italian observers since that time—all these phenomena and many others which may be referred to at length on another occasion, are demanded by the hypothesis, and are simply and sufficiently explained by it.

With regard to the extensions of volume to which I have re-

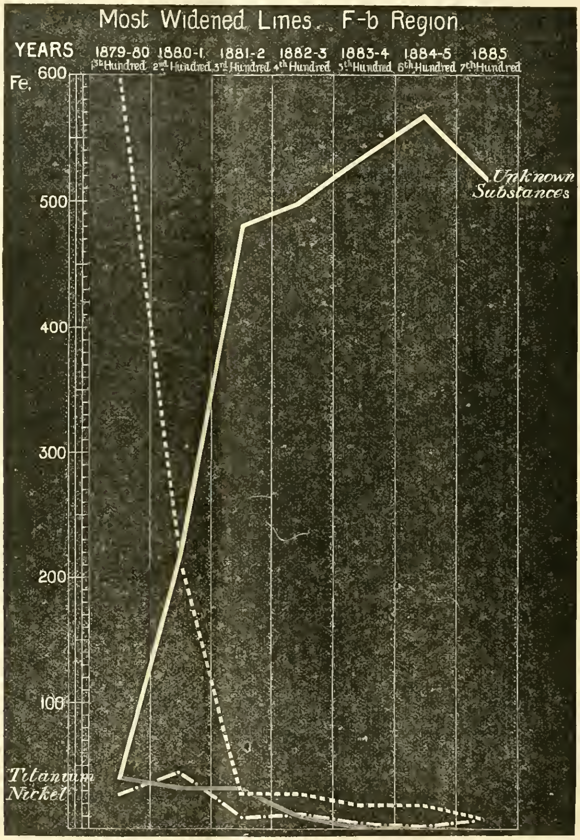


Fig. 1.—Number of appearances of known aod unkoown lines.

ferred, I find that if we assume that metallic iron can exist in any part of the sun's atmosphere, and that it falls to the photosphere to produce a spot, the vapour produced by the fall of 1,000,000 tons will give us the following volumes:—

Temperature		Pressure		Volume in cubic miles
2,000° C.	...	380 mm.	...	0·8
10,000	...	760 ,,	...	1·8
20,000	...	5 atmos.	...	0·7
50,000	...	760 mm.	...	8·8
50,000	...	190 ,,	...	35·2

If we assume the molecule of iron to be dissociated ten times by successive halving, then the volume occupied will be 1024 times greater, and we shall have—

Temperature		Pressure		Volume in cubic miles
50,000° C.	...	760 mm.	...	9,011
50,000	...	190 ,,	...	36,044

In these higher figures we certainly do seem nearer the scale on which we know solar phenomena to take place; the tremendous rending of the photosphere, upward velocities of 250 miles a second, and even higher horizontal velocities according to

Peters, are much more in harmony with the figures in the second table than the first.

I may mention, in connection with this part of the subject, that the view of the great mobility of the photosphere which this hypothesis demands, so soon as we regard metallic prominences as direct effects of the fall of spot material, is further justified by the fact that, if we assume the solar atmosphere, that is the part of the sun outside the photosphere, to be about 500,000 miles high, which I regard as a moderate estimate, the real average density of the sun is very nearly equal to one-tenth that of water, instead of being slightly greater than that of water, as stated in the text-books.[1]

We can then only regard the photosphere as a cloudy stratum existing in a region of not very high pressure. It is spherical because it depends upon equal temperatures.

The second direction in which I have attempted to develop the hypothesis has relation to the circulation in the sun's atmosphere. I have taken the facts of the solar atmosphere as a whole, as they are recorded for us in the various photographs taken during eclipses since 1871, and also in drawings made before that time, the drawings being read in the light afforded by these photographs.

I find that the working hypothesis at once suggests to us that the sunspot period is a direct effect of the atmospheric circulation, and that the latitudes at which the spots commence to form at the minimum, which they occupy chiefly at the maximum, and at which they die out at the end of one period in one hemisphere, probably at the moment they commence to form a second one in the other (as happened in 1878–79), are a direct result of the local heating produced by the fall of matter from above descending to the photosphere, and perhaps piercing it. The results of this piercing are the liberation of heat from below and various explosive effects due to increase of volume, which, acting along the line of least resistance, give, as a return current, incandescent vapours ascending at a rate which may be taken as a maximum of 250 miles a second, a velocity sufficient to carry them to very considerable heights.

The view of the solar circulation at which I have arrived may be briefly stated as follows :—

There are upper outflows from the poles towards the equatorial regions. In these outflows a particle constantly travels, so that its latitude decreases and its height increases, so that the true solar atmosphere resembles the flattened globe in Plateau's experiment (see photographs, 1878, and Fig. 3).

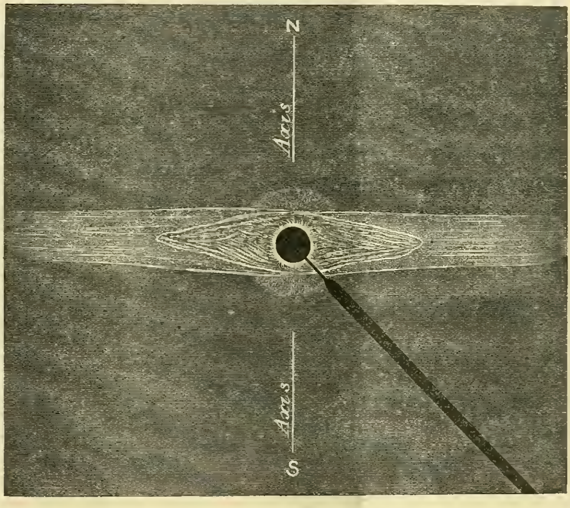


FIG. 2.—Minimum. Tracing of Newcomb's observation of 1878, the brighter portion of corona being hidden by a screen. Shows the equatorial extension and concentric atmospheres.

These currents, as they exist in the higher regions of the atmosphere, carry and gather the condensing and condensed materials till at last they meet over the equator.

There is evidence to show that they probably extend as solar meteoric masses far beyond the limits of the true atmosphere, and form a ring, the section of which widens towards the sun, and the base of which lies well within the boundary of the atmosphere (Fig. 2).

If we assume such a ring under absolutely stable conditions, there will be no disturbance, no fall of material, therefore there will be no spots, and therefore again there will be no prominences. Such was the state of things on the southern surface of the ring from December 1877 to April 1879, during which period there was not a single spot observed the umbra of which was over 15-millionths of the sun's visible hemisphere.

Assume a disturbance. This may arise from collisions, and these collisions would be most likely to happen among the particles where the surface of the ring meets the current from the poles. These particles will fall towards the sun, thereby disturbing and arresting the motion of other particles nearer the photosphere, and finally they will descend with a crash on to the photosphere, from that point where the surface of the ring enters the atmosphere some distance further down.

The American photographs in 1878 supply us with ample evidence that this will be somewhere about lat. 30°, and here alone will the first spots be formed for two reasons.

(1) In the central plane of the ring over the equator, the particles will be more numerous ; a rapid descent, therefore, in this central plane will be impossible, for the reason that the condensed matter has to fall perhaps a million of miles through strata of increasing temperature ; there will, therefore, be no spots ; and practically speaking, as is known, there are no spots at the equator, though there are many small spots without umbræ between latitudes 3° and 6° N. and S.

Above lat. 30°, as a rule, we have no spots, because there is no ring, and further the atmosphere is of lower elevation, so that there is not sufficient height of fall to give the velocities require to bring down the material in the solid form.

The lower corona, where the corona is high, and it is highest over the equator, acts as a shield or buffer ; volatilisation and

[1] The density referred to water = 1·444 and to the earth 0·255, according to Newcomb.

dissociation take place at higher levels. Where this occurs, spots are replaced by a gentle rain of fine particles slowly descending, instead of the fall of mighty masses and large quantities of solid and liquid material.

Volatilisation will take place gradually during the descent, and at the utmost only a veiled spot will be produced.

We know that when the solar forces are weak, such a descent is taking place all over the sun, because at that time the spectrum of the corona, instead of being chiefly that of hydrogen, is one of a most complex nature—so complex that before 1882 it was regarded by everybody as a pure continuous spectrum, such as is given by the limelight.

The moment the fall of spot material begins we get the return current in the shape of active metallic prominences, and the production of cones and horns which probably represent the highest states of incandescence over large areas and extending to great heights; and, besides these, the production of streamers (see Fig. 4).

Two results follow:—

(1) In consequence of the increased temperature of the lower regions, the velocity of the lower currents towards the poles, and therefore of the upper currents from the poles, is enormously increased. The disturbance of the ring will therefore be increased.

(2) Violent uprushes of the heated photospheric gases, mounting with an initial velocity of a million miles an hour, can also disturb the ring directly.

In this way the sudden rise to maximum in the sunspot curve, and the lowering of the latitude of the spots, follow as a matter of course. And the part of the ring nearest the sun, its base, so to speak, is, it would appear, thrown out of all shape, and we get falls over broad belts of latitude N. and S.

Does this hypothesis explain, then, the slow descent to minimum and the still decreasing latitude? It does more, it demands it. For now the atmosphere over those regions where the spots have hitherto been formed is so highly heated and its height is so increased, that any disturbed material descending through it will be volatilised before it can reach the photosphere.

The best chance that descending particles have now to form spots is if they fall from points in lower latitudes. The final period, therefore, of the sunspot curve must be restricted to a very large extent to latitudes very near the equator, and this is the fact also, as is well known.

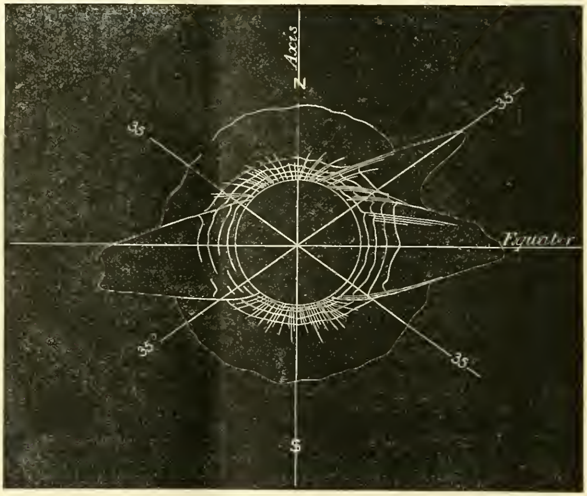


Fig. 3.—Minimum. Tracing of the results obtained by the cameras in 1878, showing inner portion of equatorial extension, and how the surfaces of it cut the concentric atmosphere in lat. 35° N. and S., or thereabouts.

It will be seen that on this view, as the brightness, and therefore the temperature, of the atmosphere, as we know, increases very considerably from minimum to maximum, the masses which can survive this temperature must fall from gradually increasing heights.

It may be pointed out how perfectly this hypothesis explains the chemical facts observed and associates them with those gathered in other fields of inquiry.

At the minimum the ring is nearest the sun, the subjacent atmosphere is low and relatively cool.

Particles falling from the ring, therefore, although they fall in smaller quantity because the disturbance is small, have the best chance of reaching the photosphere in the same condition as they leave the ring, hence at this time the widening in many familiar lines of iron, nickel, titanium, &c.

The gradual disappearance of these lines from the period of minimum to that of maximum is simply and sufficiently explained by the view that the spot-forming materials fall through gradually increasing depths of an atmosphere which at the same time is having its temperature as gradually increased by the result of the action I have before indicated, until finally, when the maximum is reached, if we assume dissociation to take place at a higher level at the maximum, dissociation will take place before the vapours reach the photosphere, and the lines which we know in our laboratories will cease to be visible.

This is exactly what takes place, and this result can be connected, as I have stated elsewhere, with another of a different kind. This hypothetical increasing height of fall demanded by the chemistry of the spots is accompanied by a known acceleration of spot movement over the sun's disk, as we lower the latitude—which can only be explained, so far as I can see, by a gradually increasing height of fall as the equator is approached.

There are two other points. (1) The sunspot curve teaches us that the slowing down of the solar activities at the maximum is very gradual. We should expect, therefore, the chemical conditions at the maximum to be maintained for some time afterwards. As a matter of fact, they have been maintained till March of the present year, and only now is a change taking place which shows us chemically that we are leaving the maximum conditions behind. (2) The disappearance of the lines of the metallic elements at maximum is so intimately connected with an enormous increase in the indications of the presence of

hydrogen that there is little doubt that we are in the presence of cause and effect. The hydrogen, I am now prepared to believe, is a direct consequence of the dissociation of the metallic elements.

It will be convenient to refer here to the facts which have been recorded during those eclipses which have been observed at the sunspot minimum and maximum.

At the minimum the corona is dim; observations made during the minimum of 1878 showed that it was only one-seventh as bright as the corona at the preceding maximum. There are no bright lines in its spectrum, and both photographic and eye-observations proved it to consist mainly of a ring round the equator, gradually tapering towards its outer edge, which some observations placed at a distance of twelve diameters of the sun from the sun's centre.

The same extension was observed in the previous minimum in 1867, and the polar phenomena were observed to be identical in both eclipses. At the poles there is an exquisite tracery curved in opposite directions, consisting of plumes or *panaches*, which bend gently and symmetrically from the axis, getting more and more inclined to it, so that those in latitudes 80° to 70° start nearly at right angles to the axis, and their upper portions droop gracefully, and curve over into lower latitudes.

Although indications of the existence of this ring have not been recorded during eclipses which have happened at the period of maximum, there was distinct evidence both in the eclipses in 1871 and 1875 of the existence of what I regard as the indications of outward upper polar currents observed at minimum.

The fact that the solar poles were closed at the maximum of 1882, while they were open in 1871, is one of the arguments which may be urged that at times the whole spot-zones are surmounted by streamers, with their bases lying in all longitudes along the zones.

It was probably the considerable extension of these streamers earthwards, in 1882, which hid the finer special details at the poles, while in 1871 the part of the sun turned towards the earth was not rich in streamers of sufficient extension.

Touching these streamers, it is an important fact to be borne in mind, that no spots ever form on the poleward side of them.

It is obvious, therefore, that spots are not produced by the condensation of materials on their upper surfaces, for in that case the spots would be produced indifferently on either side of them, and the width of the spot-zones would be inordinately increased.

Although in the foregoing I have laid stress upon the indications afforded by the observations of 1878 of the existence of a ring, it should be remarked that, so far, the eclipse appearances

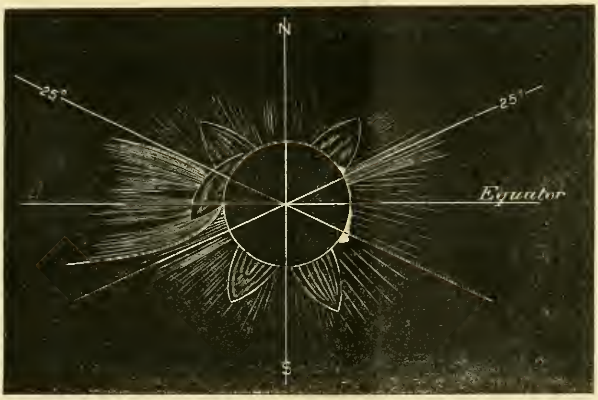


FIG. 4.—$1\frac{1}{2}$ years from maximum, 1858. Tracing of drawing by Liais, showing "cones."

on which the idea rests have not been observed at maximum. This, however, is not a fatal objection, because precautions for shielding the eye were necessary even in 1878 when the corona was dim; and if it is composed merely of cooled material it would not readily be photographed.

In may be urged by some that the phenomena observed in 1878 may only after all have been equatorial streamers.

It is obvious, therefore, that this point deserves the closest attention during future eclipses, until it is settled one way or the other.

Geological Society, June 23.—Prof. J. W. Judd, F.R.S., President, in the chair.—The President announced that he had received from Prof. Barrois an intimation that the Geological Society of France would hold a special country meeting in the district of Finistère from the 19th to the 28th of August next, during which a variety of interesting excursions would be made under the guidance of MM. Barrois, Davy, and Lebesconte. Prof. Barrois, in writing, expressed the pleasure which it would give the members of the Geological Society of France if they were joined by some of their English *confrères*, but at the same time stated that as the accommodation for travellers was limited in the district, he would be glad to have timely notice from any one intending to take part in the meeting. Particulars were to be obtained from the Assistant Secretary, who would also communicate with Prof. Barrois.—The following communications were read:—On some perched blocks and associated phenomena, by Prof. T. McKenny Hughes, M.A., F.G.S. The author described certain groups of boulders which occurred on pedestals of limestone rising from 3 to 18 inches above the level of the surrounding rock. The surfaces of these pedestals were striated in the direction of the main ice-flow of the district, while the surrounding lower rock in no case bore traces of glaciation, but showed what is known as a weathered surface. He inferred that the pedestals were portions of the rock protected by the overhanging boulder from the down-pouring rain, which had removed the surrounding exposed parts of the surface. When the pedestals attained a certain height relatively to the surrounding rock the rain would beat in under the boulder, and thus there was a natural limit to their possible height. He referred to the action of vegetation in assisting the decomposition of the limestone, and considered that there were so many causes of different rates of waste and so many sources of error, that he distrusted any numerical estimate of the time during which the surrounding limestone had been exposed to denudation. Considering the mode of transport of the boulders, he thought that they could not have been carried by marine currents and coast-ice, as they had all travelled, in the direction of the furrows on the rock below them, from the parent rock on the north. Moreover, marine currents would have destroyed the glaciation of the rock and filled the hollows with debris. Furthermore, the boulders and striæ are found in the same dis-

trict at such very different levels and in such positions as to preclude the possibility of their being due to icebergs. Nor could the boulders represent the remainder of a mass of drift which had been removed by denudation, for the following reasons: (1) they were all composed of one rock, and that invariably a rock to be found in place close by; (2) any denudation which could have removed the clay and smaller stones of the drift would have obliterated the traces of glaciation on the surface of the rock; (3) the boulder which had protected the fine glacial markings below it from the action of the rains would certainly in some cases have preserved a portion of the stiff boulder-clay; (4) the margin of the boulder-clay along the flanks of Ingleborough was generally marked by lines of swallow-holes, into which the water ran off the boulder-clay; and when the impervious beds overlying the limestone had been cut back by denudation, a number of lines of swallow-holes marked the successive stages in the process; but there was not such evidence of the former extension of the drift up to the Norber boulders; (5) the boulders themselves were not rounded and glaciated in the same way as the masses of the same rock in the drift, but resembled the pieces now seen broken out by weathering along the outcrop of the rock close by. Having thus shown the improbability of these boulders having been let down out of a mass of drift the finer part of which had been removed by denudation, or of their having been masses floated to their present position on shore-ice, he offered an explanation of their peculiar position, which he thought was not inconsistent with the view that they belong to some part of the age of land-ice. That they were to be referred to some exceptional local circumstances seemed clear from the rarity of such glaciated pedestals, while boulders and other traces of glaciation were universal over that part of the country. He therefore pointed out, in explanation, that they occurred always where there was a great obstacle in the path of the ice: at Cunswick the mass of Kendal Fell curving round at the south and across the path of the ice; at Farleton the great limestone escarpment rising abruptly from Crooklands; at Norber the constriction of the Crummack valley near Wharfe, and the great mass of Austwich grit running obliquely across its mouth. In all these cases the ice had to force its way up hill; and there would be a time when it would just surmount the obstacle after a season of greater snowfall, and fall back after warm seasons, until it fell back altogether from that part. During the season of recession, boulders would be detached below the ice-foot; during the seasons of advance they would be pushed forward; and in those exceptional localities of isolated hills from which the drainage from higher ground was cut off, the boulders were left on a clean furrowed surface of limestone, which was then acted upon by rainwater and the vegetation, except where protected by the boulders. The author said that the reason why he objected to any numerical estimate of the time which had elapsed since the boulders were left on the glaciated surface was that we did know that the rate of weathering in the limestone was most unequal. He gave cases from Devonshire and the Lake District of extensive weathering in a few years. He had called attention to the great acceleration of decomposition where the vegetation encroached on the limestone, and he maintained that we had no constant measure to apply.—On some derived fragments in the Longmynd and newer Archæan rocks of Shropshire, by Dr. Charles Callaway, F.G.S. Further evidence was added to that given in the author's previous paper (*Q.J.G.S.*, 1879, p. 661) to show that the Longmynd rocks of Shropshire were chiefly composed of materials derived from the Uriconian series, and that the Uriconian series itself (Newer Archæan) was partly formed from the waste of pre-existing rocks. This evidence consisted of (1) the presence, throughout the greatly developed Longmynd conglomerates and grits, of purple rhyolite fragments, recognised by microscopical characters as identical with the Uriconian rhyolites of the Wrekin, and the occurrence of grains, probably derived from the same rhyolites, in the typical green slates of the Longmynd; and (2) the existence of conglomerate beds containing rounded fragments of granitoid rock in the core of the Wrekin itself, whilst the Uriconian beds of other localities, and especially those of Charlton Hill, contained waterworn pebbles, chiefly metamorphic. These pebbles appeared to have been derived from metamorphic rocks of three distinct types. The views put forward were founded on microscopical evidence, of which some details were given in the paper, and were supported by the views of Prof. Bonney, who had furnished notes on the microscopical characters of the rocks.—Notes on the relations of the Lincolnshire carstone, by Mr. A. Strahan, M.A., F.G.S. The Lincolnshire carstone has hitherto been supposed to be correlative with the upper part of the Speeton series, and to be quite unconformably overlain by the red chalk (*Quart. Journ. Geol. Soc.*, vol. xxvi. pp. 326–47). But the overlap of the carstone by the red chalk, which seemed to favour this view, is due to the northerly attenuation, which is shared by nearly all the Secondary rocks of Lincolnshire. Moreover, the carstone rests on different members of the Tealby group, and presents a strong contrast to them in lithological character, and in being, except for the derived fauna, entirely unfossiliferous. It is composed of such materials as would result from the "washing" of the Tealby beds. In general it is a reddish-brown grit, made up of small quartz-grains, flakes and spherical grains of iron-oxide, with rolled phosphatic nodules. Towards the south, where it is thick, the nodules are small and sporadic. Northwards, as the carstone loses in thickness, they increase in size and abundance, so as to form a "coprolite-bed," and have yielded specimens of *Ammonites speetonensis*, *A. plicomphalus*, *Lucina*, &c. When the carstone finally thins out, the conglomeratic character invades the red chalk, similar nodules being then found in this rock. The presence of these nodules, with Neocomian species, taken in connection with the character of the materials of the carstone, points to considerable erosion of the Tealby beds. On the other hand, there is a passage from the carstone up into the red chalk. It would seem, then, that the carstone should be regarded as a "basement-bed" of the Upper Cretaceous rocks. The Lincolnshire carstone is probably equivalent to the whole of the Hunstanton Neocomian, the impersistent clay of the latter being a very improbable representative of the Tealby clay. It therefore follows that the whole Speeton series is absent in Norfolk, and also in Bedfordshire. The unconformity at the base of the carstone becomes greater southwards, and the nodules have been derived from older rocks. Similarly north of Lincolnshire, where the Speeton series is overlapped, the nodules in the red chalk, marking the horizon of the carstone, have been derived from oolitic rocks. In the south of England it would seem that equivalents of the Speeton series reappear. The Atherfield clay contains an indigenous Upper Speeton fauna, while a pebble-bed near the base of the Folkestone beds is described by Mr. Meyer as containing derived oolitic pebbles, and being probably the representative of the Upware deposit, and presumably, therefore, also of the Lincolnshire carstone.—The geology of Cape Breton Island, Nova Scotia, by Edwin Gilpin, Jun., F.R.S.C., Inspector H.M. Mines. After referring to previously published descriptions of Cape Breton geology, the author stated that the various formations found in the island had been thus classified by the officers of the Geological Survey:—

Pre-Cambrian (Laurentian)
including { The Felsite series. / The Crystalline Limestone series.
Lower Silurian.
Devonian.
Carboniferous, including
- Lower Coal-formation.
- { Gypsiferous series. / Limestones, &c.
- Millstone-Grit.
- Middle Coal-formation.

He then proceeded to give an account of each system and its subdivisions in order, commencing with the most ancient, and adding a few detailed sections of the rocks belonging to some of the principal series. He described the distribution and relations of the several divisions. The paper concluded with a few notes on the superficial geology of the island. There is a general absence of moraines and of the fossiliferous Post-Pliocene marine clays of the Lower St. Lawrence. The older beds are generally exposed, but deeper soils and deposits with erratic boulders are found overlying the Carboniferous beds. Marks of recent ice-action are found on the shores of some of the lakes, and are due to the ice being driven by the wind.—On the Decapod Crustaceans of the Oxford Clay, by James Carter, F.G.S. The author commented on the paucity of these fossils as indicated in British lists, only three or four species having hitherto been recorded. The discovery of considerable numbers of Decapod Crustaceans in the Oxford Clay of St. Ives has enabled the author to increase the list materially. Many have been collected by Mr. George,

of Northampton. These fossils occur in the clay immediately beneath the St. Ives rock, and therefore presumably in the uppermost zone of the Oxford Clay. Many of the specimens are more or less mutilated, but some fifteen or sixteen distinct species have been made out. None of these have been recorded as British except *Eryma Babeaui*, mentioned by Mr. Etheridge as having been found in the Kimmeridge Clay. Seven species are identified as foreign forms, and seven are new to science. They are distributed as follows :—

Eryon	1	species.
Eryma	5 or 6	,,
Glyphea	2	,,
Magila	2 or 3	,,
Mecochirus	2	,,
Goniochorus	1	,,
Undetermined	3	,,

Nearly all the forms being to the type of the Macrura, the Brachyura being doubtfully, if at all, represented.—Some well-sections in Middlesex, by W. Whitaker, B.A. Lond., F.G.S. Accounts of many well-sections and borings having been received since the publication of vol. vi. of the Geological Survey *Memoirs*, the author now gave more or less detailed descriptions of fifty-six of these, all in the Metropolitan county, and all either unfinished or, in a few cases, with further information as to published sections. The depths range from 59 to 700 feet, more than half being 300 feet or more deep. Nearly all pass through the Tertiary beds into the Chalk, and most have been carried some way into the latter. Papers descriptive of like sections in Essex, Herts, and Surrey have been sent to Societies in those counties. —On some Cupriferous Shales in the Province of Houpeh, China, by H. M. Becher, F.G.S. This communication contained some geological observations made during a visit to a locality on the Yangtse River, near I-chang, about 1000 miles from the sea, for the purpose of examining a spot whence copper ore (impure oxide with some carbonate and sulphide) had been procured. The principal formations in the neighbourhood of I-chang were said to be Palæozoic (probably Carboniferous) limestones of great thickness, overlain by brecciated calcareous conglomerate and reddish sandstones, which form low hills in the immediate vicinity of the city. About fifty miles further west the limestones pass under a great shale-series with beds of coal, the relations of which to the sandstones are not clearly ascertained. The copper ore examined by the writer came from the sbales, which contained films and specks of malachite and chrysocolla, and in places a siliceous band containing cuprite, besides the oxidised minerals, was interstratified in the beds. Occasionally larger masses of pure copper ore are found embedded in the strata. The ground had not been sufficiently explored for the value of the deposits to be ascertained.—The Cascade Anthracite Coal-field of the Rocky Mountains, Canada, by W. Hamilton Merritt, F.G.S. The coal-field named occurs in the most eastern valley of the Rocky Mountains, that of the Bow River, and, like other coal-fields of the country, consists of Cretaceous rocks, which lie in a synclinal trough at an elevation of about 4300 feet above the sea. The underlying beds, of Lower Carboniferous, or possibly Devonian, age, rise into ranges 3000 feet higher. Further to the eastward the Jurassic and Cretaceous coal contains a large percentage of hygroscopic water and volatile combustible matter, and has the mineral composition of lignite. The average composition is :—

	Per cent.
Fixed carbon...	42
Volatile combustible matter ...	34
Hygroscopic water	16
Ash	8
	100

As the mountains are approached, the amount of hygroscopic water is found to diminish by about 1 per cent. for every 10 miles, and 15 miles from the range the percentage is about 5. In the foot-hills the lignites pass into a true coal, with 1·63 to 6·12 per cent. of hygroscopic water, and 50 to 63 per cent. of fixed carbon. In the Cascade River coal-field the average character of the coal is that of a semi-anthracite, with the following composition ;—

	Per cent.
Fixed carbon	80·93
Volatile combustible matter ...	10·79
Hygroscopic water	71
Ash	7·57
	100·00

The coal-seams have been subjected to great pressure, and the change in the quality of the coal appears to be due to metamorphic influence.—On a new Emydine Chelonian from the Pliocene of India, by Mr. R. Lydekker, B.A., F.G.S. The author described the shell of an Emydine tortoise from the Siwaliks of Perim Island, Gulf of Cambay, which he regarded as decidedly distinct from any of the previously described Siwalik species, and proposed to refer to the genus *Clemmys*, with the name of *C. watsoni*, in compliment to the donor of the specimen.—On certain Eocene formations of Western Servia, by Dr. A. B. Griffiths, F.R.S.E., F.C.S. Communicated by the President. A great thickness of paper-shales containing paraffin occurs near the River Golabara ; these extend over 30 square miles of country. Small beds of clay with rock-salt are also found : the whole is said to resemble the paraffin and salt districts of Galicia. The paraffin shale is free from bituminous impurities. It contains :—

	Per cent.
Paraffin wax	1·75
Water of combination	3·02
Ammonia	1·18

The mineral constituents of the shale are :—

	Per cent.
Alumina	32·86
Iron oxide...	5·20
Magnesia	1·26
Lime	1·21
Potash	2·17
Soda	0·41
Silica	56·85
Loss	0·04
	100·00

The brown coal of the neighbourhood, whose natural distillation has most probably yielded the hydrocarbon in the shales, contains :—

	Per cent.
Carbon	49·2
Hydrogen	1·1
Water, combined	30·2
Water, hygroscopic	19·5
	100·00

The beds containing these coals have been invaded by eruptive porphyry and trachytic rocks, of which the former contains 75½ and the latter 61 per cent. of silica. The clays from which the shales were originally formed contain abundance of marine Diatomaceæ and Foraminifera (chiefly Nummulites), as also species of *Ostrea*, *Cyrena*, *Cerithium*, *Voluta*, and *Nautilus*, together with the remains of Placoid and Teleostean fishes.

Paris

Academy of Sciences, July 5.—M. Jurien de la Gravière, President, in the chair.—Memoir on the life and works of Louis-François-Clement Bréguet, Member of the Academy of Sciences, born at Paris on December 22, 1804, died October 27, 1883, by M. de Jonquières.—Obituary notice of M. H. Abich, Corresponding Member of the Section for Mineralogy, who died at Vienna on July 1, 1886, by M. Daubrée.—Preliminary note on the principles and method employed in a study on the movement of the hydro-extractor, about to be presented to the Academy, by M. de Jonquières.—Experiments on a new apparent paradox in hydraulics, by M. A. de Caligny.—Final objections to M. de Bussy's formulas on the roll of vessels, by M. A. Ledieu. It is pointed out that M. de Bussy's theorising is of a purely speculative character, of very little practical

utility. After the protracted studies of Froude and Rankine in England, published in the *Transactions* of the Institution of Naval Architects (1861–64), and of MM. Bertin and Bénazé in France, the subject may be regarded as exhausted.—On the real position to be assigned to the fossil flora of Aix, in Provence, by M. G. de Saporta. It is argued against the views of M. Fontannes on stratigraphic grounds that the whole series of varied and numerous deposits giving birth to the flora of Aix, cannot be reduced to the gypsum alone, or to the section of this gypsum contiguous to the beds at Cyrènes. In a further paper it will be shown that the palæontological indications are equally opposed to M. Fontanne's opinion.—Note and photographs of the thunderstorm of May 12, 1886: spiral form of lightning, by M. Ch. Moussette. The photographs taken at Auteuil on this occasion seem to indicate a general law that the electrical discharges between the clouds and the earth assume the normal form of irregular spirals.—Observations of the new planet 259 made at the Paris Observatory (equatorial of the West Tower), by M. G. Bigourdan.—On the development in series of the potential of a homogeneous revolving body, by M. O. Callandreau. In this paper the author verifies the two formulas of Legendre and Laplace relative to the exterior and interior points of a spheroid usually defined by the equation $r = a(1 + \alpha y)$.—Memoir on the rowing-vessels of antiquity, by M. Corazzini. The author attempts to solve the difficult problems associated with the construction of the *naves longæ*, and reconstructs the Roman polyremes in a manner which seems to harmonise best with the monuments and the descriptions of classic writers.—On the refraction of carbonic acid and of cyanogen, by MM. J. Chappuis and Ch. Rivière. The results of the authors' researches on the refraction of carbonic acid at 21° and up to 19 atm. are resumed in the formula—

$$n - 1 = 0{\cdot}000540p(1 + 0{\cdot}0076p + 0{\cdot}0000050p^2),$$

in which n denotes the index for the ray D, and p the pressure in metres of mercury. The refraction of cyanogen has also been studied at different temperatures between the pressures of 1m. and 2m. or 3m. of mercury, the series of experiments relative to a determined temperature being resumed in a formula of the form $n - 1 = ap(1 + bp)$.—On the electrical conductivity of the mixtures of neutral salts, by M. E. Bouty.—On the decomposition of the perchloride of iron by water, by M. G. Foussereau. The author had already employed the measure of electric resistance to determine the nature and proportion of foreign substances contained in water and alcohol, and the conditions under which these fluids acquire the greatest degree of purity. He now applies the same method to the study of the progressive alterations of fluids, and especially of saline solutions under the influence of the dissolvent. The present paper deals specially with the perchloride of iron.—Note on a transmitting dynamometer with a system of optical measurement, by M. P. Curie. This apparatus consists of a horizontal arbor supported by two bearings. Two pulleys at the extremities of the arbor serve to transmit the motion from the motor to the receiver, and the work done is measured during the motion by the torsion of the arbor between the two pulleys.—Temperature of the deep waters in the Lake of Geneva, by M. F. A. Forel. Observations taken during the years 1879–86 show that at great depths the temperature never falls below 4°, and varies normally between 4°·6 and 5°·6. From his experiments the author also infers that the heat penetrates to the lower layers mainly through the mechanical intermingling of the upper with the deep waters under the action of the winds. The same explanation, he argues, should be applicable to all lakes and to all seas confined by bars, notably the Mediterranean, whose deep waters have a mean temperature of 13°.—Absorption spectra of the alkaline chromates and of chromic acid, by M. P. Sabatier.—On the heat of transformation for vitreous selenium to metallic selenium, by M. Ch. Fabre. Vitreous is transformed to metallic selenium by heating it to 96° or 97°, the transformation being accompanied by a considerable development of heat, which is here directly determined by means of M. Berthelot's calorimeter.—Action of vanadic acid on the alkaline haloid salts, by M. A. Ditte.—On the fluorides of the metalloids, by M. Guntz. By practical tests the author has verified his hypothesis that the fluoride of lead is decomposable by all the chlorides of the metalloids. With the oxychloride of phosphorus the reaction is so regular that it gives a convenient process for preparing the oxyfluoride of phosphorus.—On the hydrate of baryta, BaO,H_2O_2, by M. de Forcrand.—A contribution to the study of the alkaloids, by M. Œchsner de Coninck.—Isomery of the camphols and of the camphors, by M. Alb. Haller.—Researches on the chemical composition of the grease of sheep's wool, by M. A. Buisine. The grease of Australian wool yielded for 100 of dry residuum 7·1 of acetic acid, 4 of propionic acid, 2·6 of benzoic acid, 2·59 of lactic acid, 1 of capric acid.—Acidimetric analysis of sulphurous acid, by M. Ch. Blarez.—Researches on the development of beetroot; study of the leaf, by M. Aimé Girard.—Comparative studies on the influence of the two orders of vaso-motor nerves, on the circulation of the lymph, on their mode of action, and on the mechanism of lymphatic production, by M. S. Lewachew.—On a process of indirect division by threes of the cellules in tumours, by M. V. Cornil.—The house-bug and the seat of its fetid secretion: the dorsal abdominal glands of the larva and nymph; the sternal thoracic glands of the adult, by M. J. Künckel.—On the influence of certain Rhizocephalous parasites on the exterior sexual characters of their host, by M. A. Giard.—On the circulatory system of the Echinidæ, by M. R. Kœhler.—On the seeds of Bonduc, and their active principle as a febrifuge, by MM. Ed. Heckel and Fr. Schlagdenhauffen. These seeds are supplied by two closely allied exotics: *Guilandina Bonducella, L.* (*Cæsalpinia Bonducella*, Flem.) and *Cæsalpinia Bonduc*, Roxb. Their therapeutic properties are shown to reside in the bitter principle, which acts against intermittent fevers as efficaciously as the salts of quinine.—On the Triassic system of the Eastern Pyrenees, in connection with M. Jacquot's recent communication, by M. A. F. Nogués.—Invertebrate fauna of the Mentone grottoes, Italy, by M. Emile Rivière. In these caves the author has discovered 171 species of invertebrates, comprising 20 fossil, 125 living marine, and 26 land species. Amongst the living marine species 50 are at once Mediterranean and oceanic, 62 exclusively Mediterranean, and 6 oceanic.

BOOKS AND PAMPHLETS RECEIVED

"A Word for Ireland," by T. M. Healy (Gill, Dublin).—"Inorganic Chemistry," by Ira Remsen (Macmillan).—"British Fungi, Lichens, &c.," by Holmes and Gray (Sonnenschein).—"Journal of the Mathematical Society of St. Petersburg," vol. vi.—"Outlines of the History of Ethics," by H. Sidgwick (Macmillan).—"Proceedings of the Academy of Natural Sciences of Philadelphia," part 1 (Philadelphia).—"The Handy Guide to Emigration to the British Colonies," new edition, by W. B. Paton (S.P.C.K.).—"Notes from the Leyden Museum," vol. viii., No. 3, July (Brill, Leyden).

CONTENTS

THURSDAY, JULY 22, 1886

HAND-BOOK OF PLANT DISSECTION

Hand-Book of Plant Dissection. By J. C. Arthur, M.Sc., Charles R. Barnes, M.A., and John M. Coulter, Ph.D. (New York: Henry Holt and Co., 1886.)

THIS work will take the same place in the botanical teaching of the United States as will be occupied in this country by the "Practical Botany" of Messrs. Bower and Vines, when the latter is completed. Both are essentially guides to the laboratory instruction which now forms the most important part of every efficient course of botany.

The American hand-book differs from its English prototype in two important respects: first, in the fact that it begins with the lowest plants, while the English work begins with the highest; and secondly, in its more rigid adherence to the type system. Prof. Bower did not limit the work entirely to the main types, but frequently introduced other plants, which happened to be more favourable for the study of particular points of structure. The authors of "Plant Dissection," on the other hand, give us the type, and the type only. Their plan has the advantage of simplicity, but several points have to be passed lightly over which could have been studied efficiently in plants other than the selected types. On the whole, the more elastic method of the "Practical Botany" seems to us to be more satisfactory. Any teacher of botany would select Cucurbita for the study of the sieve-tubes, Caltha, or some allied plant, for the embryo-sac, and so on; and yet these are not plants which would be well suited for generally typical examples.

As regards the other point, whether it is better to begin at the upper or lower end of the vegetable kingdom, it may perhaps be said that the former is the course better adapted for beginners, while the latter has its advantages in the case of advanced students. If the learner has no previous knowledge of plants at all, it may be difficult to rouse his interest in such obscure forms as Oscillaria or Cystopus, while the study of some familiar plant, such as the sunflower or shepherd's purse, is much more likely to attract him. On the other hand, if some preliminary knowledge may be assumed, there will be no objection to following the strictly logical course of proceeding from the simpler to the more complex.

The "Hand-Book of Plant Dissection" begins with a short introduction on reagents, section-cutting, &c., and then come the types, occupying the bulk of the work. They are twelve in number, and have been selected as follows:—For the lower Chlorophyceæ, *Protococcus viridis;* for the Cyanophyceæ, *Oscillaria tenuis;* for the Conjugatæ, *Spirogyra quinina;* for the Phycomycetes, *Cystopus candidus;* for the Ascomycetes, *Microsphæra Friessi;* for the Liverworts, *Marchantia polymorpha;* for the Mosses, *Atrichum undulatum;* for the Ferns, *Adiantum pedatum;* for the Gymnosperms, *Pinus sylvestris;* for the Monocotyledons, *Avena sativa* and *Trillium recurvatum;* and lastly, for the Dicotyledons, *Capsella Bursa-pastoris.* It will be seen that while one or two of these plants are strictly American forms, most of the types are cosmopolitan.

It would be easy to criticise the selection in some of the cases: thus, "Protococcus" is not really entitled to the first place on the list, for its cells are more highly organised than those of the Oscillaria. Pythium shows the sexual organs much better than Cystopus, and Microsphæra is perhaps not so characteristic an Ascomycete as might have been found. On the whole, however, the types are good ones.

A few points may be mentioned where there appears to us to be room for serious criticism. It is evident from the remarks on p. 55 that Sachs's old classification of the Thallophytes is adhered to. Surely after the publication of De Bary's papers in the *Botanische Zeitung*, in 1881, and of Goebel's "Grundzüge der Systematik," in 1882, there is no excuse for retaining this manifestly artificial arrangement. Sachs's grouping of the Thallophytes by their sexual organs alone, without any regard to general structure, has been unkindly, but pointedly, compared to the sexual system of Linnæus, which is not usually reckoned as a natural arrangement. How inexpedient the classification in question is for the student is well shown in the work before us on the page referred to, where the reader is advised to study Nemalion or Batrachospermum in order to understand the fruit of Microsphæra. Can any one seriously believe that detailed homologies can be traced between so isolated a group as the red seaweeds and a highly specialised parasitic member of the Ascomycetous Fungi?

Going on to the chapter on the Liverwort, the footnote on p. 75 seems likely to confuse rather than to enlighten the student. The archegonia are *not* called sporogonia after fertilisation by any one who wishes to keep the distinction between the sexual and asexual generations clear in the mind of the learner. The sporogonium arises from the oosphere only; the archegonium, as distinguished from the oosphere, takes no part in its formation.

In the same chapter a statement on p. 82 that "the antheridia are modified hairs" demands notice. This is a bad example of old-fashioned morphology. The antheridia of the Liverworts are modified successors of the antheridia of the lower plants. The ancestors of highly organised plants like Marchantia must have long possessed sexual organs, probably at least as long as they have possessed "trichomes." The same mistake reappears on p. 120 in the description of the fern, when the "trichomes" are said to appear "in the form of sporangia." Either this is merely a roundabout way of stating that the sporangia are of epidermal origin, or else it means that these reproductive organs are actually due to the modification of hairs. The latter view will hardly commend itself to any one who realises that the spores of the fern are homologous with those of the Muscineæ.

A repetition of the same confusion of ideas on p. 125 need not be further noticed.

In the account of the anatomy of the leaf of Pinus there is an error as to a simple matter of fact which ought to be corrected. On p. 154, *d* and *e*, the thin-walled cells of the mesophyll, are said to be empty, while those with bordered pits are described as having "more or less conspicuous contents." This is just the reverse of the truth. The thin-walled cells have protoplasmic con-

tents throughout life, while the tracheides with bordered pits (transfusion tissue) contain, in the mature condition, nothing but water.

On p. 164 the statement that there is finally "free communication" between the contiguous tracheides of the wood of Pinus is erroneous. The pits are closed, at any rate as long as the wood serves its main function of conveying the sap.

At p. 171, in the same chapter, there is a repetition of Hofmeister's old mistake as to the deliquescence of the original cell-walls of the endosperm in the Conifers. Strasburger showed in his "Angiospermen und Gymnospermen," that this idea was due to Hofmeister having confused the disorganised cells of the nucellus with those of the endosperm. The Conifers have one and the same endosperm throughout the development of the ovule: there is no distinction of "primary and secondary" endosperm.

Judging from the footnote on p. 209, there seems to be some confusion between the xylem and the bundle-sheath in Trillium.

It is to be regretted that the student is not shown how to investigate the minute structure of the angiospermous embryo-sac when ready for fertilisation.

In spite of the rather serious faults noticed, the book on the whole is a good and useful one. D. H. S.

MR. MERRIFIELD'S "TREATISE ON NAUTICAL ASTRONOMY"

A Treatise on Nautical Astronomy for the Use of Students. By John Merrifield, LL.D., F.R.A.S. (London: Sampson Low, Marston, Searle, and Rivington, 1886.)

THIS is an excellent work for the student, evidently compiled with considerable care, which may also be consulted with advantage by the seaman. Of course the author does not claim originality, excepting in one particular, viz. a method of his own for "clearing the lunar distance," as, in point of fact, nearly everything the work contains has been published in previous treatises. Mr. Merrifield deserves, however, the credit of placing clearly before the student many points which are only touched on by other writers—notably the account of the correction for refraction, and the explanation of the fact that the maximum altitude is not invariably the meridian altitude, a point which is only touched on by a footnote in Raper, and is usually ignored entirely; yet which is of considerable importance in the case of the moon. The examples, also, which are given at the end of each chapter are of great use to the student, as from them a knowledge is obtained of the subjects he is likely to be examined in; and as these questions have been selected from many examination papers, they are an excellent guide. In the theoretical part of nautical astronomy the book is nearly all that can be desired, and this part can always be learnt better on shore than in a ship, where the constant noise and interruption, together with perpetual motion at sea, renders study all but impracticable: in one or two cases, however, Mr. Merrifield also touches on the practical use of instruments, &c., and on these subjects he is naturally not so good an authority. It may perhaps, therefore, be advisable to point out the usual course of proceedings in Her Majesty's surveying-vessels, both in correcting instruments and also in ascertaining positions at sea.

First, with regard to the sextant, the error of collimation is not readily obtained, as stars only are available, and there are no means of illuminating the wires in the telescope, so that a bright moonlight night is requisite. Secondly, with respect to the errors of centering and graduation, Mr. Merrifield suggests that the combined error should be ascertained by means of measuring the distance between several pairs of stars by the instruments, the correct distances having been previously calculated. But here the varying nature of the refraction prevents good results, and a better method is to measure the distances both by the sextant and by the repeating circle as in the latter instrument all errors are eliminated.

In the account of the artificial horizon Mr. Merrifield says that "it is used for taking altitudes when the sea horizon is obscured," being apparently under the impression that it can be used on board a vessel. Were such the case, it would often relieve the mind of many an anxious navigator, but, unfortunately, the constant motion of a ship altogether precludes its use at sea; it is true that the late Capt. Becher, R.N., invented a method of observing altitudes at sea, in foggy weather, by attaching a small pendulum, suspended in oil, outside the horizon-glass of a sextant; to this a horizontal arm was fastened which carried at its inner end a slip of metal showing the true horizon when seen in a certain position; but this did not prove a success, and is now almost forgotten; and there is nothing to trust to but the compass and log when the horizon is obscured. The true use of the artificial horizon is to obtain observations on shore, and the sea horizon should never be used then. The best artificial horizon is a trough filled with mercury, covered with a glass roof, but this cannot be used in the extreme cold of the Arctic regions, and consequently there a plate of dark glass is substituted, which is adjusted by spirit levels. The error of the artificial horizon is due to two causes, first the imperfections in the glass roof, which, as Mr. Merrifield remarks, may be guarded against by reversing the roof; and secondly, owing to the attraction of mountain masses causing the mercury to depart from the true level. Could some means be found which would enable the seaman to take observations, in a vessel, independently of the sea horizon, it would be the most useful nautical discovery of the age, but this is not to be effected, as Mr. Merrifield suggests, by mounting the artificial horizon on gimbals, for even if the ship were in herself rigid, the motion at sea would preclude the possibility of obtaining observations, as the position of the observers could not be changed with sufficient rapidity to suit the ever-varying angle of reflection from the horizon, with respect to the observer on the deck; and Mr. Merrifield's own experiences of the difficulties of obtaining observations from the roof of a quiet house must have taught him that it would be much more difficult in a vessel which is constantly vibrating from the motion of the engines or other disturbing causes. The idea of placing a piece of glass on the mercury to still its vibrations, was some years ago promulgated by the late Staff-Commander George, attached to the Geographical Society, who invented a very useful little artificial horizon

for the benefit of travellers, in which the floating glass was part of the plan.

In obtaining the position of a ship at sea the difficulty is to get observations both for latitude and longitude at the same time, as all other observations depend on the distance covered by the vessel in the time which has elapsed between the observations. Now, as this distance depends not only on the direction and rate of the vessel through the water, but also on the direction and rate at which the water itself is moving, and as this latter element in the calculations cannot be ascertained with precision, it follows that all observations at sea which depend on the ship's run in the interval have an element of uncertainty. The best time to obtain simultaneous observations for latitude and longitude is at twilight, morning and evening, as then the horizon is clear, and, unless the weather is very cloudy, some stars can be seen. Here Sumner's method is invaluable, as three or more stars can be utilised and the correctness of the result guaranteed, provided, of course, that the chronometer is correct. In the day-time the only chance to obtain simultaneous observations is when the sun and moon are both visible, or when Jupiter, or Venus, happen to pass the meridian at an interval of over $2\frac{1}{2}$ hours from noon, as then, in bright weather, their meridian altitudes can be obtained by a practised observer with a good sextant.

One of the difficulties in obtaining good results at sea is owing to the varying nature of the refraction, more especially close to the horizon. This may be guarded against in the case of the meridian altitude of the sun by observing, when practicable, its altitude with the north and south horizons. To show the closeness of the results ascertained in this manner, it is only necessary to observe that H.M.S. *Triton*, when fixing the position of the Ower and Lemon light-vessel on the east coast of England in 1884, obtained the latitude on four different days, the results being as follows :—

June 25	...	...	...	Lat. $53^{\circ}\ 7'\ 56''$ N.
July 9	...	...	...	,, 53 8 0 N.
July 11	...	...	...	,, 53 7 54 N.
July 12	...	...	...	,, 53 7 57 N.

an extreme range of $6''$, or 600 feet, in the latitude. Such a close accordance shows the value of this method, which is recommended by Raper.

As regards obtaining the longitude by lunar distances, this has been gradually falling into desuetude owing to the quicker passages made by vessels and to the cheapness of chronometers. There can, however, be no doubt of its utility, as it is the only good way of obtaining the position of the ship at sea should any accident happen to the chronometers, and it is to be regretted that it is so seldom practised, particularly when we remember the excellent results obtained by the older navigators, especially by Cook. For the actual observation the repeating circle is a far better instrument than the sextant, as by it the distance between the sun and moon is observed with much greater accuracy, a matter of the utmost importance when we remember that an error of one minute in the distance makes an error of twenty-five miles of longitude under the most favourable circumstances. It is therefore evident that this observation requires to be made with the utmost care and that constant practice is necessary to obtain good results.

In the problem of obtaining the true bearing of a terrestrial object from a ship at sea, Mr. Merrifield has omitted the correction of the angular distance due to the height of the object : this is probably an accidental omission, but although it does not usually amount to much, it is desirable the student should be acquainted with it.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Laws of Solution

In my paper on "Chemical Affinity and Solution," published in NATURE, vol. xxxiii. p. 615, I gave some general proofs (taken from Thomsen's researches on thermo-chemistry) of the truth of my theory of solution. I shall now show that there are certain well-marked and definite laws of solution which are in complete accord with that theory, and seem to me to place it beyond doubt. In all chlorides, bromides, iodides, sulphates, and nitrates, for which data are available, the heats of solution in water *vary directly*—

(1) As the affinity (measured by heat of combination) of the positive element of the salt for O varies ;

(2) As the affinity (measured by heat of combination) of the negative element or radicle of the salt for H varies ;

And *inversely*—

As the affinity (measured as above) between the positive and negative elements of the salt varies.

The following examples will make this plain :—

Compound	Heat of combination	Difference	Heat of solution of chloride	Difference
[Mg,Cl_2	151010	—	35920	—
[Mg,O,Aq]	148960	2050	—	—
[Ca,Cl_2]	169820	—	17410	+ 18510
[Ca,O,Aq]	149260	20560	—	—
		− 18510	—	+ 18510
[Ca,Cl_2]	169820	—	17410	—
[Ca,O,Aq]	149260	20560	—	—
[Sr,Cl_2]	184550	—	11140	+ 6270
[Sr,O,Aq]	157780	26770	—	—
		− 6210	—	+ 6270
[$SrCl_2$]	184550	—	11140	—
[Sr,O,Aq]	157780	26770	—	—
[Ba,Cl_2]	194740	—	2070	+ 9070
[Ba,O,Aq]	158760	35980	—	—
		− 9210	—	+ 9070

Similar results are obtained if we substitute the alkali metals for above, but there is a variation in the case of metals which form insoluble oxides or hydrates. In the latter case the heats of solution are not so great as they should be if compared with above compounds. Among themselves, however, they follow the laws pretty closely, and seem arranged in groups. Thus, $ZnCl_2$ and $CdCl_2$, $FeCl_2$, $CoCl_2$, and $NiCl_2$ form two such groups.

The foregoing examples illustrate the effect of the change of the positive element of the salt on the heat of solution. Now let us change the negative element and we shall see the same result.

Compound	Heat of combination	Difference	Heat of solution	Difference
[K,Cl]	105610	—	−4440	—
[H,Cl,Aq]	39315	66295	—	—
[K,Br]	95310	—	−5080	+640
[H,Br,Aq]	28380	66930	—	—
		−635		+640
[K,Br]	95310	—	−5080	—
[H,Br,Aq]	28380	66930	—	—
[K,I]	80130	—	−5110	+30
[H,I,Aq]	13170	66960	—	—
		−30		+30

These relations obtain for the haloid salts of all the metals for which data were available for comparison. The only exception is $AuCl_3$ and $AuBr_3$, the difference of heats of solution of these salts being too great according to the foregoing laws. They are apparently proportional, however.

There is another way of showing these laws and also of showing the conditions which determine the absolute amount of heat of solution, whether positive or negative. If we take the sum of the heats of formation of any salt and of water on the one hand, and on the other, instead of measuring the heat of solution directly, take the sum of the heats of formation of the oxide, of the acid and of neutralisation, we shall find that the heat of solution is the difference of these sums—positive when the latter sum is the greater, and negative when it is the less. This exhibits in a striking manner the relations of the various affinities to solution, and is very suggestive when we consider that the heat of solution regularly increases with the heat of formation of [M,O,Aq], and when the heat of $[MO] > [M,Cl_2]$, decomposition of water takes place. Consider the following examples :—

Compound	Heat of combination	Compound	Heat of combination
[Mg,OAq]	148960	[Mg,Cl_2]	151010
[2H,Cl,Aq]	78630	[H_2O]	68360
Neutr.	27690		
	255280		219370
	219370		
Difference	35910 = Heat of solution.		
[Sr,O,Aq]	157780	[Sr,S,O_4]	330900
[H_2,S,O_4,Aq]	210770	[H_2,O]	68360
Neutr.	30710		
	399260		399260
	399260		
Difference	0 = Heat of solution. Salt insoluble.		
[K_2,O,Aq]	164560	[K_2,N_2O_6]	242970
[H_2,N_2,O_6,Aq]	102190	[H_2,O]	68360
Neutr.	27540		
	294290		311330
	311330		
Difference	− 17040 = Heat of solution.		

The above illustrate the cases of positive, negative, and zero heats of solution. These relations obtain with all salts, whether the oxide is soluble or not. The only discrepancy I found was in the case of silver chloride, which showed a slight negative heat of solution; but as its affinity for O is excessively small, it is not surprising it should be an abnormal case.

These laws of solution explain and are illustrated by many cases of constant differences in the heats of formation of similar compounds in water. Thus it has been pointed out in Muir and Wilson's "Thermo-Chemistry" that between the heats of formation of soluble chlorides, bromides, and iodides in water, there is a constant difference, no matter what the positive element is. For example, consider the following cases :—

Compound	Heat of formation	Compound	Heat of formation
[H,Cl,Aq]	39315	[H,Cl,Aq]	39315
[H,Br,Aq]	28380	[H,I,Aq]	13170
Difference	10935	Difference	26245
[K,Cl,Aq]	101170	[K,Cl,Aq]	101170
[K,Br,Aq]	90230	[K,I,Aq]	75040
Difference	10940	Difference	26130

Now the reason of this is perfectly obvious in the light of the laws of solution. Any variation from the above differences in the heat of formation of the undissolved salt is at once counterbalanced by the heat of solution, which varies inversely. Thus :—

Compound	Heat of formation	Heat of solution	Total
[H,Cl]	22000	17315	39315
[H,Br]	8440	19940	28380
Difference	13560	−2625	10935
[Na,Cl]	97690	−1180	96510
[Na,Br]	85770	−190	85580
Difference	11920	−990	10930

and so on in other cases.

WM. DURHAM

Ice on the Moon's Surface

IN May 1884 Mr. Peal, of Sibsagar, in Assam, who has studied the moon's surface with great attention, sent me a paper in which he maintained views closely resembling those of Capt. Ericsson (NATURE, p. 248) on the glacial origin of the lunar craters. In my answer I suggested that it was difficult to admit the existence of ice on the moon's surface, without a layer of water vapour over it, and that the telescope proves that if such vapour exists it is only in extraordinarily small quantities. It seems due to Mr. Peal, who was undoubtedly ignorant of Capt. Ericsson's paper of 1869, to draw attention to the correspondence. I am not sure whether the paper has been yet published.

Cambridge, July 17 G. H. DARWIN

Luminous Clouds

I AM not sure of the date, but believe it was in June 1885 that I called attention in your journal to a strange effect of bright silvery lighted clouds, which remained visible in the north-west sky after sunset until nearly 11 p.m. Several times this summer I have noted repetitions of these same curiously lighted cloud-forms, but have never seen such a wonderful display of this "after-sheen" as that of this evening, July 12.

The day from 11 a.m. until 6 p.m. had been wet, followed by a clear-up toward sundown, with a warm orange-coloured sunset near the horizon; above this, and extending nearly to the zenith, lay masses of brilliant and, one would almost say, self-illuminated cloud-ripples looking like an inverted sea of frosted silver or mother-of-pearl.

There was a strongly-marked focus in the light above the place of the sun, but it extended far beyond that both north and west. The vapour forming these cloud-waves, and which received this intense white light, must, I think, have been at a great elevation, for though all the lower vapour near the horizon retained its usual orange glow long after sunset, there was never any indication of colour upon these clouds from the beginning of the effect, about 7.30 p.m., until it disappeared soon after 10 p.m. The moon, which was in the southern part of the sky, looked quite warm in colour when contrasted with the almost bluish-white glare upon this vapour. ROBT. C. LESLIE

Moira Place, Southampton, July 12

THE luminous night clouds seen here on the 22nd ult. (NATURE, July 1, p. 192) have recurred, with a very remarkable development on the night of the 8th inst.

The sketches illustrate phases one hour apart from midnight to 2 a.m.; the last made solely by "cloud-light" in a window with northern aspect! The long luminous belt began to form at 11.30 p.m., fading out at 2.30 a.m. It extended obliquely from N. 10° W. to N. 30° E. in the wind's direction, which was light from N.W. Temperature subsequently fell.

Last night (9th inst.) the upper northern sky was obscured with cumulous cloud, but in a clear space above the horizon, from N. to N.E., a belt of cloud resembling that of the previous night extended obliquely. In this case the belt was dark; but beneath, and apparently descending from it, bright luminous patches formed of a golden lustre at midnight, and faded out at 1.30 a.m. Wind again from N.W., light. Temperature cool for season.

Examined with a good field-glass, these cloudlets present the usual cirrous type in all but singular luminosity, and little (if any) of the aurora.

D. J. ROWAN

Dundrum, co. Dublin, July 10

Animal Intelligence

A REMARKABLE instance of animal intelligence has lately come under my notice, which I venture to relate as being possibly of interest to the readers of NATURE. In a neighbour's bungalow in this district two of our common house-swallows (*Hirundo javanica*) built their nest, selecting as their site for the purpose the top of a hanging lamp that hangs in the dining-room. As the lamp is either raised or depressed by chains fixed to a central counter-weight, these chains pass over pulleys fixed to a metal disk above, on which the nest was placed. The swallows evidently saw that, if the pulleys were covered with mud, moving the lamp either up or down would destroy the nest; so to avoid this natural result they built over each pulley a little dome, allowing sufficient space, both for wheel and chain to pass in the hollow so constructed, without danger to the nest, which was not only fully constructed, but the young birds were reared without further danger. This is, in my opinion, a wonderful example of adaptation to environment, and showing a step far beyond what may be contended as instinct only.

I may here add another curious case which seems to point to another branch of reasoning. During the dry weather I have been constantly annoyed by wasps building up with mud key-holes, sometimes keys, blank cartridge cases, and even in one case a *pen-holder*. As I did not care to have my gun charged with young wasps, I used to empty out any cartridge case which I found closed up with mud, but one cartridge-case in particular I noticed had been selected. This one I had left on my office table, and each time the wasp closed it up I drew the charge of mud and "grubs," &c.; but as frequently the wasp closed it up again. I may here mention that the wasp used to deposit the egg, and several small grubs in a cell, close over the top, and repeat the operation again till the cartridge was full, when the mouth would be pasted over with a lid of mud. As I repeatedly knocked out the grub and mud, it appears the wasp started a fresh plan. I noticed somewhat to my surprise that the mouth of a cartridge I had but a few hours before emptied was pasted over, so I thought it would be interesting to see how many grubs the wasp had secured in so short a time. I therefore removed the fresh lid, that was still damp, and discovered nothing inside! I am unable to say if this was done to direct my attention to one particular cartridge case or not, while another spot was being used, but I am inclined to believe such to have been the case, for later I noticed a gap made between two bundles of letters in one of my pigeon-holes, well built up with mud, and, of course, as well packed with grubs.

Ballangoda, Ceylon, June 14 FREDERICK LEWIS

Deafness and Signs

IN my studies with regard to the sign-languages I have, like others, turned some attention to cases of deafness. In such cases the use of signs, not the finger alphabet, but natural or conventional signs, such as are used by Indians or by deaf-mutes of themselves, have appeared to me to give particular satisfaction to the sufferer. The nervousness attendant upon attempting to make out what is said being avoided, the relief is very great, and more attention is given to what is spoken. Of course such aid to those untrained is but partial, and English people accustomed solely to the use of speech are rather unapt, but nevertheless signs are valuable auxiliaries, and will be found worth trying. Individuals vary in their capability, but inasmuch as many children pass through a period of sign-language, there will be many cases of adaptability. Whoever has watched deaf-mutes conversing, without the finger alphabet or without lip-reading, will recognise the satisfaction they receive from their intercourse by signs. My only object is to call attention to what has been found by experience to be an acceptable help, and which may be extended in its application.

HYDE CLARKE

The Duration of Germ-Life in Water

A RECENT announcement by Messrs. Crookes, Odling, and Tidy, that *Bacillus anthracis* in water approximately devoid of nutrient material after "a few hours" loses its power to multiply in suitable culture-media, induces me to send you a note of my own results in the same domain.

My observations were commenced in 1877, but were shortly afterwards suspended and not resumed in earnest until May 1885.

So far I have worked only with the various forms of organisms which chanced to be present in the water—usually distilled—employed. For a *preliminary* investigation I regard this as preferable to operating on pure cultures; one is more likely to be concerned with organisms of aqueous habitat naturally, and one sees which kinds predominate from time to time, and which survive.

In dealing with an indefinite variety of micro organisms it is necessary, of course, to be extremely rigid in one's precautions to guard against intrusion of foreign germs, an intrusion which cannot be detected as in the case of pure cultures. On this account I abandoned my original *modus operandi*—it was almost identical with that of Mr. Crookes and his colleagues—and adopted the arrangement of tubes described and figured in a paper by Mr. Blunt and myself in *Proc. Roy. Soc.*, vol. xxviii. p. 202.

Of a series of such tubes containing distilled water, originally rich in germ-life, kept at a temperature varying from 18° C. to 21° C., and examined at intervals from May 2, 1885, down to now, I find that in every one micro-organisms have sooner or later developed on the addition of the nutrient material.

Each tube is a microcosm, and it has been most interesting to observe how, as elsewhere, as time went on, the first dominant form has grown more and more feeble, until it seems to have become extinct, and is now succeeded by races of quite different kind. Whether the new order will yet give place to others remains to be seen. I can at any rate say confidently that micro-organisms vary greatly in the duration of their life in distilled water, and that some forms may survive for at least fourteen months in that medium at an ordinary temperature.

Chelmsford, July 19 ARTHUR DOWNES

The Bagshot Beds

IN reply to the letter from Mr. Irving in NATURE of July 8 (p. 217), I beg to state that a mere abstract of the paper on the Bagshot Beds by Mr. Herries and myself was read at the meeting of the Geological Society on June 9, on which occasion Mr. Irving was not present; that the report of our remarks in NATURE of July 1 (p. 210) only purports to give the conclusions at which we arrive, and not the evidence by which they are supported. We trust therefore that your readers will reserve their judgment until the entire paper is published.

HORACE W. MONCKTON

1, Hare Court, Temple, July 17

A Lubricant for Brass Work

MANY besides myself have probably been inconvenienced by the corrosive action of ordinary lubricants—lard, grease, &c.—upon brass and copper, which causes the plugs of stop-cocks to leak or get fixed in their places, and does much damage to air-pump plates.

Melted india-rubber answers fairly, but it has too little "body," and too much glutinosity; moreover, it does, undoubtedly, in course of time, harden into a brittle, resinous substance. Vaseline is quite without action on brass, and never hardens; but it has not sufficient tenacity and adhesiveness.

A mixture of two parts by weight of vaseline (the common thick brown kind) and one part of melted india-rubber seems to combine the good qualities of both without the drawbacks of either.

The india-rubber should, of course, be pure (not vulcanised), and should be cut up into shreds and melted at the lowest possible temperature in an iron cup, being constantly pressed

down against the hot surface and stirred until a uniform glutinous mass is obtained. Then the proper weight of vaseline should be added, and the whole thoroughly stirred together.

This may be left on an air-pump plate for at any rate a couple of years without perceptible alteration either in itself or the brass.

H. G. MADAN

Eton College

Butterflies' Wings

CAN you inform me of any method of relaxing the wings of butterflies allowed to stiffen in the closed state?

Stretford, Manchester, July 1 J. M. B.

[If the butterflies are laid on damp sand under cover of a bell-glass or other air-tight covering they will soon relax so as to be fit for setting-out. A drop or two of carbolic acid on a sponge should be placed with them in order to prevent mouldiness. —ED.]

NOTE ON THE ABSORPTION SPECTRUM OF DIDYMIUM

IN a paper on "Radiant Matter Spectroscopy" (Part 2, Samarium),[1] I said that in fractionation of the didymium earths with ammonia—"After a time a balance seemed to be established between the affinities at work, when the earths would appear in the same proportion in the precipitate and in the solution. At this stage they were thrown down by ammonia, and the precipitated earths set aside to be worked up by the fusion of their anhydrous nitrates so as to alter the ratio between them, when fractionation by ammonia could be again employed."

That in most methods of fractionation a rough sort of balance of affinities beyond which further separation by the same method is difficult, appears to be a general rule. I have long noticed this action when fractionating with ammonia, with oxalic and nitric acids, and with formic acid. The valuable point which renders this fact noteworthy is that the balance of affinities revealed by fractionation is not the same with each method. It was in consequence of the experience gained in these different methods of fractionation that I wrote in my paper read before the Royal Society, June 10 last (*Chemical News*, vol. liv. p. 13), after saying that I had not been able to separate didymium into Dr. Auer's two earths, "probably didymium will be found to split up in more than one direction according to the method adopted."

In illustration of this I may mention that, although I have not split up didymium into the two earths, or groups of earths, which are described by Dr. Auer, other processes of fractionation give me, so to speak, other cleavage planes or lines of scission through the compound molecule didymium.

According to Dr. Auer, a line in the well-known yellow band, close to the soda line, but less refrangible (w.l. about 579), is a component of the absorption-spectrum of neodymium, and therefore, under all conditions, its intensity should follow the same variations as the other bands of neodymium in the blue (wave-lengths 482, 469, 444). Some of my didymium fractions, however, show that the line 579 does not follow the same law as the other bands I have named. Thus, in a rather low fraction (+ 6) of the didymium earths from gadolinite and samarskite I found that the neodymium line 579 was of the same degree of blackness as the adjacent praseodymium line in the yellow (wave-length about 571), but the bands in the blue of neodymium had almost disappeared. In the adjacent fractions of didymium I was enabled, by appropriate dilution, to keep this set of bands in the yellow as a standard, of exactly the same intensity; it was now seen that in successive fractions the intensities of the other more refrangible lines belonging both to neodymium and praseodymium varied greatly from strong to almost obliteration, the bands in the yellow always being kept of the same intensity.

Didymium prepared from a specimen of fluocerite differed somewhat from the other didymiums. Here the band 579 (ascribed to neodymium) was very strong, the band in the yellow of praseodymium (571) slightly weaker, and the bands in the blue of neodymium (482, 469, and 444) easily visible. On diluting the solution the bands in the blue of neodymium and the one component of praseodymium in the yellow (571) appeared to follow the same law in becoming fainter and fainter with dilution, whilst the other component band in the yellow of neodymium (579) remained unaffected.

It seems to me that a possible explanation of this variation might be founded on the great strength of the bands in the yellow, and that the two fractions of didymium then under examination might differ only in the fact that one was slightly stronger than the other. To test this hypothesis I took the two fractions first experimented on, and putting each into a wedge-shaped cell of glass viewed them together in the spectroscope. (1) I adjusted the wedges so that the group in the yellow appeared to be of the same intensity in each spectrum. On examining other parts of the spectrum it was seen that in one solution the bands in the green were tolerably strong, and the bands in the blue scarcely visible, whilst in the other solution the bands in the green were very faint, and those in the blue quite absent. (2) The position of the wedges was adjusted so that the bands in the green in each case should be of equal intensities. It was now seen that the alteration had greatly upset the balance of the bands in the yellow, the solution in which the bands in the green were faintest before, now having much stronger yellow bands than the other. The explanation mentioned above therefore falls through, and I see no other way of accounting for the facts except in the supposition that by the mode of fractionation then adopted, didymium had split up in a different manner to what it would have done if the method of Dr. Auer had been followed.

The colour of the different fractions of didymium nitrate varies from a dark rose-red at the more basic end (+ 17) to amber at the less basic end (+ 4). These variations in colour do not necessarily accompany a difference in the absorption-bands, for in one instance an amber and a rose-coloured salt were found to have almost identical spectra.

It would almost appear from these experiments, coupled with the facts I brought forward in last week's *Chemical News* (p. 14), that the "one band, one element" theory I lately advanced in connection with the phosphorescent spectrum of yttrium, may probably hold good in the case of the group of elements forming absorption spectra. According to this hypothesis, therefore, neodymium and praseodymium must not be considered as actual chemical elements, but only the names given to two groups of molecules into which the complex molecule didymium splits up by one particular method of fractionation.

WILLIAM CROOKES

[1] *Phil. Trans.*, Part 2, 1885, p. 706. A reprint of this paper is also commenced in No. 1390 of the *Chemical News*, p. 28.

HEATING AND COOKING BY GAS

A FEW years ago the public was led to believe that the use of coal-gas for lighting purposes was on its trial, and must shortly give way to the electric light. Threatened institutions live long, and even if coal-gas is destined to be eventually superseded by electricity for lighting purposes, a useful future is now opening out for it as a fuel offering many advantages over coal for domestic heating and cooking. In these fields it may possibly occur in the future that coal-gas—unless the price is everywhere considerably reduced—will have to encounter rivals such as the petroleum oils on the score of their cheapness, but at present, coal-gas, for cooking

and heating purposes, offers many facilities and advantages over any other kind of fuel.

Gas Cooking Stoves.—Those who remember the gas cooking stoves which were offered to the public even a few years ago, will acknowledge that the modern stoves now manufactured have reached a very high degree of perfection. In nearly all the larger kinds of stove intended for a family of six or more persons, the sides and top of the oven are constructed of double walls, and packed with a non-conducting fire-proof material—generally slag wool—so that but little heat escapes from the exterior of the stove to be lost by radiation; the internal surfaces of the stoves are usually enamelled, and are thus preserved from rust and decay, and easily kept clean, and in addition in some ovens, the racks for suspending the grids from which the meat is hung, slide out or turn out on a hinge, and are thus more easily cleaned than when fixed in the oven. On the tops of the stoves are placed burners for boiling kettles and saucepans, and for stewing, and an invertible burner is sometimes added, which can be rotated so as to bring the flame underneath when it is intended to grill. The following points may be enumerated as those in which cooking by gas possesses decided advantages over the ordinary kitchen range:—(1) There are no dust or cinders, and the whole process is more cleanly; (2) in some of the best stoves the oven can be heated up to a high temperature—sufficient for making pastry—in a few minutes only after the gas is lighted; (3) the different degrees of heat necessary for cooking various articles can be easily attained by limiting or increasing the supply of gas to the oven burners, or by increasing or diminishing the ventilation of the oven by opening or closing the flue-valve; and this is a point which good cooks will especially appreciate.

The principal arguments adduced by the opponents of gas cooking may be stated to be:—(1) That the cost is greater; (2) that joints of meat baked in gas ovens smell or taste of gas; (3) that the fumes and smell of cooking are more perceptible from gas ovens than from ordinary kitchen ranges; (4) that there is no supply of hot water with a gas oven; (5) that the gas stove does not warm the kitchen. We will now proceed to consider these objections *seriatim*.

(1) Although there can be no doubt that more heat is obtained from coal by burning the same value than from gas, still if attention is paid to the stove, and the gas is turned off as soon as the cooking is finished, for ordinary sized households the difference in cost between cooking by gas and cooking by coal is hardly appreciable.

(2) We may class gas ovens as of two kinds, A and B. In A, rings or rows of burners are placed at the bottom of the oven, and the air of the oven is heated up, this heated air and the products of combustion of the gas passing over and baking the meat. The burners used are usually those which give a luminous flame, for the reason that the luminous flame, although not itself of so high a temperature as the non-luminous flame from the atmospheric burner, yet radiates more heat. This greater radiation of heat is, like the luminosity, due to the separation of solid particles of carbon in the flame which become incandescent. Thus we see that the luminous flame radiates more heat to the air of the oven than the non-luminous. But it is in this class of oven especially that the baked meat smells or tastes of gas, as it is liable to become sodden with the steam and other products of combustion of the gas jets which pass over it, and no amount of ventilation of the oven will entirely cure this defect. In the other class of ovens, B, the burners are placed in rows at the bottom and along the sides of the oven walls. The oven walls are heated by the flames, and when hot radiate the heat to the joint of meat, which is thus baked by radiant heat as well as by hot air. The products of combustion of the gas jets pass up the sides of the oven and escape by the flue at the back without contaminating the meat. Atmospheric burners are almost invariably used in this class of oven, because the non-luminous flame is hotter than the luminous, and more quickly heats the oven wall, although less heat is radiated from the flame itself. The atmospheric burners have also this advantage, that the gas being mixed with twice its volume of air, the hydrogen and carbon are burnt at the same time, and no solid particles of carbon are formed, and thus there can be no soot from imperfect combustion, as so often happens in the luminous flame, in which the hydrogen of the hydrocarbons burns before the carbon, which is separated into small solid particles and strongly heated up before being finally burnt to carbonic acid. Consequently meat baked in this class of oven is not distinguishable from a joint roasted before an open fire.

(3) If a flue is carried up from the top of the back part of the oven into the kitchen chimney, the fumes from the oven cannot enter the general air of the kitchen. In all gas apparatus of whatever sort, some means must be provided for carrying off the products of combustion of the gas, and this is especially necessary in the case of gas cooking stoves. Ventilation of the oven is obtained by air passing in from below to ascend and escape with the products of combustion by the flue. The valve guarding the flue outlet is capable of regulating the ventilation, and is usually so constructed that it cannot entirely close the flue.

(4) The larger gas cooking stoves are now very usually supplied with boilers, which can be attached to the side of the stove, and can be heated below by a ring of atmospheric burners. [The burners at the top of the stove for boiling kettles and saucepans, making toast, grilling, and stewing, should also be atmospheric.] There can be no doubt that for heating a large supply of water, gas is not economical as compared with coal, but these boilers have this great advantage that they can be easily inspected and cleaned, and the fur—caused by the deposit of lime salts where the water to be heated is hard—can be easily removed. In towns and districts which are supplied with hard water (containing much carbonate of lime in solution), the ordinary kitchen boiler must be opened occasionally to remove the fur—a proceeding causing much inconvenience. If the fur deposit is allowed to accumulate too long an explosion may take place. This may happen in one of two ways; either the mouth of the supply pipe may become choked, cutting off the water from the boiler, or the boiler plates having become much heated, whilst the water in the boiler is cool owing to the intervention of a thick non-conducting layer of fur, if this deposit should crack, the cold water coming suddedly into contact with the red hot iron would cause a dangerous evolution of steam. The boilers sent out with gas cooking stoves can supply hot water for the kitchen only; they are not made to give a hot water supply under pressure available at any part of the house, as is the ordinary kitchen high-pressure boiler, so that for upstairs bath and lavatory purposes, hot water must be obtained from some form of gas bath-heater, of which we will speak presently.

(5) The gas stoves now made—being well packed and losing but little heat by radiation—certainly do not warm the general air of the kitchen as the kitchen fire does, and this negative quality in summer is a great advantage, as the kitchen remains cool instead of being at the usual unbearable temperature. In winter, if the kitchen fire is retained, this should be lighted early in the day until the room is warm, or some form of gas fire may be used—or it is even possible now to obtain a gas stove combining an open gas fire below, in front of which a small joint may be roasted, with a small gas oven above. The open gas fire will sufficiently warm a small kitchen.

The consumption of gas in a stove of the size required for a family of nine or ten persons varies from 15 to 20 cubic feet per hour (at an average pressure of 8/10) if the

oven burners alone are lighted and turned full on. In most cooking operations the amount of gas required would be only two-thirds of these quantities; the supply of gas being easily regulated to this or any other amount. If all the top burners in addition be lighted and turned full on, the average run of gas is from 40 to 60 cubic feet per hour. Twenty feet an hour for six hours a day is a fair representation of the amount of cooking required in a middle class family of ten persons. At 3*s*. per 1,000 cubic feet, this would entail an expenditure of 4·32*d*. per day, or 2*s*. 6¼*d*. per week, or 1*l*. 12*s*. 9¼*d*. per quarter. To raise a gallon of water in a copper boiler from 50° F. to 170° F., requires on the average a consumption of about 3 feet of gas, so that if very much hot water is required for culinary or domestic purposes the gas bill may be expected to show a corresponding increase.

Cooking by gas will not be introduced all at once. Gas stoves are now very generally obtained to supplement the kitchen range, for which purpose they are excellently adapted; and as their possibilities and advantages are more clearly appreciated they will no doubt come into more general use. We have indicated some of the chief points in their construction and management, and while we do not advise any one utterly to discard coal fires for cooking, we would recommend a trial of gas as being likely, where it can be obtained of good quality at moderate prices, and where the stoves will be treated with care and attention, to be found economical, cleanly, and useful.

Water and Bath-heaters.—In a house where gas is entirely used for heating and cooking, or where there is no high-pressure kitchen boiler connected with a hot water cistern by circulating pipes, capable of giving a supply of hot water on the upper floors, one of these appliances will be found very useful. There are numerous forms of this apparatus, and most of them are contrived in a very ingenious manner. The plan usually adopted is to receive the cold water at the top of the apparatus—which is of copper or copper tin-lined—where it is spread out in the form of spray or thin films to pass slowly down over surfaces of copper, receiving in its passage the necessary heat from gas burners below, to the bottom of the apparatus, where it flows out by a spout. The temperature of the issuing water will vary with the quantity of gas consumed and with the flow of the water, *i.e.* the amount passing through the apparatus in a given time. The object generally aimed at is to obtain a bath of 30 gallons of water at 100° F. in twenty minutes or thereabouts. For this purpose the water must be heated to about 105°, as when in the bath it gradually cools whilst this is filling. In the best forms of bath-heater, 25 to 30 cubic feet of gas must be consumed—at ordinary pressures, 7/10 to 10/10—to raise 30 gallons of water from 50° to 100° in 15 or 20 minutes. Here then we have an apparatus which at the cost of little more than 1*d*. is capable of providing ample material for a good warm bath. We would unhesitatingly recommend these bath-heaters, were it not the custom of most of the makers—with one or two exceptions however—to send them out without any flues or chimneys, and even sometimes to assert that no flue is necessary, as there is no smoke, and nothing unpleasant is produced by the combustion of the gas. There have however been some very unpleasant consequences from taking a bath in a small highly heated room, the air of which was loaded with carbonic acid—fainting and even partial asphyxia having been recorded under these circumstances. That the danger is no imaginary one will be seen when we consider that if in a room containing 500 cubic feet of space—the size of very many bath rooms—50 cubic feet of carbonic acid are produced by burning 25 cubic feet of coal-gas, the percentage of carbonic acid in the air is raised from ·04 to 10, and the entire oxygen of 200 cubic feet of air is destroyed. Fatal results have been known from the inhalation, even for a short period, of air containing 10 per cent. of carbonic acid. The temperature of the air of the room will also be very much raised, and will tend to help in the production of perhaps fatal syncope. We cannot then too strongly insist on the absolute necessity of providing a flue to carry off the products of combustion to the outer air of an apparatus which can produce such an enormous volume of carbonic acid in so short a space of time. The flue should be carried into a chimney with a good draught, as the escaping products are generally much cooled down by having parted with much of their heat to the water flowing through the apparatus. There are other varieties of water-heater constructed for various purposes, only one of which we are able to notice in the present article. This is a spiral water-heater for lavatories, the invention of Mr. Fletcher. In two minutes this little apparatus, at a cost of half a foot of gas, can raise nearly two quarts of water to 100° F. It is an ingenious contrivance, and free from the objections attending most of the larger apparatus described above.

Gas Fires.—These may be classified as radiation stoves, the room being heated entirely by radiation; and ventilation stoves, warm air issuing from the stove and displacing the colder air of the room. But many of these latter also warm the room by radiation from the incandescent asbestos or from the warm sides of the stove. Mr. Fletcher has calculated that with gas at 3*s*. per 1000 cubic feet, his open incandescent radiation gas fires cost for the same work about as much as coal fires when the coal is 30*s*. per ton, but with ventilating stoves the cost is about two-thirds of this. As in cooking by gas however, there are no dust, dirt, or cinders, and the fire can be immediately lighted or extinguished and requires no attention when alight. Nearly all the patterns of radiation stove now made depend on the heating of fibre or lump asbestos by non-luminous flames from atmospheric burners. The average consumption of gas required to maintain a room containing 5000 or 6000 cubic feet of space at a suitable temperature in winter, varies between 12 to 20 cubic feet per hour, depending on a large variety of circumstances. Most people when sitting in a room prefer to be warmed by radiant heat, as from an ordinary open coal fire, and to leave the ventilation of the room to accidental circumstances—which usually means a cold draught along the floor towards the fire. Ventilation stoves, if they fulfil the proper conditions, are certainly better adapted for warming large apartments, such as shops, workrooms, and halls, than radiation stoves. The conditions to be fulfilled are that the air be taken from a pure source in the outer atmosphere, that it be warmed by its passage through the stove, but not overheated or burnt—as is so often the case—and that it enter the room in an ascending direction towards the ceiling. In many cases it may be necessary that the air, rendered dry by its passage through the stove, should be moistened by passing over a tray of water before entering the general air of the room. Radiation stoves are perhaps better suited for private houses, especially for bed rooms and other apartments where a fire is only occasionally required. The flues of these stoves should open into the chimney at the back of the fireplace. The temperature of the air and products of combustion escaping through the flue will generally be found very high, but the heat thus lost is necessary to create a draught up the chimney, and assists in the ventilation of the room.

It has been said that the more general adoption of gas for heating and cooking would solve the smoke difficulty in London and those large towns where domestic and not factory smoke is the chief offender. A London pea-soup fog is certainly due to the coating of the particles of moisture suspended in the mist with "a carbonaceous sulphurous cuticle" as Mr. Harold Dixon has expressed it, and by preventing the daily evolution of millions of small particles of unconsumed carbon from our chimneys, we

should do away with the acrid yellow character of our fogs. But the mists due to the position of London on the estuary of a large river, would remain to the same extent as now, and there would still be the same amount of sulphurous acid given off into the air to be precipitated with the rain as sulphuric acid, and carry on its work of destruction on building stones and mortar. One cubic foot of coal-gas produces on combustion 0·2 to 0·5 grains of sulphurous acid, so that the amount evolved would continue to be, as now, enormous. Still the air would be deprived of its sooty particles to a great extent, and the old familiar features, characteristic of grimy London, might in time disappear. The carbonic acid which is the chief product in the combustion of coal-gas, is diffused at once into the general body of the atmosphere, and the marvellous rapidity with which this is effected is revealed to us when we know that the air of our open streets and parks differs only by the most minute quantities—if at all—in its contained carbonic acid, from the air of the mountains or the sea.

THE TOPOGRAPHIC FEATURES OF LAKE SHORES[1]

Introduction

THE play of meteoric agents on the surface of the land is universal, and there is a constant tendency to the production of the forms characteristic of their action. All other forms are of the nature of exceptions, and attract the attention of the observer as requiring explanation. The shapes wrought by atmospheric erosion are simple and symmetric, and need but to be enumerated to be recognised as the normal elements of the sculpture of the land. Along each drainage line there is a gradual and gradually increasing ascent from mouth to source, and this law of increasing acclivity applies to all branches as well as to the main stem. Between each pair of adjacent drainage lines is a ridge or hill standing about midway and rounded at the top. Wherever two ridges join there is a summit higher than the adjacent portion of either ridge; and the highest summits of all are those which, measuring along lines of drainage, are most remote from the ocean. The crests of the ridges are not horizontal, but undulate from summit to summit. There are no sharp contrasts of slope; the concave profiles of the drainage lines change their inclination little by little, and they merge by a gradual transition in the convex profiles of the crests and summits. The system of slopes thus succinctly indicated is established by atmospheric erosion under the general law of the interdependence of parts. It is the system which opposes the maximum resistance to the erosive agents.

The factor which most frequently, and in fact almost universally, interrupts these simple curves is heterogeneity of terrane or diversity of rock texture. Different rocks have different powers of resistance to erosion, and the system of declivities which, under the law of interdependence, adjusts itself to diversity of rock texture, is one involving diversity of form. Hard rocks survive, while the soft are eaten away. Peaks and cliffs are produced. Apices are often angular instead of rounded. Profiles exhibit abrupt changes of slope. Flat-topped ridges appear, and the distribution of maximum summits becomes in a measure independent of the length of drainage lines.

A second factor interrupting the continuity of erosion profiles is upheaval, and this produces its effect in two distinct ways. First, the general uprising of a broad tract of land affects the relation of the drainage to its point of discharge or to its base level, causing corrosion by streams to be more rapid than the general waste of the surface, and producing cañons and terraces. Second, a local uprising by means of a fault produces a cliff at the margin of the uplifted tract, and above this cliff there is sometimes a terrace.

A third disturbing factor is glaciation, the *cirques* and moraines of which are distinct from anything wrought by pluvial erosion; and a fourth is found in eruption.

The products of all these agencies except the last have been occasionally confused with the phenomena of shores. The beach-lines of Glen Roy have been called river terraces. The cliffs of the Downs of England have been ascribed to shore waves. Glacial moraines in New Zealand have been interpreted as shore terraces. Beach ridges in our own country have been described as glacial moraines, and fault terraces as well as river terraces have been mistaken for shore marks. Nevertheless, the topographic features associated with shores are essentially distinct from all others; and when their peculiar characters are understood there is little occasion for confusion. It is only where the shore record is faintly drawn that any difficulty need arise in its interpretation. In investigating the history of Lake Bonneville and other Quaternary water bodies of the Great Basin, the writer and his assistants have had constant occasion to distinguish from all others the elements of topography having a littoral origin and have become familiar with the criteria of discrimination. Their endeavour to derive from the peculiarities of the old shore lines the elements of a chronology of the lake which wrought them, has led them to study also the genesis of each special feature.[1]

In the discussion of shore phenomena there is little room for originality. Not only has each of the elements which go to make up the topography of a shore been recognised as such, but its mode of origin has been ascertained. There appears, however, to be room for a systematic treatment of the subject in English, for it is only in continental Europe that its general discussion has been undertaken. The writings of Elie de Beaumont include a valuable contribution,[2] and Alessandro Cialdi has devoted a volume to the motion of waves and their action on coasts.[3] These cover a large portion of the ground of the present essay, but treat the subject from points of view so diverse that the essay would be only partially superseded by their translation. The title of a work by H. Keller ("Studien über die Gestaltung der Sandküsten") indicates another discussion of a general nature, but this I have not seen. American and British contributions are contained chiefly in the reports of engineers on works for the improvement of harbours and the defence of coasts. The most comprehensive which has fallen under my eye, and one, at the same time, of the highest scientific character, is contained in the annual report of the United States Coast Survey for 1869, where Prof. Henry Mitchell, in treating of the reclamation of tide lands, describes the formation of the barriers of sand and shingle by which these are separated from the ocean.

It is proper to add that the writer became acquainted with these works only after the body of this essay was prepared. The objective studies on which his conclusions are based had been completed, and the discussion had acquired nearly its present shape before he became aware of the extent of the affiliated literature. His conclusions have, therefore, the quality of independence, and, so far as they coincide with those of earlier writers, have a corroborative value.

The engineering works whose construction has led to local investigations of shores are chiefly upon maritime coasts, where tides exert an important influence, and the literature of lake shores is comparatively meagre. It is

[1] From a paper by Mr. G. K. Gilbert in the "Fifth Annual Report of the Geological Survey of the United States for 1883-84." (Washington, 1885.)

[1] Partial outlines of the subject have been presented by the writer in connection with various accounts of Lake Bonneville, and a fuller outline was published by Mr. I. C. Russell in a paper on Lake Lahontan in the "Third Annual Report of the Geological Survey."

[2] "Leçons de géologie pratique;" tome premier; septième leçon, "Levées de sable et de galet," pp. 221-52.

[3] "Sul moto ondoso del mare e su le correnti di esso specialmente su quelle littorali pel comm" Alessandro Cialdi. Roma, 1866.

true that the phenomena of lake margins are closely paralleled by those of tide-washed coasts, but this, unfortunately, does not render the literature of the latter the more applicable, for there is a tendency to ascribe to the action of tides features which the students of inland lakes are compelled to account for independently of that agent.

It should be noted also that the point of view of the civil engineer is somewhat different from that of the present study. He is, indeed, concerned with all the forms into which the shore material is wrought by the action of the waves, but he is not at all concerned with their internal structure; and he knows them, moreover, only as subaqueous banks to be determined by sounding, and not at all as features of the dry land. The geologic student has, too, some facilities for study which the engineer lacks, for he is frequently enabled to investigate the anatomy of shore structures by means of natural cross-sections, while the engineer is restricted to an examination of their superficial forms.

Earth Shaping

The earth owes its spheroidal form to attraction and rotation. It owes its great features of continent and ocean bed to the unequal distribution of the heterogeneous material of which it is composed. Many of its minor inequalities can be referred to the same cause, but its details of surface are chiefly moulded by the circulation of the fluids which envelop it. This shaping or moulding of the surface may be divided into three parts—subaërial shaping (land sculpture), subaqueous shaping, and littoral shaping. In each case the process is threefold, comprising erosion, transportation, and deposition.

In subaërial or land shaping the agents of erosion are meteoric—rain, acting both mechanically and chemically, streams, and frost. The agent of transportation is running water. The condition of deposition is diminishing velocity.

In subaqueous shaping, or the moulding of surface which takes place beneath lakes and oceans, currents constitute the agent of erosion. They constitute also the agent of transportation; and the condition of deposition is, as before, diminishing velocity.

In littoral shaping, or the modelling of shore features, waves constitute the agent of erosion. Transportation is performed by waves and currents acting conjointly, and the condition of deposition is increasing depth.

On the land the amount of erosion vastly exceeds the amount of deposition. Under standing water erosion is either *nil* or incomparably inferior in amount to deposition. And these two facts are correlatives, since the product of land erosion is chiefly deposited in lakes and oceans, and the sediments of lakes and oceans are derived chiefly from land erosion. The products of littoral erosion undergo division, going partly to littoral deposition and partly to subaqueous deposition. The material for littoral deposition is derived partly from littoral erosion and partly from land erosion.

That is to say, the detritus worn from the land by meteoric agents is transported outward by streams. Normally it is all carried to the coast, but owing to the almost universal complication of erosion with local uplift, there is a certain share of detritus deposited upon the basins and lower slopes of the land. At the shore a second division takes place, the minor portion being arrested and built into various shore structures, while the major portion continues outward and is deposited in the sea or lake. The product of shore erosion is similarly divided. A part remains upon the shore, where it is combined with material derived from the land, and the remainder goes to swell the volume of subaqueous deposition.

The forms of the land are given chiefly by erosion. Since the wear by streams keeps necessarily in advance of the waste of the intervening surfaces, and since, also, there is inequality of erosion dependent on diversity of texture, land forms are characterised by their variety.

The forms of sea beds and lake beds are given by deposition. The great currents by which subaqueous sediments are distributed sweep over the ridges and other prominences of the surface and leave the intervening depressions comparatively currentless. Deposition, depending on retardation of currents, takes place chiefly in the depressions, so that they are eventually filled and a monotonous uniformity is the result.

The forms of the shore are intermediate in point of variety between those of the land and those of the sea bed; and since they alone claim parentage in waves, they are *sui generis*.

Ocean shores are genetically distinguished from lake shores by the co-operation of tides, which cannot fail to modify the work accomplished by waves and wind currents. The shores which constitute the objective basis of the present discussion were tideless; and the discussion is therefore limited to lake shores. It is perhaps to be regretted that the systematic treatment here proposed could not be extended so as to include all shores, but there is a certain compensation in the fact that the results reached in reference to lake shores have an important negative bearing on tidal discussions. It was long ago pointed out by Élie de Beaumont[1] and Desor[2] that many of the more important features ascribed by hydraulic engineers to tidal action, are produced on the shores of inland seas by waves alone; and the demonstration of wave-work pure and simple should be serviceable to the maritime engineer by pointing out the results in explanation of which it is unnecessary to appeal to the agency of tides.

CAPILLARY ATTRACTION

THE heaviness of matter had been known for as many thousand years as men and philosophers had lived on the earth, but none had suspected or imagined, before Newton's discovery of universal gravitation, that heaviness is due to action at a distance between two portions of matter. Electrical attractions and repulsions, and magnetic attractions and repulsions, had been familiar to naturalists and philosophers for two or three thousand years. Gilbert, by showing that the earth, acting as a great magnet, is the efficient cause of the compass needle's pointing to the north, had enlarged people's ideas regarding the distances at which magnets can exert sensible action. But neither he nor any one else had suggested that heaviness is the resultant of mutual attractions between all parts of the heavy body and all parts of the earth, and it had not entered the imagination of man to conceive that different portions of matter at the earth's surface, or even the more dignified masses called the heavenly bodies, mutually attract one another. Newton did not himself give any observational or experimental proof of the mutual attraction between any two bodies, of which both are smaller than the moon. The smallest case of gravitational action which was included in the observational foundation of his theory, was that of the moon on the waters of the ocean, by which the tides are produced; but his inductive conclusion that the heaviness of a piece of matter at the earth's surface, is the resultant of attractions from all parts of the earth acting in inverse proportion to squares of distances, made it highly probable that pieces of matter within a few feet or a few inches attract one another according to the same law of distance, and Cavendish's splendid experiment verified this conclusion. But now for our question of this evening. Does this attraction between any particle of

[1] "Leçons de Géologie pratique," Paris, 1845, v. i. p. 232.
[2] "Geology of Lake Superior Land District," by Foster and Whitney, Washington, 1851, v. ii. pp. 262, 266.

matter in one body and any particle of matter in another continue to vary inversely as the square of the distance, when the distance between the nearest points of the two bodies is diminished to an inch (Cavendish's experiment does not demonstrate this, but makes it very probable), or to a centimetre, or to the hundred-thousandth of a centimetre, or to the hundred-millionth of a centimetre? Now I dip my finger into this basin of water; you see proved a force of attraction between the finger and the drop hanging from it, and between the matter on the two sides of any horizontal plane you like to imagine through the hanging water. These forces are millions of times greater than what you would calculate from the Newtonian law, on the supposition that water is perfectly homogeneous. Hence either these forces of attraction must, at very small distances, increase enormously more rapidly than according to the Newtonian law, or the substance of water is not homogeneous. We now all know that it is not homogeneous. The Newtonian theory of gravitation is not surer to us now than is the atomic or molecular theory in chemistry and physics; so far, at all events, as its assertion of heterogeneousness in the minute structure of matter apparently homogeneous to our senses and to our most delicate direct instrumental tests. Hence, unless we find heterogeneousness and the Newtonian law of attraction incapable of explaining cohesion and capillary attraction, we are not forced to seek the explanation in a deviation from Newton's law of gravitational force. In a little communication to the Royal Society of Edinburgh twenty-four years ago,[1] I showed that heterogeneousness does suffice to account for any force of cohesion, however great, provided only we give sufficiently great density to the molecules in the heterogeneous structure.

Nothing satisfactory, however, or very interesting mechanically, seems attainable by any attempt to work out this theory without taking into account the molecular motions which we know to be inherent in matter, and to constitute its heat. But so far as the main phenomena of capillary attraction are concerned, it is satisfactory to know that the complete molecular theory could not but lead to the same resultant action in the aggregate as if water and the solids touching it were each utterly homogeneous to infinite minuteness, and were acted on by mutual forces of attraction sufficiently strong between portions of matter which are exceedingly near one another, but utterly insensible between portions of matter at sensible distances. This idea of attraction insensible at sensible distances (whatever molecular view we may learn, or people not now born may learn after us, to account for the innate nature of the action), is indeed the key to the theory of capillary attraction, and it is to Hawksbee[2] that we owe it. Laplace took it up and thoroughly worked it out mathematically in a very admirable manner. One part of the theory which he left defective—the action of a solid upon a liquid, and the mutual action between two liquids—was made dynamically perfect by Gauss, and the finishing touch to the mathematical theory was given by Neumann in stating for liquids the rule corresponding to Gauss's rule for angles of contact between liquids and solids.

Gauss, expressing enthusiastic appreciation of Laplace's work, adopts the same fundamental assumption of attraction sensible only at insensible distances, and, while proposing as chief object to complete the part of the theory not worked out by his predecessor, treats the dynamical problem afresh in a remarkably improved manner, by founding it wholly upon the principle of what we now call potential energy. Thus, though the formulas in which he expresses mathematically his ideas are scarcely less alarming in appearance than those of Laplace, it is very easy to translate them into words by which the whole theory will be made perfectly intelligible to persons who imagine themselves incapable of understanding sextuple integrals. Let us place ourselves conveniently at the centre of the earth so as not to be disturbed by gravity. Take now two portions of water, and let them be shaped over a certain area of each, call it A for the one, and B for the other, so that when put together they will fit perfectly throughout these areas. To save all trouble in manipulating the supposed pieces of water, let them become for a time perfectly rigid, without, however, any change in their mutual attraction. Bring them now together till the two surfaces A and B come to be within the one-hundred-thousandth of an inch apart, that is, the forty-thousandth of a centimetre, or two hundred and fifty micro-millimetres (about half the wave-length of green light). At so great a distance the attraction is quite insensible: we may feel very confident that it differs, by but a small percentage, from the exceedingly small force of attraction which we should calculate for it according to the Newtonian law, on the supposition of perfect uniformity of density in each of the attracting bodies. Well known phenomena of bubbles, and of watery films wetting solids, make it quite certain that the molecular attraction does not become sensible until the distance is much less than 250 micro-millimetres. From the consideration of such phenomena Quincke (*Pogg. Ann.*, 1869) came to the conclusion that the molecular attraction does become sensible at distances of about fifty micro-millimetres. His conclusion is strikingly confirmed by the very important discovery of Reinold and Rücker that the black film, always formed before an undisturbed soap bubble breaks, has a uniform or nearly uniform thickness of about eleven or twelve micro-millimetres. The abrupt commencement, and the permanent stability, of the black film demonstrate a proposition of fundamental importance in the molecular theory:—The tension of the film, which is sensibly constant when the thickness exceeds fifty micro-millimetres, diminishes to a minimum, and begins to increase again when the thickness is diminished to ten micro-millimetres. It seems not possible to explain this fact by any imaginable law of force between the different portions of the film supposed homogeneous, and we are forced to the conclusion that it depends upon molecular heterogeneousness. When the homogeneous molar theory is thus disproved by observation, and its assumption of a law of attraction augmenting more rapidly than according to the Newtonian law when the distance becomes less than fifty micro-millimetres is proved to be insufficient, may we not go farther and say thet it is unnecessary to assume any deviation from the Newtonian law of force varying inversely as the square of the distance continuously from the millionth of a micro-millimetre to the remotest star or remotest piece of matter in the universe; and, until we see how gravity itself is to be explained, as Newton and Faraday thought it must be explained, by some continuous action of intervening or surrounding matter, may we not be temporarily satisfied to explain capillary attraction merely as Newtonian attraction intensified in virtue of intensely dense molecules movable among one another, of which the aggregate constitutes a mass of liquid or solid.

But now for the present, and for the rest of this evening, let us dismiss all idea of molecular theory, and think of the molar theory pure and simple, of Laplace and Gauss. Returning to our two pieces of rigidified water left at a distance of 250 micro-millimetres from one another. Holding them in my two hands, I let them come nearer and nearer until they touch all along the surfaces A and B. They begin to attract one another with a force which may be scarcely sensible to my hands when their distance apart is fifty micro-millimetres, or even as little as ten micro-millimetres; but which certainly becomes sensible when the distance becomes one micro-millimetre, or the fraction of a micro-millimetre; and enormous, hundreds or thousands of kilogrammes' weight, before they come into absolute contact. I am supposing the area of each

[1] *Proceedings* of the Royal Society of Edinburgh, April 21, 1862 (vol. iv.).
[2] Royal Society *Transactions*, 1709-13.

of the opposed surfaces to be a few square centimetres. To fix the ideas, I shall suppose it to be exactly thirty square centimetres. If my sense of force were sufficiently metrical I should find that the work done by the attraction of the rigidified pieces of water in pulling my two hands together was just about four and a half centimetre-grammes. The force to do this work, if it had been uniform throughout the space of fifty micro-millimetres (five-millionths of a centimetre) must have been nine hundred thousand grammes weight, that is to say, nine-tenths of a ton. But in reality it is done by a force increasing from something very small at the distance of fifty micro-millimetres to some unknown greatest amount. It may reach a maximum before absolute contact, and then begin to diminish, or it may increase and increase up to contact, we cannot tell which. Whatever may be the law of variation of the force, it is certain that throughout a small part of the distance it is considerably more than one ton. It is possible that it is enormously more than one ton, to make up the ascertained amount of

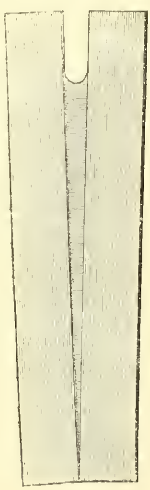


Fig. 1.

work of four and a half centimetre-grammes performed in a space of fifty micro-millimetres.

But now let us vary the circumstances a little. I take the two pieces of rigidified water, and bring them to touch at a pair of corresponding points in the borders of the two surfaces A and B, keeping the rest of these surfaces wide asunder (see Fig. 1). The work done on my hands in this proceeding is infinitesimal. Now, without at all altering the law of attractive force, let a minute film of the rigidified water become fluid all over each of the surfaces A and B: you see exactly what takes place. The pieces of matter I hold in my hands are not the supposed pieces of rigidified water. They are glass, with the surfaces A and B thoroughly clean and wetted all over each with a thin film of water. What you now see taking place is the same as what would take place if things were exactly according to our ideal supposition. Imagine, therefore, that there are really two pieces of water, all rigid, except the thin film on each of the surfaces A and B, which are to be put together. Remember also that the Royal Institution, in which we are met, has been, for the occasion, transported to the centre of the earth so that we are not troubled in any way by gravity. You see we are not troubled by any trickling down of these liquid films—but I must not say *down*, we have no up and down here. You see the liquid film does not trickle along these surfaces towards the table, at least you must imagine that it does not do so. I now turn one or both of these pieces of matter till they are so nearly in contact all over the surfaces A and B, that the whole interstice becomes filled with water. My metrical sense of touch tells me that exactly four and a half centimetre-grammes of work has again been done; this time, however, not by a very great force, through a space of less than fifty micro-millimetres, but by a very gentle force acting throughout the large space of the turning or folding-together motion which you have seen, and now see again. We know, in fact, by the elementary principle of work done in a conservative system, that the work done in the first case of letting the two bodies come together directly, and in the second case of letting them come together by first bringing two points into contact and then folding them together, must be the same, and my metrical sense of touch has merely told me in this particular sense what we all know theoretically must be true in every case of proceeding by different ways to the same end from the same beginning.

William Thomson

(*To be continued.*)

THE TOTAL SOLAR ECLIPSE, 1886 *AUGUST* 28–29

The Eclipse Expedition will leave England on the 29th inst. in the Royal Mail Steamship *Nile*, timed to arrive at Barbados on August 11. We regret to learn that Her Majesty's ship *Canada*, which was told off to assist the Expedition, chiefly by supplying artificers and assistance in camping and in the observations, has been withdrawn on some "diplomatic" service. This is a serious blow to the probabilities of good results.

From data supplied by Mr. Hind, the following details have been computed for the Island of Grenada:—

	Latitude N.	Longitude W.	Commencement of totality G.M.T.	Commencement of totality Local time
	° ′	° ′	h. m. s.	h. m. s.
Levera	12 13·5	61 37	23 17 19	19 10 51
Caliveny	12 0·0	61 43	23 17 14	19 10 22
Point Saline ...	12 0·5	61 48	23 17 10	19 9 58
Fort Frederick.	12 3·0	61 44	23 17 13	19 10 17

	Duration of totality	Sun's Azimuth	Sun's True altitude	Angle from N. point
	m. s.	° ′	° ′	
Levera	3 45	84 12	18 56	87° to W.
Caliveny	3 52	84 6	18 48	73° ,,
Point Saline ...	3 48	84 4	18 42	72° ,,
Fort Frederick.	3 49	84 3	18 46	77° ,,

The sun's altitude and azimuth and the angle from N. point are given for the commencement of totality.

The time of first contact for the middle of the island [assumed lat. 12° 6′·0, long. 61° 43′·0] is 18h. 11m. 55s. local mean time at 77°·0 N. to W. on the sun's limb; and ends at 20h. 20m. 44s. at 105° N. to E. on the limb.

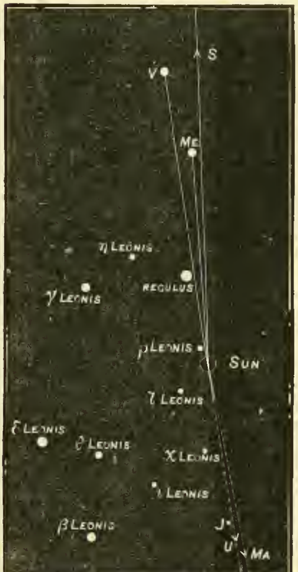
A diagram is given below showing the position of the principal stars and planets at the commencement of totality. The distances of the planets from the sun are very roughly as follows (the positions of Mercury and Venus being shown absolutely, and the directions of the others indicated by arrows):—

Mercury (Me) = 4	Mars (Ma) = 15
Venus (V) = 6	Saturn (S) = 12

Jupiter (J), Uranus (U) almost in conjunction = 8.

Local mean time of transit of Polaris and δ Ursæ Minoris for Caliveny (Grenada), long. 61° 43′ W.:—

Date 1886		Transit of δ Ursæ Minoris h. m. s.		Transit of Polaris h. m. s.
August 14	...	8 35 46	...	15 43 24
15	...	8 31 49	...	15 39 28
16	...	8 27 52	...	15 35 33
17	...	8 23 55	...	15 31 37
18	...	8 19 58	...	15 27 41
19	...	8 16 1	...	15 23 46
20	...	8 12 4	...	15 19 50
21	...	8 8 7	...	15 15 54
22	...	8 4 11	...	15 11 58
23	...	8 0 14	...	15 8 3
24	...	7 56 17	...	15 4 7
25	...	7 52 20	...	15 0 12
26	...	7 48 23	...	14 56 15
27	...	7 44 26	...	14 52 20
28	...	7 40 30	...	14 48 24
29	...	7 36 33	...	14 44 28



HORIZON

Diagram of configuration of stars and planets during the total solar eclipse, 1886 August 28-29, for Grenada. V = Venus; ME = Mercury; MA = Mars; J = Jupiter; S = Saturn; U = Uranus.

We reprint from *Science* the following paper by Mr. J. Norman Lockyer:—

In order to obtain the greatest amount of assistance from observations of the eclipsed sun, it is necessary to consider in the most general way the condition of solar inquiry at the time the observations are made. If any special work commends itself to those interested in the problem,—work which may be likely to enable us to emphasise or reject existing ideas,—then that work should take precedence of all other.

Next, if the observers are sufficient in number to undertake other work besides this, then that work should be arranged in harmony with previous observations; that is, the old methods of work should be exactly followed, or they should be expanded so that a new series of observations may be begun in the light and in extension of the old ones.

In my opinion, and I only give it for what it is worth, the three burning questions at the present time—questions on which information is required in order that various forms of work may be undertaken to best advantage (besides eclipse-work)—are these:—

(1) The true constitution of the atmosphere of the sun. By this I mean, Are the various series of lines of the same element observed in sunspots, *e.g.*, limited to a certain stratum, each lower stratum being hotter, and therefore simpler in its spectrum, than the one overlying it? and do some of these strata, with their special spectra, exist high in the solar atmosphere, so that the Fraunhofer lines, represented in the spectrum of any one substance, are the result of an integration of the various absorptions from the highest stratum to the bottom one? This view is sharply opposed to the other, which affirms that the absorption of the Fraunhofer lines is due to one unique layer at the base of the atmosphere.

I pointed out before the eclipse of 1882 that crucial observations could be made during any eclipse, including the time both before and after totality. I made the observations: they entirely supported the first view, but I do not expect solar inquirers to throw overboard their own views until these observations of mine are confirmed; and I think one of the most important pieces of work to be done during the next eclipse is to see whether these observations can be depended upon or not.

One observer, I think, should repeat the work over the same limited region of the spectrum, near F; another observer should be told off to make similar observations in another part of the spectrum. I have prepared a map of the lines near E, for this purpose, showing those brightened on the passage from the arc to the spark, and those visible alone at the temperature of the oxy-hydrogen flame. Whereas some of the spark lines will be seen seven minutes before and after totality as short, bright lines, some of the others will be seen as thin, long lines just before and after totality. We want to know whether the lines seen at the temperature of the oxy-hydrogen flame will be seen at all, and, if so, to what height they extend.

(2) The second point to which I attach importance is one which can perhaps be left to a large extent to local observers, if the proper apparatus, which may cost very little, be taken out.

With this eclipse in view, I have for the last several months gone over all the recorded information, and have discussed the photographs taken at the various eclipses in connection with the spots observed, especially at those times.

The simple corona observed at a minimum with a considerable equatorial extension (twelve diameters, according to Langley), the complex corona observed at maximum when the spots have been located at latitudes less than 20°, have driven me to the view, which I shall expand on another occasion, that there is a flattened ring round the sun's equator, probably extending far beyond the true atmosphere; that in this ring are collected the products of condensation; and that it is from the surfaces of this ring chiefly that the fall of spot-forming material takes place.

If we take any streamer in mid-latitude, we find, that, while the spots may occur on the equatorial side of it, none are seen on the poleward side. I regard the streamers, therefore, like the metallic prominences, as a sequel to the spot; and there is evidence to suggest that a careful study will enable us to see by what process the reaction of the photosphere and underlying gases produced by the fall of spot-material tends to make the spot-material discharge itself in lower and lower latitudes, as the temperature of the sun's lower atmosphere gets enormously increased.

The observations of Profs. Newcomb and Langley at the minimum of 1878, on the equatorial extension, are among the most remarkable. Prof. Newcomb hid the

moon and 12′ of arc around it at the moment of totality by a disk of wood, carefully shielding his eyes before totality. Prof. Langley observed at a very considerable elevation. It is therefore quite easy to understand why this ring has not been seen or photographed at maximum. At maximum no precautions have been taken to shield the eye; no observations have been made at a considerable elevation; while the fact that the ring, if it exists, consists of cool material, fully explains how it is that the photographic plates have disregarded it.

I would propose, therefore, that the repetition of Prof. Newcomb's observations of 1878 be made an important part in the arrangements of the eclipse for this year. A slight alteration in the method will be necessary, as the ring will be near the vertex and the lowest point of the eclipsed sun.

(3) Another point of the highest importance at the present moment has relation to the existence of carbon. Until Tacchini's observations of 1883, the only trace of carbon in the solar spectrum consisted of ultra-violet flutings. He observed other flutings in the green near the streamers in the eclipse referred to.

Duner's recent work puts it beyond all doubt that stars of Class III. *b* have their visible absorption produced chiefly by carbon vapour.

On any theory of evolution, therefore, we must expect the sun's atmosphere to be composed to a large extent of carbon at some time or other; so that the highest interest attaches to this question in connection with the height in the atmosphere at which the evidence of carbon is observed. The existence of the ultra-violet flutings among the Fraunhofer lines tells nothing absolute about this height, although I inferred, at the time I made the announcement, that it existed at some height in the coronal atmosphere.

These three points, then, are those to which I attach special importance at the present time.

We next come to photographs of the corona. I believe, that, with our present knowledge, the chief thing we have to seek in such photographs is not merely the streamers and their outlines, which we are sure to get anyway, but images on a larger scale; so that in a series of short exposures we may endeavour to get some records which will eventually help us in determining the directions of the lower currents. At present we do not know absolutely whether these flow to or from the poles. My own impression is that the panaches at the poles indicate an upper outflow.

In coming to the photo-spectroscopic observations, I am of opinion, that of the two attacks which I first suggested for the eclipse of 1875, and which have also been used in the last two eclipses of 1882 and 1883, one of them should be discarded, and the whole effort concentrated on the other.

We have learned very much from the use of the prismatic camera,—one of the instruments referred to; but the results obtained by it are not of sufficient accuracy to enable them to be fully utilised. On the other hand, though the slit spectroscope failed in 1875, it succeeded with a brighter corona and more rapid plates in 1882; and, with a proper reference spectrum, every iota of the facts recorded can be at once utilised for laboratory work and subsequent discussion.

On these grounds, then, I would suggest that slit spectroscopes alone be used for photographic registration. I think falling plates should be used, and that the work should begin ten minutes before totality, and continue till ten minutes after; provided the slit be tangential, or nearly so, to the limb.

I may state that arrangements have been made here to take such a series of photographs on the uneclipsed sun; and, with the improved apparatus, I am greatly in hopes that we may get something worth having.

This paper was communicated to the Eclipse Committee, and formed in part the basis for the plan of operations on this occasion, which, as approved by the Committee, are as follows:—

Coronagraph before and after totality.. / Camera and prismatic camera during totality	Capt. Darwin
Camera and slit spectroscopes ... .. / Integrated intensity of corona	Capt. Abney
Camera and slit spectroscopes	Dr. Schuster
,, ,, ,,	Mr. Maunder
Observations of chromosphere before and after totality, and search for carbon bands during totality	Rev. S. J. Perry
Observations of chromosphere before and after totality, and direction of solar currents during totality	Mr. Turner
Images of corona on large and small scale (2 inches and ¾ inch) with photoheliograph and a 6-inch object-glass by Henry	Mr. Lockyer

Prof. Thorpe replaces Capt. Abney in the above list, and Prof. Tacchini joins the expedition at the invitation of the Royal Society.

NOTES

We regret to learn of the death of Dr. Abich, the eminent Russian geologist.

Mr. David Stephenson, of Edinburgh, the well-known civil engineer, died at North Berwick on Saturday last. He was born in 1815, and was a son of Mr. Robert Stephenson, the celebrated engineer of the Bell Rock and other lighthouses. His abilities in his profession were soon recognised. He was appointed at an early age engineer to the Lighthouse Board, and while occupying that position he constructed a number of important lighthouses. In the course of his career he held the office of consulting engineer to the Highland and Agricultural Society and to the Convention of the Royal Burghs, as also engineer to the Board of Fisheries and the Clyde Lighthouse Trust. Mr. Stephenson was a voluminous writer; his more important works included "A Sketch of Civil Engineering in North America," "The Application of Modern Hydrometry to the Practice of Civil Engineering," "Reclamation and Production of Agricultural Land," and "Principles and Practice of Canal and River Engineering." He was an occasional contributor to the columns of Nature.

The death is announced of Mr. Charles Mano, seven days after leaving Colon for France, at the age of fifty-five. M. Mano had made various journeys in Spanish America for scientific purposes. In Mexico he discovered several ancient cities which had never before been seen by any European. He was the scientific Commissioner of the Governments of Colombia and of Guatemala.

The arrangements for the Brighton meeting of the British Medical Association on the 10th, 11th, 12th, and 13th proximo are rapidly approaching completion. In the section of pathology, the new science of bacteriology will receive a good deal of attention, and microscopic photographs of these mysterious organisms will be shown by Dr. Heneage Gibbes and Dr. Crookshank, while the latter will also exhibit the various organisms growing in gelatine, &c.

We learn from the *Sidereal Messenger* for July that the contract for mounting the 36-inch objective has been awarded by the Lick trustees to Warner and Swasey, of Cleveland, O., for 42,000 dols. The telescope is to be 57 feet long; the diameter of the tube 42 inches. Provisions are made by which it will be possible for the observer at the eye-end of the telescope to command all the possible motions, and these same motions can also be controlled by an observer stationed on a small balcony

20 feet above the floor. It is expected that the mounting will be completed in April 1887, and that the glass will be brought to Mount Hamilton and put in place some time during the summer following. The total cost of the equatorial and dome will will be about 164,850 dols.; the cost of the dome being 56,850 dols.; the mounting, 42,000 dols.; the visual objective, 53,000 dols.; the additional photographic lens, 13,000 dols.

We have received a copy of the address of Sir William Manning, as Chancellor of the University of Sydney, at the annual commemoration. The report which it contains is one of progress in almost every direction. The death of Prof. Smith, who had long held the Chair of Experimental Physics, led to a re-arrangement of duties, a Professorship of Physics being substituted, with a wider and different range of teaching in Physical Science, including portions of the duties before discharged by the Professor of Mathematics as Professor also of Natural Philosophy. The list of private benefactions appended to the address is a remarkable one. It amounts to 317,414*l.* 12*s.* 6*d.* Of this, one amount, the Challis Bequest, is estimated at 180,000*l.*, and is anticipated to reach about 200,000*l.* As this noble donation has only recently fallen into possession, its application has not yet been fixed; the only point determined about it is that no part of it shall be used on buildings of any kind, but the capital shall be kept intact to produce an income for direct educational purposes. Another highly important gift is the Macleay Natural History Collection, valued at 25,000*l.* A building has been erected to receive the collection, and an endowment of 6000*l.* for a Curator has been promised. The other gifts include one of 30,000*l.* for the library. The amount of the donations since 1879 exceed a quarter of a million sterling—a magnificent sum for any community, however wealthy, to contribute in a few years to a single educational institution.

On Thursday last week the Photographic Exhibition, promoted by the Glasgow Town Council, was opened in the Corporation Galleries with a numerously attended *conversazione.* It is the fullest exposition, historical, practical, and scientific, of the art of photography which has yet been given. By means of an admirable series of examples it illustrates the development of photography from the earliest attempts of Wedgewood, Niepce, Daguerre, Fox-Talbot, and numerous other discoverers, to the latest products of those who are acknowledged at the present day as masters of the art. In the department of photo-lithography the numerous methods of photo-engraving and phototype-printing are fully represented by means of exhibits from the principal workers in that line. One of the most interesting sections is that which illustrates the applications of photography to the various branches of science, divided into its relations to geography, ethnology, microscopy, meteorology, and astronomy. In the last of these, the greatest of the recent triumphs in celestial photography by the Brothers Henry, of the Paris Observatory, are admirably shown; and there are also splendid examples of a similar kind from the Royal Observatory, as well as from Mr. A. Ainslie Common and others eminent in that field. The apparatus range from the primitive appliances of Daguerre to the latest ones of Messrs. Mason and Co., Glasgow; Mr. Stanley, London; and Mr. Marion, of the same city. Mr. James Paton, the curator of the galleries, has superintended the arrangements for the exhibition, which are of a most satisfactory nature.

We regret to learn of the probable early recall of the Commissioner of the Philippine Forest Department, and the practical suspension of the work in which he is engaged. The step is much to be regretted on many grounds, and it is to be hoped the Spanish Government will re-consider its decision in the matter. Until recently our knowledge of Philippine vegetation was extremely scanty, notwithstanding the collections made by the late Mr. Hugh Cuming. Even these it remained for Don Sebastian Vidal, Commissioner of Forests there, to place in accessible form, the materials for his recently-published "Phanerogamæ Cumingianæ Philippinarum" having been collected whilst engaged in working up his collections at Kew some two or three years ago. The extensive collections recently made by the Forest Department, a portion of which has been transmitted to Kew for determination, has, we believe, yielded a considerable proportion of novelties, including a number of genera not hitherto known from the islands. Information respecting these additions will probably be forthcoming in due course, as already we have an outline of the flora at the hands of one of the Kew staff. The above, together with the fact that the large island of Mindanao, and several others, is practically unexplored, shows how much yet remains to be done in this direction. From an economic stand-point, and for the development of the natural resources of the islands, the work of the department is an important one. The demand for timber, owing to the exhaustion of the forests in various directions, is assuredly forcing the forestry question into the foreground. As an example of how little we know of the Philippine flora, we may mention the St. Ignatius's bean, of which until recently nothing was known beyond the fact that it finds its way into the markets of this country as a source of the deadly poison strychnine, and was said to be sold in the market at Manila. Now, we believe, the plant has been discovered, and information respecting it will doubtless be shortly forthcoming. Such matters as these naturally engage the attention of the Forest Department, and it will be a matter for sincere regret if the work so well begun should come to a sudden termination, just at a time when its importance is beginning to be realised.

A Philippine correspondent, writing on May 24 last, informs us that the great volcano, Mayon, in the south of the Island of Luzon, is in eruption. He remarks:—"I tried the ascent, and climbed to about 5000 feet, when incandescent stones and ashes obliged me to come quickly down. I crossed a patch of forest—*Litsea verticillata*, *Myrica vidaliana*, and *Vaccinium* abundant—half burnt and covered with ashes. The sight was magnificent, but not much botanical work to be done there. I never saw anything like it as a sublime scene of devastation; ashes and stones and smoke everywhere, and a fearful noise like heavy artillery all around." *Myrica vidaliana*, it may be remembered, was described only about a year ago, from specimens collected at this very spot. At present it has not been found elsewhere, though it probably exists on other volcanic peaks in the island.

The *Melbourne Argus* of June 11 gives some particulars of the eruption of Mount Tarawera, in New Zealand, which was briefly reported by telegram. The first news of the outbreak was received at Auckland from the telegraphist at Rotorua on the morning of June 10. He said:—"We have all passed a fearful night here. The earth has been in a continual quake since midnight. At 2.10 a.m. there was a heavy quake and a fearful roar, which made every one run out of their houses. A grand yet terrible sight for those so near as we were presented itself. Mount Tarawera, close to Lake Rotomahana, suddenly became an active volcano, belching out fire and lava to a great height. The eruption appears to have extended itself to several places southward. A dense mass of ashes came pouring down here at 4 a.m., accompanied by a suffocating smell as from the lower regions. An immense black cloud, which extended in a line from Tapeka to Pairoa Mountain, was one continued mass of electricity all night, and is still the same. The thunder-like roaring of three or four craters, the stench, and the continual quaking of the earth, had the effect of completely frightening people." Things became so threatening that the telegraphist deemed it prudent to abandon his post; but he afterwards returned. At Wairoa the schoolhouse was fired by the light-

ning and smothered in mud and stones, and two hotels were reduced to ruins. Twenty bodies were recovered. For about six miles north of Te Awamutu the whole of the surrounding country was covered with blue mud 3 feet deep. It was reported that all Rotomahana had disappeared. Many natives lost their lives; but the exact number is not known. The sounds of the explosion were heard at Hamilton, about eighty miles distant, early in the morning. They were like great guns at sea. The windows of houses in Hamilton were shaken. At Maketu there was darkness until 10 a.m. The earthquakes lasted from 2.30 a.m. till 8.15, with very strong lightning and earth-currents. Four volcanoes were going at Wairoa. The Tikitapu bush has been uprooted. All the country down to Tauranga was in total darkness, with thick clouds of sulphurous matter and gypsum in the air. The following description of the scene was given in a message from Taupo:—"At 3 a.m. a terrific report aroused the sleeping inhabitants of Taupo, when an immense glare of a pillar-shaped light was observed to the north-north-east. A great black cloud hung over this pillar, concave on the under-side, and convex on the upper, whilst meteors on all sides shot out from the cloud in every direction, shedding an unearthly bluish light. Loud reports, accompanied by very heavy shocks of earthquake, followed in quick succession, and kept on until 6 o'clock, when the daylight and the clouds of ashes rendered the sight invisible. At 2.15 a.m. the two extinct volcanoes of Ruawhai and Tarawera threw an immense column of flame and smoke into the heavens. Molten lava and hot mud were rained abroad, while huge rocks and masses of fire went up and around in all directions. The earthquakes were terrible. Tongariro is quiet. Heavy snow is falling on the ranges and the cold is intense. The rumbling still continues at Maketu, and dust is still falling. The whole country is covered from 1 to 6 inches with dust."

THE series of anthropoid apes at the Zoological Society's Gardens at the present time is well worthy of attention. Besides "Sally," the bald-headed chimpanzee (*Anthropopithecus calvus*), which has now been two years in the Regent's Park, there is a second chimpanzee of the ordinary species (*A. troglodytes*), which enables these two forms to be compared side by side. A young orang (*Simia satyrus*) has likewise recently arrived, and a white-handed gibbon (*Hylobates lar*), from Malacca, deposited by Mr. Dudley Hervey, Resident Councillor of the Straits Settlements, exemplifies the third type of the highest division of the Quadrumana. It is much to be wished that the long-talked-of plan of building a new compartment by the side of the existing monkey-house for the Anthropoids could be carried out. At present these highly interesting animals are not very conveniently lodged along with the sloths and ant-eaters, on the other side of the Gardens.

THE half-yearly general meeting of the Scottish Meteorological Society will be held to-day, when the following papers will be read:—"The Extent of the Areas of the different Mean Annual Rainfalls over the Globe," by Mr. John Murray; "On the Temperature of the Water in the Firth of Clyde and connected Lochs," by Dr. Hugh Robert Mill, F.R.S.E.

MR. FRANK E. BEDDARD, Prosector of the Zoological Society, has been appointed Lecturer on Biology at Guy's Hospital.

ACCORDING to the programme of the approaching celebration of the 500th anniversary of the foundation of Heidelberg University, a grand historical procession designed and to be personally directed by Prof. Carl Hoff, of the Karlsruhe School of Art, will march through the town on August 6, starting at 9 a.m. More than 900 persons with 300 horses and 14 state coaches will take part in the procession, which is to give a pictorial representation of the five centuries which have succeeded the foundation of the University, and to comprise the following groups:—(*a*) Founding of the University by Elector Ruprecht I., 1386; (*b*) public entry of Frederich the Conqueror after the battle of Seckenheim, 1462; (*c*) nurture of science and art by Elector Otto Heinrich, 1556–59; (*d*) life among the people of the Merry Palatinate at the end of the 16th century: procession illustrating the vintage of the Palatinate; (*e*) entry of the Elector Frederick V. with his consort, Elizabeth of England, June 17, 1613; (*f*) Bohemian Embassy, 1619; (*g*) time of the Thirty Years' War (1618–48), and of the War of the Orleans Succession (1688–97); (*h*) Elector Karl Ludwig, with retinue, 1632–80; (*i*) time of the Elector Karl Philipp, 1716–42: hunting cavalcade; (*k*) Elector Karl Theodor, 1742–99; (*l*) Restoration of the University by Karl Friedrich of Baden, 1803: the students of the nineteenth century; (*m*) the Burschenschaften; (*n*) the Corps; (*o*) the new German Empire. Judging by the arrangements now nearly completed, the procession may be expected to surpass all previous exhibitions of the kind in the splendour of its equipage and the historical truth of its representation, which will be carried into even its minutest details. For the sake of a proper view of the procession, stands are to be erected at all convenient points along the line of the procession, and the sale of tickets for the numbered seats of the stands has already begun. A plan of the procession, issued by the firm Koester, Heidelberg, (price 20 Pfennige) shows the arrangement of the stands, with the prices of the various seats, and gives information respecting hotel accommodation, &c. A very considerable number of lodgings, we learn, have already been engaged by strangers intending to be present at the ceremonies connected with the celebration. All intending visitors who have not yet secured accommodation in respect of board and lodging are invited to make early application to the Commission specially appointed for the negotiation of such business—Wohnungs Commission, Rathhaus, Heidelberg. Beds are still to be had at the moderate price of 15 marks for the whole term of the celebration, while hotel-keepers, &c., have publicly engaged to keep their prices within strictly reasonable limits.

AT the Conference of the Colonial and Indian Exhibition, held on the 30th ult., Prof. Fream read a paper on "Colonial Forestry," dealing with the present condition of forestry in the larger colonies. In Canada there is need of conservation and of tree-planting, and everything now seems ripe for the establishment of a department of forest conservancy in the Dominion. In New South Wales such a department is at work under the Ministry of Mines; in Victoria a considerable area is reserved, but even this is not commensurate with the demand for timber for industrial purposes. In South Australia, Queensland, and New Zealand, efficient forestry departments exist. In Australia and the Cape Colony, English forest trees are being successfully cultivated, and "in all the colonies the reckless waste and wanton destruction of former days have given place to wise systems of conservancy, such as are worthy of a tree-loving people."

A SERIES of photographic views from a balloon has been taken by M. Nadar, of Paris, whose father, twenty-five years ago, was the first to attempt photographing from a balloon, with only partial success. The stereotype plates of the views taken were presented to the Academy of Sciences at their meeting on July 12.

SEVERAL attempts have lately been made by the Marquis of Lorne to transmit live whitefish (*Coregonus albus*), which have been reared by the National Fish Culture Association, to the Isle of Mull, where his lordship is endeavouring to acclimatise this valuable American species. After several futile attempts two consignments of them have reached their destination in safety. Great difficulty attends the operation of removing white

fish from one place to another. The best carrier for removing them in is an ordinary carboy filled to the top with water. Not more than fifty specimens of yearling fish should be placed in one carrier. The autumn is the best time for transmitting them.

WE are informed by Mr. W. August Carter, of the Colonial and Indian Exhibition Fisheries Section, that a large specimen of a smooth hound, recently imported into the aquarium of the Exhibition from Brighton, gave birth last week to ten young ones, this species being viviparous. She did not deliver them simultaneously, but two at a time, at intervals of about twenty hours, occupying six days in yielding the entire number. All the young on appearing were perfectly formed, and resembled in every respect their matured congeners with the exception of the colour of the upper portion of the body and fins, which was white throughout instead of grey. Unfortunately nine expired shortly after birth, lacking the conditions necessary to their existence, such as deep water, where in their natural state they always repair for six months during their alevin stage. The remaining fish was devoured by its parent, which is in excellent condition and moves actively around the tank.

FROM the report of the Stockholm Observatory for the last year, we learn that during the year Prof. Gyldèn continued the calculations for the development of certain theories respecting the chief planets, and that they are so far advanced as to already embrace the terms of the first and second orders in relation to the masses pertaining to the theory for the system Jupiter-Saturn-Uranus. The Astronomer-Royal also continued his lectures on theoretical astronomy, chiefly supported by King Oscar, which were attended by several eminent foreign astronomers. Several well-known astronomers from Russia and Germany have also pursued their studies at the observatory during the year, two of whom, Drs. Shdanow and Harzer, of Pulkowa, having, as the result of the same, published important papers on the astronomical theory of perturbation. Three more parts of the work, "Astronomical Observations and Researches at the Stockholm Observatory," were issued during the year.

BEFORE adjourning this summer the Swedish Parliament granted a sum of 325*l*. towards the continuation of the *Acta Mathematica* during the ensuing financial year 1886-87.

WE have received Nos. 45–47 of the first part, 34–36 of the second part, of the well-known and valuable "Encyclopædia of Natural Sciences," now in course of publication by the house of Eduard Trewendt, of Breslau. The three numbers of the first part include the seventeenth number of the "Manual of Botany," containing the beginning of an important note by Prof. Oscar Drude, of Dresden, on "The Systematic and Geographical Arrangement of the Phanerogams," illustrated with finely executed drawings by the author, and a map. The two other numbers belong to the "Alphabetical Manual of Zoology, Anthropology, and Ethnology," advancing that work from "Kalunda" to "Landrace." Nos. 34 and 36 of the second part carry on the "Alphabetical Manual of Chemistry" from "Essigsäure" to "Furfurangruppe." Of special interest is the excellent work by Dr. R. Nietzki, of Basel, on "Organic Colouring Materials." The 35th number, again, brings us nearer to the conclusion of the "Alphabetical Manual of Mineralogy, Geology, and Palæontology," containing, as it does, palæontological contributions by Dr. Fr. Rolles—Trias System, Birds, Wanderings of Plants and Animals in the Course of Geological Epochs, Mollusks, and Worms, as also mineralogical contributions by the Editor. The few articles that still remain to be written on geology, interrupted by the sudden death of Prof. von Lasaulx, will be taken up by Prof. Hœrnes, of Graz, so that this "Handwörterbuch" will be completed in the course of this summer.

THE Prince of Monaco has left Lorinet with his yacht *Hirondelle* to prosecute the series of marine observations begun last year. The cruise will be made between Cape Finisterre and the English southern coast. Five hundred tubes have been prepared, and will be thrown on the sea. They will carry printed forms of the same kind described on a former occasion. Dredgings and thermometric readings will be made on the bottom of the sea.

THE administration of the Jardin des Plantes of Paris has organised an exhibition of the objects collected on the Congo by M. Savorgnan de Brazza.

THE Franco-Algerian telegraph system is being completed to Biskra, but the communication between Biskra and Tugurth and Dabila oasis, situated at a great distance to the south, is kept up by means of the optical telegraph, the sun being utilised in daytime, and at night electricity. The optical system will be always kept in operation, as it is apprehended that nomads might cut the wires.

THE Kazan Society of Naturalists has issued the fourteenth volume of their *Memoirs* (*Trudy*), which contains a very interesting paper, by MM. Stuckenberg and Vysotski, on the Stone Age at Kazan. The commencement of a museum of Stone Age implements at Kazan was made in 1877, and now it already has a first-rate collection, both in the number and variety o implements, and M. Zausailoff is publishing a beautiful atlas of drawings representing them. The paper of MM. Stuckenberg and Vysotski contains most interesting details as to the places where remains of man were found, illustrated by three maps. Three different terraces are seen in the valley of the Volga. The upper terrace, rising 50 to 150 feet above the second, consists of yellow-brownish sandy clay, covering layers of sand. It contains remains of mammoths and other extinct mammals. The second terrace is much more recent ; and it is on its surface, as well as on the slopes of the former, and sometimes on the surface of the third terrace, that the stone implements are found. The third terrace, which is still inundated by the Volga, was probably almost covered by its waters during the Stone Age. All implements found are Neolithic ; that is, they belong to what we should call the lacustrine period. As to the implements themselves, many of which are figured on the sixteen plates which accompany the paper, they have mostly been made of the local flint originating from the Permian deposits. A few are made of the Eocene sandstone which extends to the south of the Kazan Government ; and, finally, boulders of granite, diorite, gneiss, quartzite, and so on, have also been used for the fabrication of some of the hammers. Broken pottery, together with bones of horses, oxen, and pigs, accompany the stone implements.

M. PALMIERI, the director of the Vesuvian Observatory, has succeeded in exhibiting the negative electricity developed when steam is condensed by cold, and positive electricity liberated when evaporation takes place. A platinum shell is placed in communication with one of the plates of a condenser. The golden leaf is separated when a piece of ice is placed in the shell, and also when it is full of water if exposed to the rays of the sun. The electricity has been proved positive in the first instance, and negative in the second.

DURING the last few weeks great tracts of the fertile island of Seeland, in Denmark, have been devastated by maybugs, whole fields and meadows having been laid quite bare. Last year the damage done was very great, but this year it is far worse, being estimated at some 25,000*l*. The distress among farmers is in consequence very great.

A MIRAGE was observed at Algiers prior to the outbreak of the destructive thunderstorm which broke over the city on the 7th

inst. Cape Matifan appeared from Algiers close at hand with a sharply cut rock of granite at its extremity. The temperature was 43°·2 C. in the shade, showing that the air above the sea was very hot, and that the explanation of the phenomenon is to be found in the same causes as those determining a mirage in the Sahara. The lowering of the temperature was very rapid, falling as much as 2° C. at Bouzarcah Observatory. The 7th inst. was the hottest day that has yet been felt there this season. Lightning struck the Government barrack at Mustapha, and ignited piles of hay, inflicting damage to the extent of 4000*l*.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*) from India, presented by Mr. F. W. Steward; a Ring-tailed Lemur (*Lemur catta*) from Madagascar, presented by Mrs. Collcutt; six Prairie Marmots (*Cynomys ludovicianus*) from North America, presented by Mr. F. J. Thompson; two Common Foxes (*Canis vulpes*) from Russia, presented by Mr. Harrison Cripps, F.R.C.P.; a Common Rhea (*Rhea americana*) from South America, presented by Mr. J. W. Bell; four Red-bellied Squirrels (*Sciurus variegatus*) from Trinidad, presented by Mr. R. J. Lichmere Guppy; two Peba Armadillos (*Tatusia peba*) from South America, presented by Mr. J. Clements; a Greater Black-backed Gull (*Larus marinus*), British, presented by Mr. Henry Stevens, M.D.; twenty-four Sand-Lizards (*Lacerta agilis*), a Slowworm (*Anguis fragilis*), a Common Snake (*Tropidonotus natrix*) from Germany, presented by Mr. S. Schaefer; two Sarus Cranes (*Grus antigone*) from North India, eight European Tree Frogs (*Hyla arborea*) from Germany, purchased; two Long-fronted Gerbilles (*Gerbillus longifrons*), an Elliot's Pheasant (*Phasianus ellioti*), a Bronze-winged Dove (*Phaps chalcoptera*), a Barred-shouldered Dove (*Geopelia humeralis*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

SCHULHOF'S RESEARCHES ON THE ORBIT OF COMET 1873 VII. (COGGIA—WINNECKE).—The elements of Comet 1873 VII. bear a certain resemblance to those of Comet 1818 I., which was observed by Pons. Prof. Weiss asserts the identity of these two comets, and adopts sixty-two years as the most probable value of the period of revolution. In the *Bulletin Astronomique*, tome iii. p. 125 *et seq.* M. L. Schulhof has published a most exhaustive discussion of the orbit of Comet 1873 VII., and has gone into the question of its possible identity with 1818 I., as well as with 1457 I. (the observations of which by Toscanelli have recently been discussed by Prof. Celoria) in a most thorough manner. The opinion which he expresses, with some reserve, as the result of his investigations, is that the Comets 1873 VII. and 1818 I. are distinct bodies with a short period of revolution, having a common origin. The Comet 1457 I. is probably identical with 1873 VII., but it is also possible that the two comets, 1873 VII. and 1818 I. are fragments of 1457 I., which must have been a much more conspicuous object than either of them, to have been seen by Toscanelli and by the Chinese with the naked eye.

SOLAR ACTIVITY DURING THE FIRST HALF OF 1886.—The numbers and areas of sunspots have shown upon the whole a decided falling off during the past half-year as compared with the last six months of 1885, although no month of the present year has shown so low a daily average as December 1885. There has been, however, a steady increase in the number of days on which the sun's disk was free from spots, one side of the sun being, on the average, much less spotted than the other, causing an apparent short period in the variation of the spotted area, of about a synodic rotation of the sun in duration. The month in which the mean daily number of sunspots was least was February; that in which it was most was March. An exceedingly fine group was observed on May 8.

Prominences have shown fewer fluctuations in their numbers and size, but have been fully one-fourth less numerous on the average than in 1885.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *JULY* 25–31

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on July 25

Sun rises, 4h. 15m.; souths, 12h. 6m. 14·6s.; sets, 19h. 57m.; decl. on meridian, 19° 38′ N.: Sidereal Time at Sunset, 16h. 11m.

Moon (one day after Last Quarter) rises, 23h. 35m.*; souths, 6h. 39m.; sets, 13h. 54m.; decl. on meridian, 11° 23′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	6 50	13 46	20 42	10 6 N.
Venus	1 37	9 46	17 55	22 22 N.
Mars	10 59	16 35	22 11	5 18 S.
Jupiter	9 47	15 54	22 1	0 32 N.
Saturn	2 43	10 51	18 59	22 15 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich)

July	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
27	85 Tauri	6	0 4	0 49	90 224
27	σ^2 Tauri	5	3 6	3 43	10 297
27	σ^1 Tauri	$5\frac{1}{2}$	3 23	near approach	334 —

July	h.	
25	5	Mercury at greatest distance from the Sun.
28	22	Venus in conjunction with and 0° 6′ south of μ Geminorum.
28	23	Venus in conjunction with and 3° 46′ north of the Moon.

Variable Stars

Star	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·2	81 16 N.	July 28,	22 51	*m*
Algol	3 0·8	40 31 N.	,, 28,	1 47	*m*
			,, 30,	22 36	*m*
δ Libræ	14 54·9	8 4 S.	,, 31,	21 22	*m*
R Scorpii	16 10·9	22 40 S.	,, 31,		*M*
U Ophiuchi	17 10·8	1 20 N.	,, 27,	23 52	*m*
W Sagittarii	17 57·8	29 35 S.	,, 26,	0 0	*m*
β Lyræ	18 45·9	33 14 N.	,, 26,	2 0	m_2
η Aquilæ	19 46·7	0 43 N.	,, 25,	0 0	*M*
δ Cephei	22 24·9	57 50 N.	,, 25,	21 30	*M*

M signifies maximum; *m* minimum; m_2 secondary minimum.

Meteor Showers

The principal shower is that of the *Aquarids*, maximum July 28; radiant R.A. 340°, Decl. 13° S. Other showers are as follows:—The *Andromedes* (I.), R.A. 8°, Decl. 36° N.; near χ Persei, R.A. 32°, Decl. 53° N.; near β Ursæ Majoris, R.A. 165°, Decl. 53° N.; and near the Pole, R.A. 300°, Decl. 87° N.

ON LAYING THE DUST IN MINES

IN a paper recently contributed to the South Wales Institute of Engineers,[1] Mr. Archibald Hood, the President, says:—

"It was probably first suggested by Faraday and Lyell about the year 1845 that coal-dust was in some way inflammable. This idea was subsequently set forth by several French engineers, but all that was done previous to the year 1875 bears the same relation to subsequent demonstrations as the steam-engine of Hero of Alexandria bears to the steam-engine of the nineteenth century."

Assuming Mr. Hood's date to mark correctly the commencement of the real battle between the new theory and its predecessors, it cannot surely be urged that the period of ten years which has since elapsed has been too long wherein to destroy the vast herd of previously existing chimeras, and to introduce and establish a new and different order of ideas. Doubtless the result attained up to the present has been prodigiously accelerated by the labours of the Royal Commission on Accidents in Mines, and of the

[1] "On the Watering of Dusty Mines." The South Wales Institute of Engineers, March 18, 1886.

similarly constituted bodies in France and Germany, all of which have been called into existence and have completed their labours within the period named. Indeed, scarcely had the ink with which the English Report was written been dry when the Home Office introduced a new Mines' Regulation Bill which provides, amongst other things, that "*In all dry and dusty mines the airways and travelling roads are to be kept clear of dust* OR *well watered, and a shot is not to be fired until the place and that near it is cleared of dust and then well watered*" ("*Mining Journal*").

The crudeness of the idea embodied in the first alternative, which appears to contemplate the possibility of removing the dust from roadways and airways without the simultaneous use of water, reminds one of an incident of the interview between Christian and the Interpreter ("Pilgrim's Progress"):—

"Then he took him by the hand and led him into a very large parlour that was full of dust because never swept; the which, after he had reviewed it a little while, the Interpreter called for a man to sweep. Now when he began to sweep the dust began so abundantly to fly about that Christian had almost therewith been choked. Then said the Interpreter to a damsel that stood by, 'Bring hither water and sprinkle the room,' the which, when she had done, it was swept and cleansed with pleasure."

It has all the appearance of being a compromise between efficiency on the one hand and ignorance or prejudice on the other, and closely resembles, in this respect, the first General Rule of the Act for the Regulation and Inspection of Mines, 1860 (23 and 24 Vic., cap. 151), according to which a mine was required to be ventilated only in such a way as to be safe *under ordinary circumstances*. But just as these qualifying words were found to be a cloak for all kinds of inefficiency in the matter of ventilation, and had to be ultimately expunged after a twelve years' trial, so we venture to predict will this other unscientific alternative, if passed into law, cause endless trouble and disaster, and require to be similarly dealt with at some future time.

To lay the dust sufficiently well to prevent the spread of an explosion requires a much smaller quantity of water than appears to be generally supposed.

This has been stated more or less directly several times in describing the results of coal-dust experiments; but it was very clearly brought out in the examination of the workings of Pochin Colliery, in Monmouthshire, after the great explosion in November 1884. The flame which in that case had all but filled the mine, and had penetrated into the remotest parts of three districts of workings ventilated by separate air-currents, was found to have been arrested by a slight dampness on one of the roadways leading to several working places. A cask conveying water from a dip place to a point more convenient to the pumps was hauled along this roadway four times every twenty-four hours, and it was stated by the manager of the colliery at the time that the dampness in question was due simply to accidental leakages from this cask and not to any intentional application of water for the purpose of laying the dust. At the inquest on Mardy explosion also, in January last, it was pointed out that a similar accidental or irregular system of watering appeared to have stopped the flame in four different directions, and to have saved the lives of many of the workmen (*Western Mail*, January 21, 1886).

Systematic watering with the avowed object of preventing the spread of explosions has hitherto been practised in very few collieries in this country. Llwynypia Colliery in the Rhondda Valley is a notable exception. Soon after the earliest coal-dust experiments had been made there in 1875 the intelligent proprietors and manager constructed a number of water-tanks on wheels, each provided with a perforated pipe at the back like an ordinary watering cart. Some of these were intended to be drawn by horses along the less frequented roadways, others to be attached to the trains of waggons which are drawn along the underground railways by means of wire ropes actuated by engine-power. The result of watering by this means was satisfactory and remarkable. The whole mine became cooler and more pleasant to live in. The dust, as such, disappeared not only from the floor of the roadways but also from the timbers and from the ledges formed by the irregular projections in the side-walls, and became consolidated into a firm, compact, and slightly humid mass under foot.

On their first arrival in this country in 1880, MM. Pernolet and Aguillon, who were sent by the Commission du Grisou to study the state of the English mines, expressed the opinion then generally held, that watering the floor of a dry mine would leave ample supplies of dust on the timbers and side-walls to carry on an explosion once begun. But after seeing the actual results in Llwynypia Colliery with their own eyes they altered their views considerably, as will appear from the following extract from their Report, which describes this incident of their visit:—

"*Ainsi, à Llwynypia, où les chantiers se développent jusqu'à* 1500 *mètres du puits, et où l'extraction est de* 550 *tonnes par jour avec un seul poste, il suffit par jour de* 5 *wagons a'une capacité d'un demi-mètre cube, soit de* 4·500*m. d'eau. Nous avons pu constater que les galeries étaient partout très propres et l'atmosphère très épurée, bien que cette mine passât auparavant pour une de cells où l'atmosphère était le plus chargée et le boisage le plus recouvert de poussières.*"[1]

About a year and a half ago the Home Office began unexpectedly to prosecute the managers of a few widely separated mines in different parts of the country for firing blasting shots while the men ordinarily employed were underground. The practice of blasting under these conditions had been going on unchallenged ever since the passing of the Coal Mines' Regulation Act, 1872, and it was with a feeling somewhat akin to consternation that the colliery owners viewed the new reading then for the first time seriously sought to be attached to part of one of the General Rules. The manager of the Standard Steam Coal Colliery in South Wales was selected out of hundreds of others in the same predicament, and a prosecution against him was begun. The colliery owners of the district rallied round the Monmouthshire and South Wales Collieries' Association, and undertook the defence. Happily, however, for the ends of justice, as well perhaps as for the cause of science, the case soon became involved in a whirlpool of legal formalities from which, as far as present appearances go, it is little likely to escape until after the passing of the new Mines' Bill.

During the earlier stages of this prosecution the representatives and advisers of the owners met the Inspector of Mines for the district (the late Mr. T. E. Wales) and asked him to represent to the Home Office that they were prepared immediately to submit to a new rule compelling them to water their mines systematically if the objectionable interpretation of the shot-firing rule were withdrawn. At the same time they expressed the opinion that the rule they were themselves proposing would afford a real protection to the lives of the miners, and that the one they desired to be superseded had been founded upon a misapprehension of the true causes of explosions. This intelligent proposal was, however, allowed to fall to the ground, and the Juggernaut of office rolled on its ponderous and relentless course.

Where simple tanks on wheels are difficult or expensive to manipulate, they may be advantageously replaced by a system of pipes bringing water from the surface, or from a reservoir at a convenient height in the shaft, and distributing it at different points of the workings, in the form of a fine spray. This arrangement has been successfully applied both at Llwynypia and Standard Collieries. At the latter colliery the pressure of water at the bottom of the shaft is regulated to fifty pounds on the square inch. The water pipes, which are one inch and a half in diameter, lie on the floor at one side of the roadway, or are supported on timber as the case may be. At distances of fifty yards apart upright branch pipes rise vertically from the main to a height of about four feet, each provided with a leaden plug with one minute hole. The jets of water are directed horizontally across the roadway, and the spray is carried along in the air-current, moistening the floor, more or less, all the way from one jet to the next. The cost of first establishment is stated to be about 5*l.* per hundred yards, and the cost of maintenance to be almost *nil.*

If the dew-point of the air entering a mine were by any simple means raised to the normal temperature of the strata in which the workings are situated, it is obvious that no system of watering would be necessary, and that any desirable degree of dampness could be maintained in the roadways. The only objection to this method is, that it would necessitate raising the general temperature of all dry mines.

A slight dampness, such as that which prevails in shallow mines at all times, is sufficient to lay the dust effectually; and it is highly probable that, so soon as anything approaching this condition is maintained also in deep mines, we shall have heard the very last of "Great Colliery Explosions."

W. GALLOWAY

[1] P. 287, "Exploitation et Réglementation des Mines à Grisou en Belgique, en Angleterre et en Allemagne." Rapport de Mission fait à la Commission chargée de l'étude des moyens propres à prévenir les explosions de grisou dans les Houillères, par MM. A. Pernolet et L. Aguillon, "Angleterre." Paris, 1881.

THE SUN AND STARS[1]

IX.

LET us consider the case, then, on the supposition of small masses of matter. Where are we to find them? The answer is easy;—in those small meteoric masses which an ever-increasing mass of evidence tends to show occupy all the realms of space.

In connection with this, perhaps I may be permitted to quote the following from one of my "Manchester Lectures":—

"There is one point to which I think I may be permitted to draw your attention, although at present it rests merely upon an unindorsed observation of my own. I thought it would be worth while to try what would happen if I inclosed specimens of meteorites, taken at random, in a tube from which I subsequently exhausted the air by a pump. After the pumping had gone on for some considerable time, of course we got an approach to a vacuum; and arrangements were made by means of which an electric spark could pass along this apparent vacuum, and give us the spectra of the gases evolved from the meteorites. Taking those precautions which are generally supposed to give us a spark of low temperature, and passing the current, we got a luminous effect which, on being analysed by the spectroscope, gave us that same spectrum of hydrocarbon which Mr. Huggins, Donati, and others have made us perfectly familiar with as the spectrum of the head of a comet. There, then, we get the atmosphere of meteorites, not necessarily carbonaceous meteorites, but meteorites taken at random; and this atmosphere is exactly what we get in the head of a comet.

"Now let me go one step further; and to take that step with advantage, allow me to refer to another point, . . . that whereas Schiaparelli has connected meteorites and falling stars with comets, Profs. Tait and Thomson, on the other hand, have connected comets with nebulæ, both of them being, according to those physicists, clouds of stones. Now how has one to carry these spectroscopic observations into the region of the nebulæ? A Leyden jar was included in the circuit, and we had what is generally supposed to be an electric current giving us a very much higher temperature than we had before. What, then, was the spectrum? The spectrum, so far as the known lines were concerned, was the spectrum which we get from the nebulæ; for the hydrocarbon[2] spectrum, which we get from the atmospheric meteorites at a low temperature, was replaced by the spectrum of hydrogen; the spectrum of hydrogen coming, of course, from the decomposition of the hydrocarbon, with the curious, but at present unexplained, fact that we got the spectrum indications of hydrogen without indications of carbon. In my laboratory work I have come across other curious cases in which compound vapours, when dissociated, only gave us one spectrum at a time—by which I mean that in a vapour consisting of two well-known substances, under one condition we only get the spectrum of one substance, and under another condition we get the spectrum of the other substance alone; so in others, again, of both combined. The evidence seems, therefore—though I do not profess to speak with certainty—entirely in favour of the ideas of Sir William Thomson and Prof. Tait on the one hand, and of Schiaparelli on the other."

I have given the above extract to show that a mass of meteorites at a temperature higher than that found to exist in a comet's head could give us the hydrogen spectrum which was discovered with such richness in the *Nova*, which is represented in the spectra of most nebulæ, and which remained in the spectra of the *Nova* after all the other lines had gone.

These considerations enhance the interest of the *Nova* to the spectroscopist if we accept the bright line observed in the star by Dr. Copeland and others to be veritably the chief nebula line.

This line brightened relatively with each decrease in the brilliancy of the hydrogen lines. On December 8, 1876, it was much fainter than F, while by March 2, 1877, F was a mere ghost by the side of it. On any probable supposition the temperature must have been higher at the former date.

Now it is well known that within certain limits the lines in the spectrum of a compound body get brighter with *decrease* of temperature, because at the higher one the compound almost entirely ceases to exist as such, and we get the lines of its constituents. It is a fair theory then to suggest that the famous nebula line may belong to a compound. Nay the fact as it stands alone further points to the possibility that the compound in question contains hydrogen as one of its constituents.

At present we know very little indeed about these new stars. The star which appeared last year in the nebula in Andromeda is, if possible, still more difficult to understand, because, although it was so near the centre of the nebula in apparent position, we do not know that it was near the nebula locally, or whether it was simply in the line of sight. Therefore the views with regard to that star are much complicated by the fact that it is uncertain whether it was associated with a nebula. It may have had nothing to do with it. I have received this morning from Paris a photograph taken by the Brothers Henry, who are working now at the Paris Observatory, recording the very interesting discovery that apparently growing out of the side of one of the stars of the Pleiades is a real nebula. Those of you who remember the photographs of the corona during different eclipses will imagine that there may be some connection between this star and the surrounding nebula. Now it seems certain that there is some connection between this star and the nebula, and it may be that, in fact, what we call nebula in this case is a very considerable expansion of the star's coronal atmosphere. So obvious is that suggestion, that I spent last night in trying to observe its spectrum. The fog was too much for me, but still, although there was very little light, it did look very much as if there were some bright lines in the bright part of the spectrum of the star. And if that is so, it will not only show you the possible connection of the nebula in Andromeda with the new star in Andromeda, but it also shows you the importance of the question of area which I brought before you in the previous part of the lecture, if the bright lines we got are due, not to the star itself, but to the incandescent area which surrounds it.

Finally, then, with regard to the new stars generally. That they are stars in our sense is, I think, quite impossible. Some of them, you know, lose their brilliancy in a very few weeks. Now we know that any body like the star which we are most familiar with—our sun—if ever it got to a sufficient degree of temperature to increase its light ten or twenty times, would not lose its temperature in ten or twenty days, or ten or twenty years, or ten or twenty thousand years, so that the more rapidly any of these bodies cool down, the less likely is it that the bodies which cool down have any considerable mass.

So obvious was that that on the appearance of the star in 1866 I made the suggestion, as I have said, that the body which gave us this light might be quite close to us. Well that was negatived. It was found that it was not—that it was at a stellar distance—that it was no more possible to tell its distance than it was that of an ordinary star.

We are driven then to the conclusion that, as we must account for a tremendous increase of light, and we know that this light was produced at a very great distance, and that one very large mass cannot be in question, we must distribute the light among a great many masses—the idea of a collision between two stars must give way to the idea of some action of a meteor-swarm in the case of a star already existing, like T Coronæ, and of a collision between two meteor-swarms in the case of a new one like that of 1876; that seems a possible explanation of a great many of these "stars"—the components of meteor-streams driven to incandescence supplying that light in consequence of the innumerable multitude of their components, the light dying out very quickly because these innumerable components are small and far apart.

The next order of variable stars to which I shall refer you is very well represented by β Lyræ. The curious point about this star is that it has a double minimum. Another star, η Argûs, with a bright line spectrum, is also remarkable from the fact that its maximum varies in the same sort of way.

In this star we get a number of differences. If you start from the maximum of the star you find it of three-and-a-quarter magnitude. It then in three-and-a-half days goes down to four-and-a-quarter. It then goes up so that in about six days it gets back again to its original brilliancy. It then goes down again, but does not stop where it did before, but goes half a magnitude lower, and then at last it ends the sequence of phenomena by getting up to its original brightness in thirteen days.

Now, although we cannot explain how it is, we have the fact that a curve of that kind is associated with a bright line spectrum. In η Argûs, one of the most remarkable stars in the heavens, we have very much the same conditions. This star is in the southern hemi-

[1] A Course of Lectures to Working Men delivered by J. Norman Lockyer, F.R.S., at the Museum of Practical Geology. Revised from shorthand notes. Continued from p. 230.

[2] The Lectures from which I am quoting were delivered many years ago, before the spectrum was recognised to belong to carbon.

sphere, and during the last twenty or thirty years a considerable discussion has been going on among astronomers as to whether the surrounding nebula is or is not changing its position with regard to the star. Now what happens to the star? I may tell you that the curve is only a rough one. But still you see the point fairly enough. This is, that this star, which has a bright line spectrum like β Lyræ, has a period not of thirteen days, but of seventy years. We find that the star, which is at first possibly below a sixth magnitude star, rises up to the first magnitude, but then goes down to the second, and so on. The curve shows a period of seventy years, the curve being very irregular.

The third and fourth classes, so far as we can see, resemble our sun. The curves suggest that of the sun's spot period, when we can make anything out of them at all.

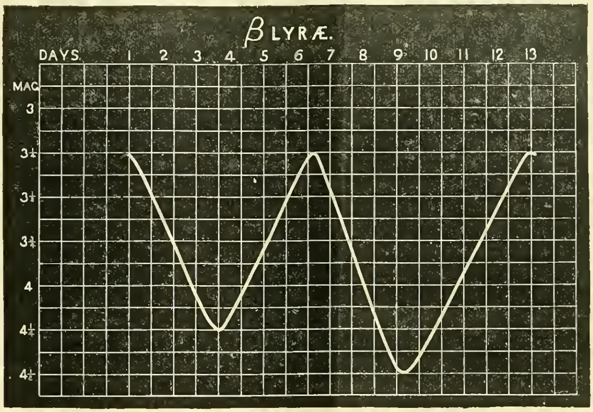


Fig. 27.—Light curve of β Lyræ.

But when we come to another class, in which we get a large light change in one period, there is one star, the history of which is so extraordinary that it is quite worth while to throw its light curve on the screen. It is called Mira, or the Marvellous. It is in the constellation of the Whale, and what happens to it in just a little less than a year is this. First it is of the second magnitude, and then in about eighty days it descends to the tenth magnitude, and then, so far as the observations have gone, it is invisible. In about another hundred days it again becomes visible as a star of the tenth magnitude. It then increases its light to the second magnitude, and begins the story over again. But sometimes at the maximum its brilliancy is not quite constant. That is to say, sometimes it goes nearer the first magnitude than the second. What happens to the light of the star below the tenth magnitude it is impossible to say. Whether it follows more nearly either of the dotted curves in the diagram is not known. Below the tenth magnitude no observations have been made, because it is very difficult to observe a star under those conditions. What one knows is that it remains invisible for about 140 days or something like that, and then it begins its cycle over again.

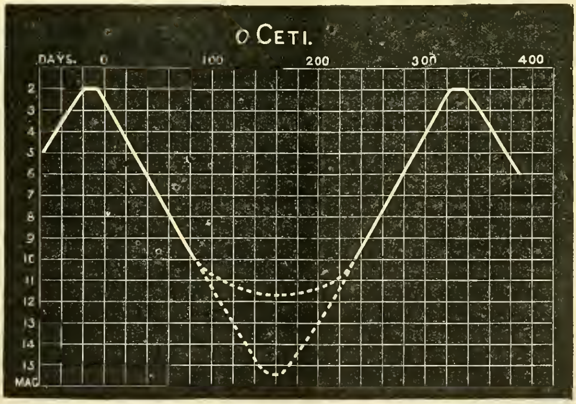


Fig. 28.—Light curve of Mira.

The next diagram illustrates the last conditions of variability,

the class of stars in which, if you remember, I told you that the variability probably did not depend upon the star itself, but upon its surroundings; and this is the famous star Algol, which is always visible in our latitudes. The history of the light changes of Algol is this. If we take the beginning of a cycle it is a star of the second magnitude. Suddenly in three hours it goes down to the fourth, and then it comes up in another three hours to the second, and goes on again for very nearly three days; and then it goes down again, comes up again, and goes on again for another three days, and so on. The diagram shows the exact

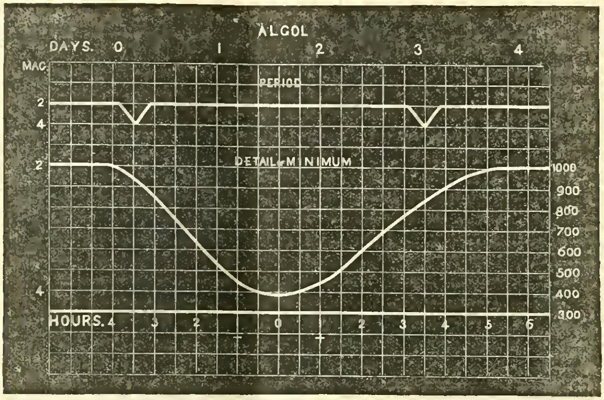


FIG. 29.—Light curve of Algol.

hape of the light curve as it has been determined by Prof. Pickering, dividing the light into a thousand parts.

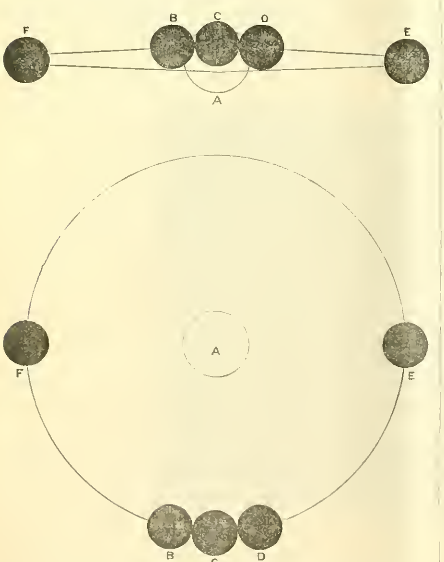


FIG. 30.—Plan and section of the orbit of the Companion of Algol.

There is another star very like this—a star which is in 81° N. declination, No. 25 in a well-known Catalogue. The difference between Algol and this is that the rise and fall are a little more rapid. Its light is feeble for about the same time as the other one, but at the bottom the curve is flat, by which I mean that, instead of going suddenly down and coming suddenly up again, it stops at its least luminosity for some little time.

Prof. Pickering has shown, I think, beyond all reasonable doubt that what is happening is this. If we take this diagram to represent in plan a large star giving out light, and B, C, D, E, F represent also in plan different positions of a dark body revolving round that central star; and then if you take the thing in section, so that the star and its satellite are represented as they really are in the plane which joins the earth and the star, you will see that in one part of the revolution of the dark body it eclipses the light body. Now, a further investigation of those conditions in the case of the second star has shown that there must be a total eclipse, and therefore Prof. Pickering draws the conclusion that in the former case the light of the body which revolves round the central one may be considered as *nil*—that is to say, that it is a dark body; but that in the case of the star D. 25—81° N.—there must be luminosity from the star which eclipses the other. And a very beautiful justification of that has recently been noted, because, although there is no change in the spectrum of Algol, there is a considerable change in the spectrum of that star the bottom curve of which is flat, showing that probably the companion has a large coronal atmosphere, and that the light of the central star has to pass through it. The light of the composite star practically changes from green to red very much as our sunlight would change if it had to pass through the atmosphere of another sun like itself coming between us.

I have prepared two or three other notes with regard to those special matters touching the stars which depend upon their distances, to show you that our sun, after all, is a small star—that there are several suns in the universe near enough to us to have had their distances already determined, which are considerably more brilliant and more imposing in every way than the star which is near us. But the clock tells me that I must leave all that to some other time, and I now end the course by thanking you very much for the indulgence that you have shown me in listening to what I have been able to tell you with regard to the constitution of our central body, and to the application of the knowledge which we have got in that way to an endeavour to cull some of the secrets of the physical construction of those suns which are very much farther removed from our ken.

J. NORMAN LOCKYER

SOCIETIES AND ACADEMIES

London

Physical Society, June 26.—Prof. W. E. Ayrton, F.R.S., Vice-President, in the chair.—Mr. E. M. Langley was elected a Member of the Society.—The following communications were read:—On certain sources of error in connection with experiments on torsional vibrations, by Mr. Herbert Tomlinson. During a long series of researches on the torsional elasticity and internal friction of metals, the author has come across the following sources of error in connection with torsional vibrations. In some of the earlier experiments a horizontal brass bar was suspended by a wire and oscillated, the times of oscillation being observed by the ordinary lamp, mirror, and scale. The moment of inertia was varied by sliding two brass cylinders, suspended from the bar by fine wires, backwards and forwards along it. It was then found that under certain conditions the bar executed a few vibrations of rapidly decreasing amplitude, came to rest, and then commenced to swing again, the amplitude increasing to a maximum, again decreasing, and so on. This effect was finally traced to an approach to synchronism between the time of oscillation of the bar and that of the small cylinders about their axes of suspension, the absorption of energy being due to these being set in vibration. The effect entirely disappeared upon clamping the cylinders rapidly to the bar. On another occasion, however, the old phenomenon reappeared, and after much time spent in investigating it, was found to be due to a somewhat similar cause, a near approach to synchronism between the periods of torsional and pendulous vibrations. If the axis of the wire passed accurately through the centre of mass of the vibrator, this would not occur; and this condition it is practically impossible to fulfil. Another source of error lies in the fact that, in a wire recently suspended, the torsional vibration-period will always be found to be slightly greater than when it has been suspended for some time and frequently oscillated.—On a mode of driving electric tuning-forks, by Prof. S. P. Thompson. It is invariably found that the frequency of an electrically maintained fork is continually changing. This great inconvenience the author believes to be due to the fact that the impulses are given to the prongs at a disadvantageous moment, namely, when they are at the extremities of their swings. It is desirable that the impulse should be given at the middle of the swing, and to effect this it is suggested that each fork should make and break the circuit of the magnet influencing the other one, and it was shown how the electrical connections could be made to effect this in a simple manner.—Prof. Silvanus P. Thompson then read some further notes on the formulæ of the electro-magnet and of the dynamo. The author pointed out that a misapprehension of his former paper on this subject had given rise to certain critical remarks by Dr. O. Fröhlich, to which he replied. The author also explained the new form given recently by Dr. Fröhlich to the formula of the electro-magnet, rendering it much more readily applicable to the various equations of dynamo-machines. Formerly the Lamont-Fröhlich formula had been written—

$$m = M \frac{kx}{1 + kx},$$

where M and k are constants, and x the magnetising force. Dr. Frohlich now suggested a formula of the form—

$$m = V \frac{x}{x + x'},$$

where V is the maximum value of m, and where x is either the current or the potential applied to the electro-magnet, and x' the diacritical value of the same; the "diacritical" value, as defined by the author in 1884, being that value which produced the state of half-saturation of the magnetic circuit. The author, folowing the lines laid down by Fröhlich in the use of this equation, showed that the general equation of the self-exciting dynamo is necessarily of the form

$$\psi = \Psi - \psi',$$

where ψ is either current or potential, ψ' the "diacritical" value of the same, and Ψ the "maximal" value of the same; that is to say, is the value which ψ would have if the given machine were run at the given speed and with the given internal and external resistances, but having its magnets independently excited to absolute saturation. Further deductions concerning the "dead turns" of the dynamo, their independence of speed, and dependence upon the resistances of the circuit and upon the construction of the machine were shown.

Sydney

Linnean Society of New South Wales, April 28.—Prof. W. J. Stephens, F.G.S., President, in the chair.—The following papers were read:—On some Lepidoptera from the Fly River, New Guinea, by E. Meyrick. Mr. Meyrick's paper contains an account of the Lepidoptera (Heterocera), collected by the recent New Guinea Expedition. Specimens of twenty-five species were met with, of which fifteen appear to be new, and are described by Mr. Meyrick. Nearly all of these may be said to be of normal Indo-Malayan types. A few specimens, from their bad condition, were unidentifiable or unfit for description.—Catalogue of the described Coleoptera of Australia, part 4, by George Masters. This part contains the names of, and references to, all the known species of the families—*Trixagidæ*, *Eucnemidæ*, *Elateridæ*, *Cebrionidæ*, *Rhipidoceridæ*, *Dascillidæ*, *Malacodermidæ*, *Cleridæ*, *Lymexylonidæ*, *Cupesidæ*, *Ptinidæ*, *Cioidæ*, *Bostrychidæ*, *Tenebrionidæ*, *Cistelidæ*, *Pythidæ*, *Monommatidæ*, *Melandryidæ*, *Lagriidæ*, *Pedilidæ*, *Anthicidæ*, *Pyrochroidæ*, *Mordellidæ*, *Rhipidophoridæ*, *Cantharidæ*, and *Œdemeridæ*, numbering 1494 species.—Miscellanea Entomologica, by William Macleay, F.L.S. This is the first of a series of papers descriptive of some of the new or rare Coleoptera in the Macleay Museum. The intention of the author is to accompany these descriptions with a general review of the genera or groups dealt with. The present paper is a revision of the genus *Diphucephala*, to which over twenty new species are added.—A revision of the Staphylinidæ of Australia, part 1, by A. Sidney Olliff, Assistant Zoologist, Australian Museum. The object of this paper is to furnish entomologists with descriptions of all the Australian Staphylinidæ at present known, to summarise the characters of the genera, and to make known a number of new forms. This first part contains the sub-family Aleocharinæ, of which the tribes Aleocharina, Gyrophænina, and Gymnusina are all represented. Among the most remarkable of the new forms belonging to the first of these tribes is a species from New South Wales described under the name *Apphiana veris* (gen. et sp. nov.), and characterised by having the basal joints of the antennæ enormously dilated on the outer side; the second joint being twice as broad as long, the third equally broad, but shorter, the fourth, fifth, and sixth shorter and gradually decreasing in breadth. In *facies* the species resembles a *Pelioptera*.—Notes from the Australian Museum, by E. P. Ramsay, F.R.S.E., and J. Douglas Ogilby. Two species of fish are described in this paper—*Myripristis carneus*, from the Admiralty Islands, presented to the Australian Museum by Capt. Farrell, and *Sygnathus parviceps*, from the Clarence River district, presented by Mr. Temperly, Inspector of Fisheries.—The Hon. James Norton exhibited a number of fossils (Chætetes and Spirifers) from Black Head, a few miles south of Kiama. Also, specimens of a porphyritic rock from Coolangatta, Shoalhaven, with the large crystals present in some, and decomposed by weathering in others.—Mr. Whitelegge exhibited specimens of a large species of *Nitella* with the following explanatory note. "A short time ago I found in the Parramatta River a very remarkable member of the above genus. It is an erect growing plant between 3 and 4 feet in height, mostly branching near the base, and giving off some five or six whorls of simple leaves, each leaf consisting usually of three cells, sometime of only two. The stem and leaves (six in number) are usually about $\frac{1}{16}$ of an inch in diameter. The internodal cells of the stem are usually 4 or 5 inches, but sometimes much longer. I have measured some of the largest yet found, and they are from 7 inches up to $8\frac{1}{2}$ long. It is highly probable that the cells of this plant are larger than those of any hitherto recorded. There are several other features which may not have been noticed in the genus. For instance, the leaves can be readily disarticulated from the stems without any apparent injury to either. When a cell is ruptured the sound produced is not unlike that of the bursting of the air-bladders of seaweeds. The rotation exhibited in the inner nodal cells differs from that of the stem and leaves, inasmuch as the chlorophyll granules take part in the general rotation. The protoplasm in the young leaves, when viewed under the microscope with the edge of the cell in focus, appears as a series of elevations and depressions, and with the higher part of the cell in focus, these elevations appear as clear spaces surrounded by small granules. Within the layer of protoplasm there exist large numbers of spherical clusters of needle-like

crystals, which circulate along the line of demarcation between the cell-sap and the protoplasm."

PARIS

Academy of Sciences, July 12.—M. Jurien de la Gravière, President, in the chair.—On the relations that exist between the geodetic and geological sciences, by M. Faye. The author's remarks are intended to show that the distinction formerly drawn between these two sciences can no longer be maintained. Thus in geodesy, for example, the sum of the forces acting on the terrestrial globe cannot be considered apart from those incessantly modifying its relief. The recent objection regarding the Quaternary glaciers is specially dealt with, not from the geological standpoint, but from that of the attraction exercised by them on the seas.—Note on the navigation of the Suez Canal at night, by M. de Lesseps. The question of nocturnal navigation, which would practically double the capacity of the canal, has now been studied exhaustively, and successfully solved by the adoption of signal lights along the route and electric lights on board the vessels in transit.—Experiments on waves, and especially on the diminution of the mean lateral pressures of undulations in canals, by M. A. de Caligny. A series of experiments are reported made on a miniature artificial canal, with the view of testing the various actions of translation and side pressure of the waves on floating bodies.—Reflections on the critical remarks of M. Hugoniot, which appeared in the *Comptes rendus* of June 28, by M. Hirn. The reference is to the author's last experiments on the flow of gases, some of whose conclusions are here sustained against M. Hirn's objections.—Identity of origin of the fluorescence $Z\beta$ by reversion, and of the bands obtained by Mr. Crookes in vacuum, by M. Lecoq de Boisbaudran. It is shown that the red band 619 of Mr. Crookes's former spectrum of yttria is due to the same earth as the author's band $Z\beta$ obtained by reversion, and that this band does not consequently characterise a new element.—Observations made during the cholera epidemic of 1885, by M. A. Guérard.—This work, by the engineer-in-chief of the Marseilles harbour works, traces the progress of the epidemic during the years 1884-85, and attributes its virulence primarily to the contaminated waters of the little River Huveaune used for domestic purposes in the districts which suffered most.—Observations of the new planet 259 and of the comet Brooks III., made at the Observatory of Nice (Gautier equatorial), by M. Charlois.—Solar observations during the first six months of the year 1886, by M. Tacchini. These observations show a progressive diminution of the phenomenon of solar spots, as well as of the solar protuberances.—On the Peruvian metrical standard, by M. Foerster. Admitting the authenticity of this standard, the author asks that accurate determinations be made of the value in metres of its two lengths, in order that all geodetic measurements, old and recent, be reduced to the same unity, that is, the international metre. In some subsequent remarks the same course was urged by M. Wolf.—Note on M. G. A. Hirn's experiments on the discharge of gases through orifices, by M. Parenty.—A new method of constructing the screw, by M. Trouvé. During the course of protracted experiments on the application of electricity to the propulsion of ships, the author has been led to study the various forms of screw now in use, and to devise another, here described, of far more simple structure.—On a physiological condition influencing photometric measurements, by M. Aug. Charpentier.—On the heat of formation of selenhydric acid, by M. Ch. Fabre. The three methods here described for measuring this heat of formation yield a mean of -9.44 cal. for gaseous selenhydric acid.—On a new species of asparagine, by M. A. Piutti. This new substance, recently discovered by the author while assisting at the preparation of asparagine in M. G. Parenti's laboratory, at Sienna, has a rotatory power, as determined by Laurent's great polarimeter, equal to, and with contrary sign to that of ordinary asparagine. The paper elicited some remarks by M. Pasteur on the great difference in taste of the two varieties of asparagine.—Distribution of a base between two acids; special case of the alkaline chromates, by M. P. Sabatier.—On the titanates of crystallised baryta and strontian, by M. L. Bourgeois. This paper is devoted to a study of the crystallised earthy alkaline titanates, which are obtained by the application of the known method—fusion of the elements of the salt in the corresponding chloride.—Action of chlorine on the seleniocyanate of potassium, by M. A. Verneuil. From the experiments here described, it appears that the action of chlorine on the alkaline seleniocyanates differs greatly from that which it exercises on the corresponding sulphocyanates. Bromine and iodine give rise to analogous phenomena.—Transformation of glucose to dextrine, by MM. E. Grimaux and L. Lefèvre. The transformation here effected for the first time is shown to throw some light on the somewhat obscure history of the dextrines.—On the transformation of the amides to amines, by M. H. Baubigny.—Isomery of the camphols and camphors; camphol of valerian, by M. Alb. Haller. A comparison of the properties of this camphol and its derivatives with those of the camphol of N'gai and its corresponding derivatives shows complete identity between these two products. In a further communication the author hopes to show that these two camphols themselves are also identical with that derived from the spirit of madder.—Electrolysis of an ammoniacal solution with the electrodes of carbon, by M. A. Millot.—On an alcoholate of crystallised potassa, by M. Engel. The body here determined, and named "alcoholate of potassa," has the formula—

$$KOH + 2(C_2H_6O).$$

—On propionic acid, by M. Ad. Renard.—Researches on the development of beetroot (continued); general conclusion; by M. Aimé Girard.—The law of connections applied to the morphology of the organs of the Mollusks, and especially of Ampullaria, by M. E. L. Bouvier.—On the presence of Ricins (Mallophages) in the quills of birds' feathers, by M. Trouessart.—On the absorption of carbonic acid by leaves, by MM. Dehérain and Maquenne. From the experiments here described it is shown (1) that the proportion of pure carbonic acid absorbed under atmospheric pressure varies with the quantity of water contained in the leaves; (2) that the coefficient of absorption of this acid by the water contained in the leaves is in the normal temperature superior to the coefficient of solubility of the same gas in water; (3) that the absorption is extremely rapid, which explains how the foliage is able to extract the extremely minute quantities of carbonic acid (some ten-thousandths) contained in the normal atmosphere.—On the crystallographic association of the triclinic felspars, by M. R. Bréon.—On the "ophite" eruptive rocks of Corbières, by M. Viguier.—Note on the primitive and Cambrian micaceous schists of Southern Andalusia, by MM. Ch. Barrois and Alb. Offret.—On injections of toxic gaseous medicines through the rectum; successful treatment of pulmonary affections by this means, by M. L. Bergeon.

CONTENTS

THURSDAY, JULY 29, 1886

ELECTRIC TRANSMISSION OF ENERGY

Electric Transmission of Energy. By Gisbert Kapp, C.E. (London: Whittaker and Co., 1886.)

SINCE the invention of the electric telegraph the subject of the electric transmission of energy is that subject which of all others has most attracted the attention of practical scientific men. Under this head are comprehended every form of telegraph and of telephone, electric railways, and the electric transmission of power for the driving of lathes and other machines. Even the novel apparatus which has been described for enabling us to see what is happening at distant places and the very transmission of light itself through the interstellar ether must be regarded as parts of the great subject which Mr. Kapp has undertaken to treat of in this small volume. On examining the book, however, it will be found that the author has wisely confined his attention to the electric transmission of energy for the purpose of its being transformed at a distant place into mechanical energy for driving machinery. Indeed, it may be said that much more than half the book is devoted to the subject of the dynamo-machine, and that much less than half of it is devoted to the subject of the electric transmission of energy. Before electric energy can be transmitted it is necessary to produce it. It is rather difficult to imagine a store of electric energy existing anywhere and ready for transmission; and hence its production, transmission, and transformation into some other form of energy are circumstances which are exactly coincident with one another: as its transmission therefore implicitly involves its production and transformation, Mr. Kapp is perfectly justified in devoting as much of his book as he pleases to a description of the dynamo-machine.

Few people are better qualified to speak from experience of the most recent practice in the manufacture of dynamo-machines than the author of this book, and his paper entitled "Modern Continuous-Current Dynamo-Electric Machines and their Engines," read on November 24, 1885, before the Institution of Civil Engineers, and the discussion upon it, are to be regarded as exceedingly valuable helps to the electrical engineer. This book will be valuable to students who do not possess a copy of Mr. Kapp's original paper. It contains additional matter, much of it good; but to some of it we would offer a mild objection. For example, some distinction might have been made between the magnetic theories of Weber and Prof. Hughes. Mr. Kapp has certainly a good working knowledge of the theory of the dynamo-machine, and he leads up to the theory in a very ingenious way, but we are afraid that students will benefit more by reading an elementary treatise on electricity and magnetism, the writer of which may have had less originality than Mr. Kapp, than by taking their elementary notions from this book. Thus, for example, the following statement may have a perfectly orthodox meaning to Mr. Kapp:—"We can either assume that the lines are of different strength, and that the mechanical force with which a given free magnet pole is urged along any one particular line, is dependent on the strength of that line, which may be different from that of any other line belonging to the same field" (p. 18); but it will give great trouble to a student who knows that the resultant force is not the same at all points in a line of force, and who will find it inconsistent with the statements which Mr. Kapp has himself to make later on.

Again, we are disposed to think that it would be graceful in Mr. Kapp, and other makers of dynamo-machines at the present time, to give a little more credit to Gramme, and to refrain from dwelling so much on the great advances which have been made in recent years in the construction of dynamo-machines. When we compare modern machines with the Gramme machine of ten years ago, we see improvements on the original machine certainly, but they are very small. They consist mainly in ways of winding the conductor on the armature, so that it shall not readily slip or heat. How little of an essential kind has been introduced in the field-magnet arrangement may be gathered from the sheet of diagrams given at p. 102 of this book. In fact, a modern dynamo-machine may be said to be a Gramme or Siemens machine, the field-magnet circuit of which has been modified in a fanciful manner. Happily such modification in shape, however fanciful, does not seem to have impaired very much the efficiency of the arrangement, whereas it has enabled makers to greatly alter the outside appearances of machines, so that good Gramme and Siemens dynamo-machines are no longer called by these names, but by the names of the makers who have given them such various outside appearances. Large modern machines are superior to ancient small machines in efficiency and in the "output" per pound weight: firstly, because they are larger—and this is the main cause of their superiority; secondly, because the mechanical engineers to whom the details in construction have always been intrusted are now, some of them, slightly acquainted with the laws of electricity and magnetism; and, thirdly, because the manufacture of numerous machines has enabled costly manufacturing tools to be introduced, and these tools enable a method of construction to be employed which would in the past have been prohibited by the expense.

Again, we object somewhat to Mr. Kapp's use of the terms "theoretical" and "practical." For example, in discussing the efficiency of the electromotor when doing various amounts of work, at p. 129, he says that a certain statement which he has made is theoretically quite accurate, but from a practical point of view it requires some modification, and he proceeds to show that the want of accuracy was due to the fact that all considerations of magnetic and material friction had been neglected. We should have said in such a case that the statement was theoretically quite inaccurate. We consider that much mischief is occasionally done by what is usually called the comparison of theory and practice. If the mathematical results derived from some hypothesis which is evidently wrong be called a theory, we must of course have disagreement between theory and practice, and it is greatly in consequence of this that the majority of practical engineers have acquired a contempt for theory and for the reading of books which deal with the theoretical principles underlying their professional work. If the results of speculation on absurd hypotheses must be

compared with facts, the terms to be used are "hypothetical" and "practical."

This is one of three advertised books of "The Specialists' Series" which deal with electric engineering. Another of the three is devoted to the subject of magneto- and dynamo-electric machines, and the third is on electric lighting. We think it probable that in the greater part of Mr. Kapp's book he is going over ground which belongs almost altogether to the author of one of the other books of the series. As Mr. Kapp treats his subjects well, however, we cannot much object to this; but what we do object to is, that while taking up the subjects of the other authors, he has not given us his own subject. In sixty-three pages, or about one-fifth of the book, an instructive account is given of the various attempts which have been made to drive carriages on railways, telpher lines, ploughs, cranes, fans, and pumps, and we understand from Mr. Kapp's introduction that it is to this sort of transmission of energy that his book is devoted.

Now it is not merely sufficient for the author to give an account of what has already been done in this way; the reader expects a correct theoretical treatment of the whole subject, the cost of conductors, the fall of potential along the conductor, and the efficiency of transmission. These questions are sufficiently well taken up for a treatise on electric lighting, but for a book on the electric driving of machines at a distance the subject can hardly be said to be touched upon. Thus, for example, the development of Sir William Thomson's law as applied in electric light installations, and published by Prof. Forbes in his lectures at the Society of Arts, is carefully given. Now small alteration of potential difference at an incandescent lamp may produce disastrous effects on the lamp, may destroy it, or may cause sudden darkness, and this is the most important consideration in arranging conductors for lighting purposes; whereas, in the electric driving of trains or machinery, small alterations of potential difference are of no importance whatsoever. In consequence of this, in driving machinery electrically there may be a very considerable fall of potential along the conductor from the dynamo to the motor, and hence motors may be worked directly at distances which it would be absurd to contemplate in working an incandescent lamp. In fact the question of cost of conductors must be treated from quite a different point of view in the two cases, and it seems to us that Mr. Kapp has taken up the point of view which is most remote from his subject.

We think Mr. Kapp's book a very valuable addition to electrical engineering literature. It will be widely read, and it deserves the popularity which it will receive. Had we not thought it to be so excellent in many ways we should not have criticised it so narrowly, and, in spite of our warning to the student, we are very glad to meet with originality in leading up to the theory of the dynamo-machine. We are glad to see that the author has slightly amplified his account of the method, now in general use, of calculating the probable electromotive force of a dynamo-machine, which he published in his paper. The method is known to be practically correct, although it is based on a magnetic hypothesis of which there is no recognition in any book on physics—the hypothesis of magnetic resistance. We could have wished that Mr. Kapp had dwelt more upon this hypothesis, as we know of no actual results of experiment having yet been published which give it a general verification.

In reading over this criticism we feel that our objections to the book have all been brought very prominently forward. It would be very easy to point out here much that is good in the book, but perhaps our readers would then find this article long and tedious. Any reader of the book will find original and interesting views in every chapter; it is not every reader who would for himself have noticed the faults which we have here gathered together. We have achieved the difficult task of finding fault with an excellent book.

JOHN PERRY

OUR BOOK SHELF

The Aryan Maori. By Edward Tregear. (Wellington, N.Z.: George Didsbury, 1885.)

THIS little book contains a theory that the ancestors of the New Zealanders belonged to the Aryan race, and were a pastoral people. To signify this, the cover is adorned with a golden picture, seemingly representing a Maori warrior in native guise, accompanied by a sturdy little Highland bull. Now, it being notorious that the New Zealanders, when discovered, had no cattle nor remains of them in their country, the reader's curiosity is aroused to see how Mr. Edward Tregear supports this unlikely thesis. His method proves to be a philological paradox which we have never met with before. For example, it is argued (p. 31) that the Maoris once knew the bull by a word like the latin *taurus*, a bull. How so, one asks, when they no more had the word in their vocabulary than the beast on their land? The answer is, that in the absence of the word *taurus* itself the author relies on a dozen or so of other Maori words which he alleges to refer to it. The following are a few of them:—*Tara*, had courage; *tararau*, made a loud noise; *tararua*, had two points or peaks; *tareha*, was red; *tarehu*, caught one unawares; *tarore*, had a noose put on him; *taruke*, lay dead in numbers (if it was characteristic of the bulls to lie dead in numbers, how multitudinous the cows and calves must have been in the Aryan-Maori herds!). The poverty of the Maori language in consonants makes it easy to the author to play this fanciful game with his dictionary to his own full satisfaction. He takes a real interest in studying the Maoris, and though he has gone astray this time, he may, if a young man, do something more worth doing in the collection of native customs, legends, games, and the like which the older natives still remember.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Tidal Friction and the Evolution of a Satellite

IN NATURE, vol. xxxiii. p. 367, is an article by Mr. G. H. Darwin, defending his theory of tidal evolution, and dealing with what I have written respecting that theory. Space will here prevent my replying at length to the above; but as the author of it seems to think that my inquiry has been confined too much to the mode of origin of the moon, I have pushed it out in other directions, when important results have been obtained. I purpose here chiefly devoting my space to these, which can be put in a comparatively short and simple form.

But before entering on the new ground I think a few words of explanation will be necessary. Mr. Darwin takes exception to a proposition of mine in that it holds his theory to be dependent upon the genesis of the moon at the present surface of the earth. I was led to this conclusion chiefly from the apparent stress laid on the condition by the writer on that subject in Thomson and Tait's "Natural Philosophy," but as the author now states that his theory is not so founded, I think it ought to be allowed that it is not. But I think the argument can be put in another way; for if the moon be allowed to have separated at a period over four hours, the theory would be at variance with the calculations of its author (for he fixes the period at between two and four hours). A flock of meteorites is proposed as a form in which the moon might have receded from the earth. Nothing can be gained by this, for the flock of meteorites cannot come nearly so close to the earth as the moon in a single mass, without the constituent members being separated and each compelled to describe an independent orbit with its own period. In other words, the tidal force would separate the flock of meteorites at a greater distance than it would the single body. And at a greater distance it is not necessary, as the moon in her conglomerate form could not be objected to.

Also, he quotes and questions a passage, which is to the effect that two heavenly bodies cannot revolve about their centre of inertia with their surfaces nearly in contact, unless one is smaller than, and denser than, the other by a certain amount. The case was intended for where the two bodies move as parts of a rigid body, *i.e.* each keeps the same face towards the other; but I omitted to insert this condition in giving the rule.

Coming now to the results of my second investigation. In his last reply, as well as in several other places, Mr. Darwin advances the Martian system as affording a striking confirmation of the influence of tidal friction. The view is that the extreme minuteness of the inner satellite has preserved it as a standing memorial of the primitive time of rotation of Mars round his axis (see the *Observatory*, July, 1879).

Now I think it must have been taken for granted that the smallness of the satellite would allow the above state of things to come about, for an estimate of the comparative effects produced on Mars by solar and by satellite tidal friction, and the reaction on the satellite shows that, according to the estimated dimensions of the latter body, its period must be considerably more disturbed than that of Mars. Prof. Newcomb estimates the diameter of the inner satellite at from 10 to 40 miles. If we take the lower estimate and suppose the body to be only as dense as the sun, then its mass will be 86000^3 times smaller than the sun's. But the distance of the satellite—6000 miles—is 23333 times less than the sun's distance, and this number must be cubed to get the effect on the tidal force through greater proximity. After making the proper allowances, as above, it will be found that the satellite has a tidal force fifty times less than that of the sun. If the tidal retardation or acceleration, as the case may be, varies as the square of the tidal force, as Mr. Darwin allows, then 1/2500 of the planet's retardation must be counteracted by the satellite tides, which go round in the reverse direction to the solar tide. Here the reaction on the satellite must be considerable, for an approximate calculation will show that the orbital momentum of that body is only about 1/2,800,000 of the planet's rotational momentum. It will not be necessary to work out the calculation. Suffice it to say that the density and dimensions of the planet are taken from Newcomb's tables, and that the distribution of density is supposed to be like that of the earth, giving a rotational momentum equal to that of homogeneous density multiplied by 0·83.

If solar tides lengthen the Martian day by one minute, then the rotational momentum will have been reduced by about 1/1400 of the whole, and the satellite must have produced an effect in the opposite direction 2500 times as small, so that the actual effect of the satellite is to increase the rotational momentum by 1/3,500,000. And since the reaction on the satellite will be equal and in the opposite direction, more than half its momentum (which is 1/2,800,000 of the planet's) will be lost, which will reduce its distance to the surface of the planet. Hence we are led to the startling conclusion that before solar tidal friction can alter the rotation-period of Mars by one minute, the inner satellite must fall into the planet. I have not taken into the calculation the circumstance that the satellite tide goes round quicker than the solar one, nor that, as the satellite approached the planet, its tides would increase, the purpose here being only to give an account of the relative changes that should take place at about the present configuration. Further, it would seem that solar tides could not have reduced the period of Mars much, even if it be supposed that the inner satellite was first at any greater distance, for then it must either have gone out and attained a longer period than Mars, or it must have fallen into the central body long ere this. There seems no escape from these conclusions, unless the little body gives out an extraordinary amount of light for its size (for its probable size is judged by its brightness), but this seems so improbable, that it would be unreasonable to suppose so. As for the density, the inferior limit must be not less than half that allowed, otherwise the tidal force of the planet would break the body up.

Now a few words may be said on the future history of the moon. Before, I have said that the tracing back of the moon has apparently been carried too far in one direction; and now I think that the tracing forward, supposing tidal friction to have free play in the future, has been carried too far in the other direction. According to Thomson and Tait ("Elements of Natural Philosophy"), the moon's distance should increase to 347,100 miles by the time the earth's rotation relatively to the moon is stopped, when the two bodies should revolve in about 48·3 days. Now it would take all the rotative momentum that the earth would lose during the change to send the moon to the above distance; or, in other words, if there were no solar tides or other causes to prevent all the rotational momentum lost by the earth going to increase the moon's orbital momentum, the moon's distance would not be increased to beyond the above. But it is clear that a considerable portion of the rotative energy would be lost in solar tidal friction, which would have no part in increasing the moon's distance from the earth. For the moon to recede to the distance named, the earth must not only have its present moment of momentum, but also as much as solar tides would extract during the interval. At the present time the retardation through solar tides is not a small fraction of the whole, and it should increase till, at the other end of the journey, it will be more than half the whole retardation, for then the solar will be greater than the lunar tide.

I believe that Messrs. Darwin and Ball, who wrote a year or two before the date of Thomson and Tait's work, give the distance to which the moon will recede as even greater than the above, and say her period of revolution will be about fifty days. Certain remarks made by Mr. Darwin in the larger work of Thomson and Tait leave some doubt as to whether a correction has recently been made in the moment of momentum of the earth's rotation, but even if the earth be supposed homogeneous, the rotational momentum would not be sufficient to send the moon to the above distance, when allowance is made for solar tidal retardation. Hence it seems that no allowance has been made for the effects of this agency, and that when such allowance is made, the moon's destination must fall far short of the estimates given. If the distance (347,100 miles) be reduced to about 320,000 miles, I think it would be nearer the mark.

Mr. Darwin says that the eccentricity of the lunar orbit, the obliquity of the ecliptic, and other elements would be coordinated together by supposing that the moon first had a separate existence at no great distance from the present surface of the earth, and with small differential motion with respect thereto. I will only say that the case is so complicated, and the data so unreliable, that the results of the calculations involved seem to be little better than guess-work.

As for the distribution of satellites in the solar system, I think the majority of diverse theories would hold that they should be more numerous far from the sun, for the simple reason that solar disturbance would be less there. JAMES NOLAN

Dergholm, Victoria, May 25

MR. NOLAN is correct in supposing that I made no numerical calculation with regard to the inner satellites of Mars. I accept the calculation which he gives, and admit that the present period of revolution of the satellite cannot be regarded, as I supposed, as a memorial of the primitive period of rotation of the planet.

I see, however, no reason, as yet, to recede from the following statement (*Phil. Trans.*, Part II. 1880, p. 883), which embodies the essence of the argument, without the erroneous phrase:—

"It is here (in the case of Mars) alone in the whole system that we find a satellite moving orbitally faster than the planet rotates. This will also be the ultimate fate of our moon, because, after the moon's orbital motion has been reduced to

identity with that of the earth's rotation, solar tidal friction will further reduce the earth's angular velocity, the tidal reaction on the moon will be reversed, and the moon's orbital velocity will increase, and her distance from the earth will diminish. But since the moon's mass is very large, the moon must recede to an enormous distance from the earth, before this reversal will take place. Now the satellites of Mars are very small, and therefore they need only to recede a short distance from the planet before the reversal of tidal friction."

No one can have any datum for saying that the Martian satellite must have fallen into the planet "long ere this," but Mr. Nolan shows that the satellite is now near the end of its history.

I do not think that Sir William Thomson made any allowance for solar tidal friction in estimating the ultimate distance of the moon. Both he and I only cared to obtain the result in round numbers.

I should be very much obliged to Mr. Nolan if he would give a reference to the proof of the theorem, that two heavenly bodies cannot revolve about their centre of inertia, as parts of a rigid body with their surfaces nearly in contact, unless one is smaller and denser than the other by a certain amount.

July 15 G. H. Darwin

Peripatus in Demerara

Considering the great antiquity and importance of *Peripatus* it seems desirable to make a public notification of the fact that I have found a species, apparently *Peripatus Edwardsi*, in the Demerara division of British Guiana. Four specimens were obtained by me, but three of them, owing to some unknown cause, became considerably damaged and practically useless. The fourth specimen, which was found by me nearly a month ago, is still alive and evidently in good health. It is, when in progression, about $3\frac{1}{2}$ inches in length, but it often elongates itself considerably more and at other times becomes nearly coiled into a thick lump. It possesses thirty-one pairs of feet, the last three of which it rarely puts to the ground except when it goes backwards for short distances. Several other pairs at intervals along the body are carried off the ground in the same manner. It seems distinctly restless under the influence of light, appearing comfortable only when it retreats into some moist and darkened corner. When handled, it frequently discharges its viscid secretion, but as frequently neglects to do it when handled for the first time after a long interval, but more especially when touched or taken up for three or four times in rapid succession. It has been kept in an old sardine tin with small pieces of decayed wood, which were taken from the stump in which it was found, and the wood is kept in a moist condition. The locality from which it was obtained was the Hoorubea Creek, about twenty miles from Georgetown, on the east coast of the Demerara River, close to the meeting-point of an extensive forest and a water savannah. The four specimens were obtained in the same locality; and, though I have sought for them continually in other places, up to the present I have been unable to find others. From the long period of time during which this specimen has survived in confinement, I think there will be no difficulty, when I have obtained a large number of specimens, in sending them alive to England to Prof. Moseley and others. Unfortunately I have no possible access here to any literature on the group. I do not think it is generally known, but Mr. Im Thurn has once previously found specimens of Peripatus in the Essequebo division of British Guiana. His specimens were, however, very small ones.

British Guiana Museum John J. Quelch

Upper Wind-Currents over the Bay of Bengal in March, and Malaysia in April and May

In my last letter to Nature, vol. xxxiii. p. 460, on the subject of upper winds, I described the circulation of the Indian Ocean from the equator, where the north-west wind changes into the north-east monsoon, as far north as Ceylon, in the month of February. From there, about the beginning of March, I took a section of the weather, as nearly straight as practicable, from Colombo, through Calcutta, and 400 miles due north to Darjeeling.

The general weather system at that season is very simple. A belt of high pressure lies across the Bay of Bengal, from about Madras, to the southern limits of Burmah. The north-east monsoon blows to the south of this, towards the low pressure below the equator; the belt, of course, covers a calm area; while to the north a south-west wind blows towards a low pressure somewhere beyond the Himalayas.

The upper currents over the north-east monsoon always blew from some more easterly point than the surface-wind; the cloudless sky over Madras prevented any observations; north of this the higher clouds always came from some point more northerly than the south-west wind below. The lofty range of the Himalayas seemed to make no difference; at Sendukphu I succeeded in getting a photograph of a cumulo-form cloud trailing from the summit of Kanching Junga (29,000 feet) well from the west-north-west, while a south-west wind was driving up mist from the plains. The existence of cumulus at so high a level has, I think, been denied by some meteorologists.

All these observations are in complete accordance with the normal circulation of the northern hemisphere; but the character of south-west monsoons deserves notice. The term south-west monsoon is unfortunately used for two different stages of the same weather sequence, and much confusion comes thereby. Maury and others think only of the direction of the wind; common parlance all over the East talks of the monsoon as of a rainy season which sets in suddenly, long after south-west winds have been blowing for weeks or months previously.

The facts of the case are these:—As early as January a light south-west wind commences in the north of the Bay of Bengal, first only as a sea breeze; later, when we encountered it, as a light continuous wind. Nothing can be more lovely than the weather then; bright blue sky, scarcely a light cloud, with a warm gentle wind; the monsoon, unlike March, begins like a lamb and goes out like a lion. As the season goes on an area of low pressure, which has been gradually forming over Northern Bengal, becomes more pronounced, and the south-west wind gradually works further and further to the southwards below Ceylon. Then, sometimes in June, a sudden total change comes over the weather, while the only alteration the isobars show is a slight motion of the lowest pressure towards the North-west Provinces of India. A sudden burst of rain and thunder breaks over Ceylon, and then the bad weather works slowly northwards. This is the commencement of the south-west monsoon in common talk. Everyone will tell you how many days it takes to work up to Bombay on one side and to Calcutta, by way of Burmah and Assam, on the other. Madras escapes for the present, only to be deluged in November by the north-east monsoon. So we get the curious sequence that the wind works downwards, the rain upwards; and also the fact that the greatest and most sudden change in the year is associated with no striking change in the distribution of pressure. The Indian meteorologists are of opinion that this sudden change in the character of the same wind is due to a sudden irruption of air, highly charged with vapour from the neighbourhood of the equatorial doldrums, but that the south-east trade is not linked with the south-west monsoon in a continuous current, except occasionally and temporarily. Would it not be of the highest interest and importance to discover whether this sudden change of weather is associated with any change in the relation of the upper and lower winds? In my letter to Nature (vol. xxxiii. p. 460) I showed that over the south-west monsoon of the Gulf of Guinea the upper currents were those of the southern hemisphere, and that the south-east trade there seemed to grow gradually into a south-west wind as it crossed the line. If in Ceylon and India the higher clouds continue to come, as we found them, from west or north-west after the burst of the south-west monsoon, there must be a doldrum between it and the south-east trade; but if the upper currents turn towards south or south-east after the burst, then undoubtedly the south-east trade has invaded the northern hemisphere. The latter is of course the old theory of the monsoon; and perhaps another test may be applied to the solution of these alternatives. If the south-east trade blows into a doldrum, there must be a belt of high pressure between Ceylon and the equator to give gradients for south-west winds. Has this ever been found? I do not think that calm alone is sufficient to be called a "doldrum." During the north-west monsoon, which is unquestionably the north-east monsoon drawn across the line, the direction of the wind changes gradually, but the velocity is often less just on the equator than on either side. I made some special inquiries on this point.

In the Philippines, China, and Japan the upper winds over the south-west monsoon follow the normal course of the northern hemisphere; but there is no burst of the monsoon in those countries.

Some meteorologists have asserted that the south-west monsoon may be considered a stationary cyclone. This might be so if we define a cyclone simply as an irregularly circular area of low pressure round and into which the wind blows spirally. But when we look at the kind of rain and varieties of cloud which give distinctive character to various parts of a cyclone, our own observations and the information we have received from others entirely discountenance this idea.

In Malaysia, between Singapore and Borneo, in the early days of April the surface-winds were all from about north-east, and the clouds at various levels always from more south of east. In North Borneo, later in the month, the south-west land breeze of the morning always went round by south-east to north-east in the afternoon and evening, while the higher clouds came always from about north-east.

In Sooloo and the Philippines during the month of May the surface winds were much complicated by land and sea breezes, but the sequence of upper currents was always that proper to the hemisphere.

So far for ordinary weather. I was not fortunate enough to meet with a typhoon, but the reports of the observatories at Manilla, Hong Kong, and Tokio are all agreed that the relation of upper and lower currents is the same in a typhoon in the China Seas as in a European cyclone.

Yokohama, June 12 RALPH ABERCROMBY

Mock Sun

I INCLOSE sketch of the first mock sun I have been fortunate enough to see at Cranbrook, Kent, on July 20, 5 to $5\frac{1}{2}$ p.m.

About 10m. before noticing this fine phenomenon we had noticed a fragment of it, not knowing what was to follow; and we were struck by the extraordinary position of the bow with reference to the sun, viz. about 45° from it, and at an unaccountable angle to the horizon. The latter picture I can only draw by memory. The upper drawing is from one made on the spot in presence of two intelligent adult witnesses, who were consulted on each point which I proceed to notice.

(1) The rainbow near the zenith was of the breadth and brilliancy of an ordinary rainbow (the same was the case with the fragment seen ten minutes earlier, which was lost when the rest came out). The fact of the arc seen near the zenith belonging to two circles, one small and one large, touching each other, was sufficiently certain to my eye, confirmed by another educated eye, but not admitted by the third less educated one. I draw it as I apprehended it. The colours were unusually vivid against a thin veil of fleecy clouds.

(2) The halo-circle round the sun, and the arched eyebrows, so to call them, were about half the breadth of the rainbow, and washy in colour. The shapes drawn are quite faithful, and were so sharp as to leave no room whatever for doubt or imagination.

(3) The interior area of the circle was darker than the ouside.

(4) The position of the mock sun was not diametrical. The sun, seen through a handkerchief whose edge was stretched through the two mocks, was about two-thirds of its own breadth below the edge, clear.

(5) The white rays (about half the breadth of the mock lights) were seldom seen both at the same time, but were quite decided outside the circle and traceable within it, but nowhere nearly so bright as the mock lights.

(6) The mock lights were short fragments of arcs of rainbows, more vividly coloured than the halo-circle outside of which they stood clear of it, but not so broad and not quite so vivid as the great rainbow arc.

These fragments were *not* tangential. Short as they were, their own axis was clearly determined by all three witnesses to be inclined towards the radial ray, and more inclined to the arc of the halo. But I have unconsciously given a curved shape to the short fragment. It was too short to show a curve. There was no pretence of a disk, as if really a mock sun. It was only a very vivid fragment of a rainbow. A third fainter one was at the top of the halo.

The sky was much covered with thin cirrus; a fine sunny evening; air peculiarly clear for distant views.

Collingwood, July 22 W. J. HERSCHEL

P.S.—Radius of halo-circle, measured as best I could, $22\frac{1}{2}° \pm 2\frac{1}{2}°$. Radius, continued to the rainbow, 45° with proportionate error. The arc of the halo-circle was generally absent next to the mock lights, but could sometimes be traced.

"The Duration of Germ-Life in Water"

IN a letter bearing this title in your last issue (p. 265) Mr. Downes refers to the recent publication by Messrs. Crookes, Odling, and Tidy, of some experiments which they have made on the vitality of the *Bacillus anthracis* in water, with regard to which I should like to call attention to the fact that this subject has during the past three years been investigated by various experimenters, including Koch, Cornil, and Babes, Nicati and Rietsch. Within the past two months no less than three papers have been published on this subject, two of them in Germany by Dr. Wolfhügel and Meade Bolton respectively, whilst the third, by myself, "On the Multiplication of Micro-organisms," was communicated to the Royal Society at the meeting in June last. In this paper I have recorded a number of experiments made both with the mixtures of organisms found in various natural waters, as well as with three well-characterised forms which are associated with disease, viz. Koch's "Comma" spirillum of Asiatic cholera, Finkler-Prior's "Comma" spirillum of European cholera, and the Bacillus pyocyaneus, which produces the greenish-blue colouring matter frequently present in abscesses. The methods of research which have been independently selected both by Wolfhügel, Meade Bolton, and myself, are identical, and consist in the examination, by gelatine plate-cultivation, of waters purposely impregnated with the organisms in question. This method is obviously the one which most recommends itself for the purpose, as it not only enables one to ascertain the presence or absence of the organisms, but also to quantitatively follow their multiplication or reduction. I may mention that these three organisms present great differences in their behaviour under similar circumstances; thus the *Bacillus pyocyaneus* is possessed of far greater vitality in water than either of the other two, its presence being demonstrable even in distilled water after fifty-three days, in numbers exceeding manyfold those originally introduced. Koch's "Comma' spirillum, on the other hand, was in the purest forms of potable water no longer demonstrable after the ninth day, whilst in London sewage it was found in largely multiplied numbers after twenty-nine days; whilst Finkler's spirillum could in no case be detected after the first day, and frequently not even on the day of inoculation. A curious phenomenon, which my experiments, as well as those of Wolfhügel and Meade Bolton have brought to light, is that when organisms of this kind, which are not the natural inhabitants of water, are introduced into this medium, a large proportion of them are frequently at first destroyed, a greater or less multiplication in their numbers often subsequently taking place.

The *Bacillus anthracis*, as is well known to bacteriologists, appears in two very distinct forms, the *bacillus*-form and the *spore*-form, and these present very great differences in their powers of endurance, the former being destroyed with comparative ease, whilst the spores are remarkable for their vitality. Mr. Crookes and his colleagues have apparently experimented with the bacillus-form of anthrax only, which they state is rapidly destroyed when introduced into London water, but Dr. Meade Bolton, who has dealt with anthrax in both its forms, has shown that the spores of anthrax retain their vitality even in distilled water for upwards of ninety days, and that it is only the bacilli which rapidly perish in some kinds of potable water. In polluted well-water Meade Bolton has also shown that even the bacilli are persistent for upwards of ninety days, and the spores for nearly a year, whilst Wolfhügel has found that in polluted river-water (the River Panke, in Berlin), even when diluted tenfold with distilled water, the anthrax bacilli undergo extensive multiplication. PERCY F. FRANKLAND

Normal School of Science,
South Kensington Museum, S.W., July 26

Animal Intelligence

IN NATURE for July 22, on p. 265, Mr. Frederick Lewis calls attention to a nest-building wasp who closed up her nest without filling it first with grubs or laying an egg. There is nothing uncommon in this neglect on the part of the wasp, as any one who has at all studied their habits in the tropics will know, such perfectly empty nests being frequently met with. I have often thought the empty nest might have something to do with the fact that the wasp may not have been prepared to deposit her egg; but then, if that were the case, we should occasionally find nests with the remains of the caterpillars or

spiders collected. When a wasp has once chosen a site for building, it is very difficult to drive her away.

63, St. George Street, Leeds HY. LING ROTH

The Microscope as a Refractor

I AM rather surprised, after the judicious remarks of Dr. Gladstone on this subject in NATURE of July 1 (p. 192), to find Mr. Gordon Thompson still maintaining his opinion to have introduced anything not yet known or tried with the microscope adapted to this purpose. If he had had time to go over the papers of Royston Pigott (*Proceedings* of the Royal Society, 1876), of Mr. Sorby (*Mineralogical Magazine*, 1878), and of myself (*Proceedings* of the Royal Society, 1884), he could have convinced himself that all what he proposes has been already elaborated and applied. He could also have learnt why the method with the microscope is limited in its exactitude to the third decimal, as the mathematical expression which it involves is deduced from not very strict principles, this being as well the case with the formula for the hollow prism.

The Hague, July 21 L. BLEEKRODE

HERRMANN ABICH

AS briefly reported in NATURE last week this venerable geologist died at Vienna on July 1. As far back as the year 1831 he began his scientific career by the publication of an important memoir, in which by novel methods of chemical analysis he determined the composition of various minerals of the Spinel family, and showed how alike by chemical composition and crystalline form they could all be ranged in one group. This early paper gave evidence of the carefulness of observation which distinguished him through life. It was followed by other chemical and mineralogical essays, especially in the department of volcanic products. Gradually he was led to devote special attention to the phenomena of volcanic action, and in the course of his investigations to visit most of the volcanic districts of Europe. His folio atlas of views illustrative of Vesuvius and Etna (1837), and his "Vulkanische Bildungen" (1841), are among the best known of his writings. He had great facility as a sketcher, and some of his drawings of volcanic craters have done duty for nearly half a century in text-books in many languages. The east of Europe presented a wide and almost unknown field for his exploration. As far back as 1840 he published notices of his wanderings in the Caucasus. He ascended to the summit of Mount Ararat, and devoted most of the remainder of his life to the investigation of the vast region of the Caucasus and south-eastern Europe. Many papers published from time to time in the scientific journals record his unwearied industry. But perhaps the most striking and durable monument of his scientific achievements is his great work, "Geologische Forschungen in den Kaukasischen Landern," the publication of which he was superintending at the time of his death. This magnificent monograph, of which only the first part has been published, brings before the reader in a series of maps, sketches, large panoramic views, and detailed descriptions a picture of the external aspect and geological structure of the Caucasian region and impresses him with a profound admiration for the author's geological prowess. Abich had during the last few years settled in Vienna, availing himself of the typographic facilities to be found in the Austrian capital. He has been a notable instance of the longevity attained by many active field-geologists, for he almost reached the age of three score and ten years, retaining to the end his enthusiasm and industry. It is to be hoped that the second part of his monumental work, which is to treat of the eastern half of the Armenian Highlands, has been left in such a state as to admit of publication.

CAPILLARY ATTRACTION[1]

II.

NOW in this second way we have, in performing the folding motion, allowed the water surface to become less by 60 square centimetres. It is easily seen that, provided the radius of curvature in every part of the surface exceeds one or two hundred times the extent of distance to which the molecular attraction is sensible, or, as we may say practically, provided the radius of curvature is everwhere greater than 5000 micro-millimetres (that is, the two-hundredth of a millimetre), we should have obtained this amount of work with the same diminution of water-surface, however performed. Hence our result is that we have found 4·5/60 (or 3/40) of a centimetre-gramme of work per square centimetre of diminution of surface. This is precisely the result we should have had if the water had been absolutely deprived of the attractive force between water and water, and its whole surface had been coated over with an infinitely thin contractile film possessing a uniform contractile force of 3/40 of a gramme weight, or 75 milligrammes, per lineal centimetre.

It is now convenient to keep to our ideal film, and give

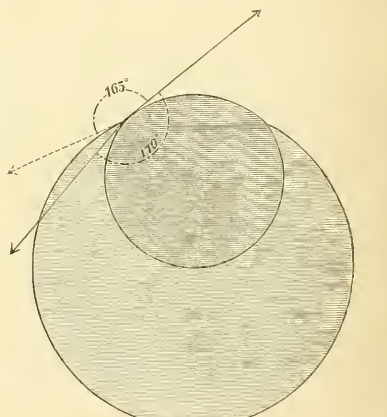


FIG. 2.

up thinking of what, according to our present capacity for imagining molecular action, is the more real thing—namely, the mutual attraction between the different portions of the liquid. But do not, I entreat you, fall into the paradoxical habit of thinking of the surface film as other than an ideal way of stating the resultant effect of mutual attraction between the different portions of the fluid. Look, now, at one of the pieces of water ideally rigidified, or, if you please, at the two pieces put together to make one. Remember we are at the centre of the earth. What will take place if this piece of matter resting in the air before you suddenly ceases to be rigid? Imagine it, as I have said, to be enclosed in a film everywhere tending to contract with a force equal to 3/40 of a gramme or 75 milligrammes weight per lineal centimetre. This contractile film will clearly press most where the convexity is greatest. A very elementary piece of mathematics tells us that on the rigid convex surface which you see, the amount of its pressure per square centimetre will be found by multiplying the sum[2] of the curvatures in two mutually-perpendicular normal sections

[1] Continued from p. 272.

[2] This sum for brevity I henceforth call simply "the curvature of the surface" at any point.

by the amount of the force per lineal centimetre. In any place where the surface is concave the effect of the surface tension is to suck outwards—that is to say, in mathematical language, to exert negative pressure inwards. Now, suppose in an instant the rigidity to be annulled, and the piece of glass which you see, still undisturbed by gravity, to become water. The instantaneous effect of these unequal pressures over its surface will be to set it in motion. If it were a perfect fluid it would go on vibrating for ever with wildly-irregular vibrations, starting from so rude an initial shape as this which I hold in my hand. Water, as any other liquid, is in reality viscous, and therefore the vibrations will gradually subside, and the piece of matter will come to rest in a spherical figure, slightly warmed as the result of the work done by the forces of mutual attraction by which it was set in motion from the initial shape. The work done by these forces during the change of the body from any one shape to any other is in simple proportion to the diminution of the whole surface area; and the configuration of equilibrium, when there is no disturbance from gravity, or from any other solid or liquid body, is the figure in which the surface area is the smallest possible that can enclose the given bulk of matter.

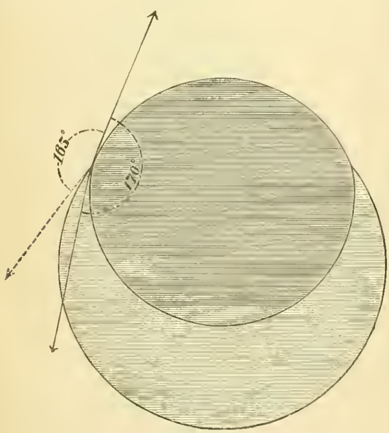


Fig. 3.

I have calculated the period of vibration of a sphere of water[1] (a dew-drop!) and find it to be $\frac{1}{4}a^{\frac{3}{2}}$, where a is the radius measured in centimetres; thus—

For a radius of	$\frac{1}{4}$	cm. the period is	$\frac{1}{32}$	second
,,	1	,,	,,	$\frac{1}{4}$	,,
,,	2·54	,,	,,	1	,,
,,	4	,,	,,	2	,,
,,	16	,,	,,	16	,,
,,	36	,,	,,	36	,,
,,	1407	,,	,,	13,200	,,

The dynamics of the subject, so far as a single liquid is concerned, is absolutely comprised in the mathematics without symbols which I have put before you. Twenty pages covered with sextuple integrals could tell us no more.

Hitherto we have only considered mutual attraction between the parts of two portions of one and the same liquid—water for instance. Consider, now, two different kinds of liquid: for instance, water and carbon disulphide (which, for brevity, I shall call sulphide). Deal with them exactly as we dealt with the two pieces of water. I need not go through the whole process again; the result is obvious. Thirty times the excess of the sum of the surface-tensions of the two liquids separately, above the tension of the interface between them, is equal to the work done in letting the two bodies come together directly over the

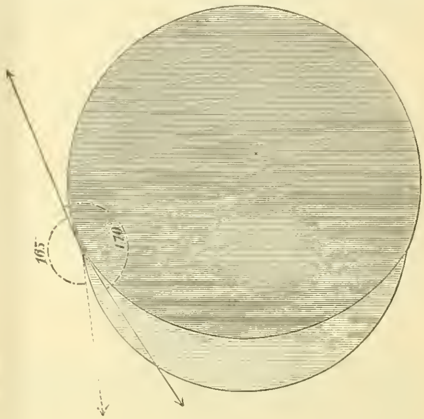


Fig. 4.

supposed area of thirty square centimetres. *Hence the interfacial tension per unit area of the interface is equal to the excess of the sum of the surface-tensions of the two liquids separately, above the work done in letting the two bodies come together directly so as to meet in a unit area of each.* In the particular case of two similar bodies

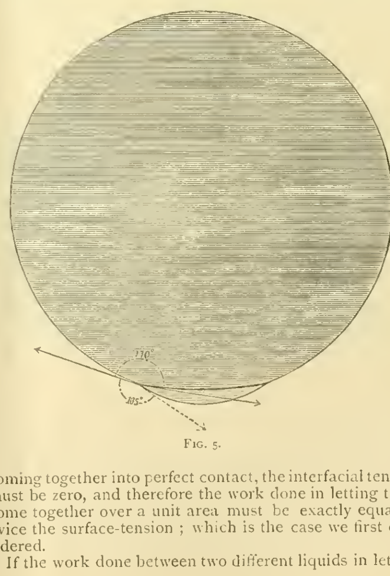


Fig. 5.

coming together into perfect contact, the interfacial tension must be zero, and therefore the work done in letting them come together over a unit area must be exactly equal to twice the surface-tension; which is the case we first considered.

If the work done between two different liquids in letting

[1] See paper by Lord Rayleigh in *Proc. Roy. Soc.*, No. 196, May 5, 1879.

them come together over a small area, exceeds the sum of the surface-tensions, the interfacial tension is negative. The result is an instantaneous puckering of the interface, as the commencement of diffusion and the well-known process of continued inter-diffusion follows.

Consider next the mutual attraction between a solid and a liquid. Choose any particular area of the solid, and let a portion of the surface of the liquid be preliminarily shaped to fit it. Let now the liquid, kept for

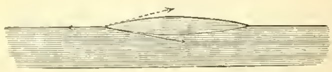


FIG. 6.

the moment rigid, be allowed to come into contact over this area with the solid. The amount by which the work done per unit area of contact falls short of the surface-tension of the liquid is equal to the interfacial tension of the liquid. If the work done per unit area is exactly equal to the free-surface tension of the liquid, the interfacial tension is zero. In this case the surface of the liquid when in equilibrium at the place of meeting of liquid and solid is at right angles to the surface of the

FIG. 7.

solid. The angle between the free surfaces of liquid and solid is acute or obtuse according as the interfacial tension is positive or negative; its cosine being equal to the interfacial tension divided by the free-surface tension. The greatest possible value the interfacial tension can have is clearly the free-surface tension, and it reaches this limiting value only in the, not purely static, case of a liquid resting on a solid of high thermal conductivity, kept at a temperature greatly above the boiling-point of

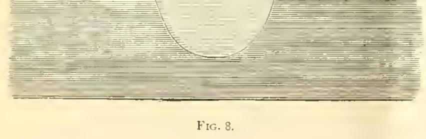


FIG. 8.

the liquid; as in the well-known phenomena to which attention has been called by Leidenfrost and Boutigny. There is no such limit to the absolute value of the interfacial tension when negative, but its absolute value must be less than that of the free surface tension to admit of equilibrium at a line of separation between liquid and solid. If minus the interfacial tension is exactly equal to the free-surface tension, the angle between the free surfaces at the line of separation is exactly 180°. If minus the interfacial tension exceeds the free-surface tension, the liquid runs all over the solid, as, for instance, water over a glass plate which has been very perfectly cleansed. If for a moment we leave the centre of the earth, and suppose ourselves anywhere else in or on the earth, we find the liquid running up, against gravity, in a thin film over the upper part of the containing vessel, and leaving the interface at an angle of 180° between the free surface of the liquid, and the surface of the film adhering to the solid above the bounding line of the free liquid surface. This is the case of water contained in a glass vessel, or in contact with a piece of glass of any shape, provided the surface of the glass be very perfectly cleansed.

When two liquids which do not mingle, that is to say, two liquids of which the interfacial tension is positive, are placed in contact and left to themselves undisturbed by gravity (in our favourite laboratory in the centre of the earth suppose), after performing vibrations subsiding in virtue of viscosity, the compound mass will come to rest, in a configuration consisting of two in-

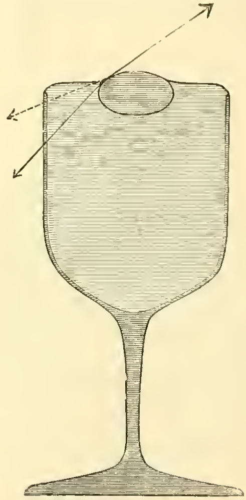


FIG. 9.

tersecting segments of spherical surfaces constituting the outer boundary of the two portions of liquid, and a third segment of spherical surface through their intersection constituting the interface between the two liquids. These three spherical surfaces meet at the same angles as three balancing forces in a plane whose magnitudes are respectively the surface tensions of the outer surfaces of the two liquids and the tension of their interface. Figs. 2 to 5 illustrate these configurations in the case of bisulphide of carbon and water for several different proportions of the volumes of the two liquids. (In the figures the dark shading represents water in each case.) When the volume of each liquid is given, and the angles of meeting of the three surfaces are known, the problem of describing the three spherical surfaces is clearly determinate. It is an interesting enough geometrical problem.

If we now for a moment leave our gravitationless laboratory, and, returning to the Theatre of the Royal Institution, bring our two masses of liquid into contact, as I now do in this glass bottle, we have the one liquid floating upon the other, and the form assumed by the floating liquid may be learned, for several different cases, from the phenomena exhibited in these bottles

and glass beakers, and shown on an enlarged scale in these two diagrams (Figs. 6 to 8); which represent bisulphide of carbon floating on the surface of sulphate of zinc, and in this case (Fig. 8) the bisulphide of carbon drop is of nearly the maximum size capable of floating. Here is the bottle whose contents are represented in Fig. 8, and we shall find that a very slight vertical disturbance serves to submerge the mass of bisulphide of carbon. There now it has sunk, and we shall find when its vibrations have ceased that the bisulphide of carbon has taken the form of a large sphere supported within the sulphate of zinc. Now, remembering that we are again at the centre of the earth, and that gravity does not hinder us, suppose the glass matter of the bottle

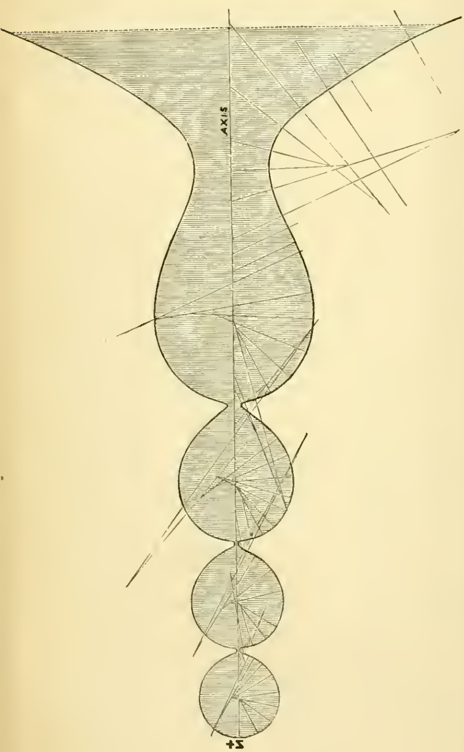


FIG. 10.

suddenly to become liquid sulphate of zinc, this mass would become a compound sphere like the one shown on this diagram (Fig. 3), and would have a radius of about 8 centimetres. If it were sulphate of zinc alone, and of this magnitude, its period of vibration would be about $5\frac{1}{2}$ seconds.

Fig. 9 shows a drop of sulphate of zinc floating on a wine-glassful of bisulphide of carbon.

In observing the phenomena of two liquids in contact, I have found it very convenient to use sulphate of zinc (which I find, by experiment, has the same free-surface tension as water) and bisulphide of carbon; as these liquids do not mix when brought together, and, for a short time at least, there is no chemical interaction between them. Also, sulphate of zinc may be made to have a density less than, or equal to, or greater than, that of the bisulphide, and the bisulphide may be coloured to a more or less deep purple tint by iodine, and this enables us easily to observe drops of any one of these liquids on the other. In the three bottles now before you the clear liquid is sulphate of zinc—in one bottle it has a density less than, in another equal to, and in the third greater than, the density of the sulphide—and you see how, by means of the coloured sulphide, all the phenomena of drops resting upon or floating within a liquid into which they do not diffuse may be observed, and, under suitable arrangements, quantitatively estimated.

When a liquid under the influence of gravity is supported by a solid, it takes a configuration in which the difference of curvature of the free surface at different levels is equal to the difference of levels divided by the surface tension reckoned in terms of weight of unit bulk of the liquid as unity; and the free surface of the liquid leaves the free surface of the solid at the angle whose

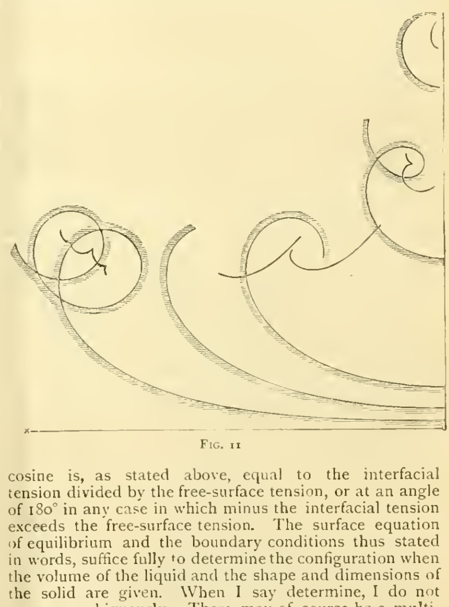


FIG. 11

cosine is, as stated above, equal to the interfacial tension divided by the free-surface tension, or at an angle of 180° in any case in which minus the interfacial tension exceeds the free-surface tension. The surface equation of equilibrium and the boundary conditions thus stated in words, suffice fully to determine the configuration when the volume of the liquid and the shape and dimensions of the solid are given. When I say determine, I do not mean unambiguously. There may of course be a multiplicity of solutions of the problem; as, for instance, when the solid presents several hollows in which, or projections hanging from which, portions of the liquid, or in or hanging from any one of which the whole liquid, may rest.

When the solid is symmetrical round a vertical axis, the figure assumed by the liquid is that of a figure of revolution, and its form is determined by the equation given above in words. A general solution of this problem by the methods of the differential and integral calculus transcends the powers of mathematical analysis, but the following simple graphical method of working out what constitutes mathematically a complete solution, occurred to me a great many years ago.

Draw a line to represent the axis of the surface of revo-

lution. This line is vertical in the realisation now to be given, and it or any line parallel to it will be called vertical in the drawing, and any line perpendicular to it will be called horizontal. The distance between any two horizontal lines in the drawing will be called *difference of levels*.

Through any point, N, of the axis draw a line, N P, cutting it at any angle. With any point, O, as centre on the line N P, describe a very small circular arc through P P′, and let N′ be the point in which the line of O P′ cuts the axis. Measure N P, N′ P′, and the difference of levels between P and P′. Denoting this last by δ, and taking a as a linear parameter, calculate the value of

$$\left(\frac{\delta}{a^2} + \frac{1}{\mathrm{OP}} + \frac{1}{\mathrm{NP}} - \frac{1}{\mathrm{N'P'}}\right)^{-1}.$$

Take this length on the compasses, and putting the pencil point at P′, place the other point at O′ on the line P′ N′, and with O′ as centre, describe a small arc, P′ P″. Continue the process according to the same rule, and the successive very small arcs so drawn will constitute a curved line, which is the generating line of the surface of revolution inclosing the liquid, according to the conditions of the special case treated.

This method of solving the capillary equation for surfaces of revolution remained unused for fifteen or twenty years, until in 1874 I placed it in the hands of Mr. John Perry (now Professor of Mechanics at the City and Guilds Institute), who was then attending the Natural Philosophy Laboratory of Glasgow University. He worked out the problem with great perseverance and ability, and the result of his labours was a series of skilfully executed drawings representing a large variety of cases of the capillary surfaces of revolution. These drawings, which are most instructive and valuable, I have not yet been able to prepare for publication, but the most characteristic of them have been reproduced on an enlarged scale, and are now on the screen before you.[1] Three of the diagrams, those to which I am now pointing (Figs. 10, 11, and 12), illustrate strictly theoretical solutions—that is to say, the curves there shown do not represent real capillary surfaces—but these mathematical extensions of the problem, while most interesting and instructive, are such as cannot be adequately treated in the time now at my disposal.

WILLIAM THOMSON

(*To be continued.*)

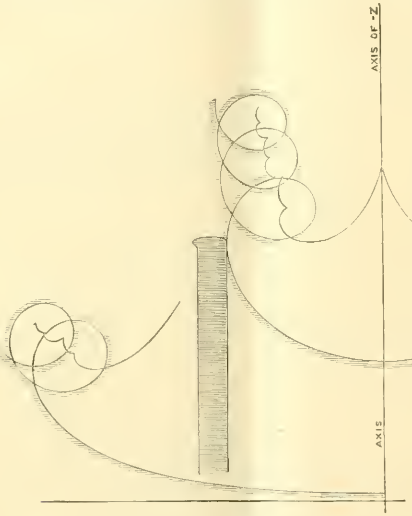


FIG. 12.

THE SCIENCE AND ART DEPARTMENT EXAMINATION IN CHEMISTRY

THE new editor of the "Science and Art Directory" announces a new departure of the most important kind in the teaching of chemistry. In addition to the oral instruction in the elementary stage, there is now introduced an alternative first stage or elementary course intended for those students who only require the elements of chemistry as a foundation for their studies in other subjects.

We give the new syllabus so that it may speak for itself, and congratulate the Department on a step in harmony with the views of the best friends of scientific education in this country.

SOLUTION.—Disappearance of a solid in a liquid by solution. Saturation of a liquid. Effect of increase of temperature on saturation. Effect of lowering the temperature on saturation. Crystallisation. Filtration. Solvent properties of water. Rain, spring, river, and pond waters, &c. Solid matter in different waters; how estimated. Loch Katrine water. Thames water. Sea water. Hard and soft waters. Mineral waters. Similar solvent of other liquids. Solution of one liquid in another. Liquids insoluble in one another. Solution of gases in water and other liquids. The effect of heat on the quantity of gas dissolved by a liquid.

Experiments.—Suspend a piece of white sugar by a thread in a glass vessel containing water. Dissolve salt in water. Show on a balance that sugar or salt and water when separate and when dissolved weigh the same. Show that salt is obtained

[1] The diagrams here referred to are now published in Figs. 10 to 24 of the present report of the lecture at the Royal Institution. These figures are accurate copies of Mr. Perry's original drawings, and I desire to acknowledge the great care and attention which Mr. Cooper, engraver to NATURE, has given to the work.

from the solution by evaporation. Saturate water with nitre, and show that the solubility is increased by increase of temperature. Demonstrate the formation of crystals. Illustrate the removal of substances in suspension, and the non-removal of substances in solution by filtration. Show by evaporation the solid matter dissolved in a sample of pump, or river, or spring water, and explain the method for its quantitative determination. Show the like solvent action with other liquids, as calcium chloride, in alcohol and sulphur in carbon disulphide. Compare the result of admixture of spirit or oil of vitriol with water with that of oil or mercury with water. Heat ordinary water and collect the expelled air.

AIR.—Surrounds the globe. Wind is air in motion Breathing. Air occupies space. The bulk of any quantity of air is much changed by temperature and by pressure. Air has weight. The necessity of air for animals and plants. Bodies when burning require air. Air a mixture of two gases. Oxygen and nitrogen. The proportion of nitrogen to oxygen. Oxygen the active body in air. Bodies burn in it alone and more brilliantly than in air. The combination of oxygen with iron and with other bodies. Increase of weight of bodies which unite with oxygen. Nitrogen does not combine directly with bodies. The nearly constant composition of pure air. Presence of other gases in small amount in air. Water in the air as a gas. The drying up of water.

Experiments.—That air occupies space may be shown by plunging a bell jar into a vessel of water. Fit a flask containing water with an india-rubber plug and delivery tube, heat the water, and collect the expelled air over water. Close the short limb of a syphon tube containing air, and compress the air in the long limb by pouring in mercury. Weigh a flask, fitted with a stop-cock, full of air, and then exhaust by an air-pump, and weigh again. Show that a lighted candle is soon extinguished when burnt under a bell jar, but that it continues to burn if fresh air be from time to time supplied. Burn phosphorus in a tall bell jar over water, and show the diminution of bulk of air. Ignite phosphorus, place it in the remaining gas. Burn some phosphorus under a dry bell jar to show the compound of phosphorus and oxygen which is formed. Place phosphorus in a graduated tube over water to show that at ordinary temperatures it combines with the oxygen of the air and removes it, so that by measuring the volume of gas left, the amount of oxygen contained in air can be roughly determined (to introduce the phosphorus, fuse it in a test-tube under water, and introduce the end of a long wire into it, then let it cool). Burn charcoal and sulphur in oxygen, and call attention to their disappearance. Demonstrate by lime water and by litmus paper that a new body is in each case formed. Burn iron powder (*Ferrum vidactum*) on a scale pan of a balance, to show that an increase of weight occurs. A glass or metal vessel filled with ice or cold water can be used to show the condensation of mixture upon it. Place calcium chloride on the pan of a balance to show the gradual increase of weight which occurs.

WATER.—Its three states. Expansion of water by heat. Equal volumes at different temperatures have not the same weight. Formation of currents in water by heating. Boiling point. Increase of volume on conversion of water into steam. Distillation. Pure water. Hydrogen and its properties. The burning of hydrogen in air, and the weight of the product compared with the weight of the hydrogen; the difference due to oxygen of the air with which hydrogen has combined. Hence oxygen and hydrogen are the constituents of water. Combination of oxygen and hydrogen with explosion to form water. If by measure there be twice as much hydrogen as oxygen, or by weight eight times as much oxygen as hydrogen, then no gas remains—all becomes water. All water composed of these two bodies in this proportion. These two bodies can then be separated from water and can be made to make, unite, and form water. In all cases of chemical combination bodies are united in constant proportion.

Experiments.—Illustrate the characteristic properties of ice, water, and steam. Show that equal volumes of hot and cold water do not counterbalance one another. Fill a flask to the bottom of the neck with cold water, and then heat to show expansion of the water. Show current by heating a large flask of water. To illustrate distillation, distil water containing copper sulphate. Show Liebig's or other forms of condensers. Show the mode of determining the boiling point of a liquid. Show that the temperature remains constant, and that on dissolving substances in water the boiling point is raised. To show the presence of hydrogen in water, pass steam through a red-hot iron tube filled with coarse iron turnings or nails. Water formed of two components, both gaseous. Note the change in the iron both in appearance and weight. This increase of weight and the weight of gas which comes off equals weight of steam which has disappeared. Hence two substances in water, one the combustible gas that comes through the tube, the other the body which remains with the iron. Collect the hydrogen over cold water in proof that it is not steam, also show that it burns. After the tube is cooled show the iron from inside of the tube and explode the oxygen and hydrogen. Plunge a burning taper into jar of hydrogen held mouth downwards, to show burning of the gas and extinction of the taper. Show by a balloon, or soap bubbles, or inverted beaker glass suspended from a balance, that hydrogen is lighter than air. Condense the water formed by the burning of a jet of hydrogen.

Carbon.—Charcoal, graphite, or blacklead and diamond. When wood, sugar, meat, bread are heated carbon remains. Charcoal not changed in the air at ordinary temperatures. Combination of carbon with the oxygen of the air at a red heat. Carbon dioxide a compound of carbon and oxygen. Chemical combination of carbon and oxygen is attended by the evolution of a definite amount of heat expressed by amount of water it will heat. Combustion. The properties of carbon dioxide. Water dissolves carbon dioxide at ordinary temperatures. Action of carbon dioxide on lime-water; no animal can live in this gas. 100 parts of carbon dioxide are composed of 27·27 parts of carbon, and 72·73 parts of oxygen. Carbon dioxide obtained from marble, limestone, oyster-shells, chalk, &c. Charcoal fire. Coal composed of carbon, hydrogen, and a little oxygen, &c.; its burning is the carbon and the hydrogen combining with oxygen. Whenever oil, tallow, coal gas are burnt this carbon dioxide and oxide of hydrogen (water) are formed. Respiration produces similar changes. In expired air the same products arise as from the burning of the food, and there is the same evolution of heat. Carbon a constituent of all animal and vegetable bodies.

Experiments.—Specimens of charcoal. Make charcoal by heating wood in covered crucible. The black lead of a pencil as a specimen of graphite. Sugar heated on piece of tin plate. Show that acids and alkalies do not change charcoal, but that when heated it soon burns away, and only ash is left. Take a small piece of charcoal in a glass tube, pass air over it into lime water, and show no change takes place until the charcoal is made red hot; as the charcoal disappears the lime water becomes milky. Show by means of the balance, or by soap bubbles, or by passing it from one vessel to another, that carbon dioxide is heavier than air, that it acts on lime water, that a burning candle is extinguished in it. Its solubility in water shown by agitating a tube of the gas over water. Prepare the gas from marble by the action on it of dilute acid. Collect all the gas given off from a small piece of marble weighing 5 or 10 grains. Show by collecting in inverted beaker the products of combustion of a candle, of a lamp, and of a gas flame, and adding lime water, that carbon dioxide is given off. Show also by means of lime water that respired air contains this gas.

SULPHUR.—Known also as brimstone. Where found. Its properties. Is also found chemically combined with many metals, so not recognisable by the eye. Sulphur heated in the air melts; more strongly heated it burns, then the sulphur disappears; the strong smell produced belongs to a new body formed by the burning, a compound of sulphur and oxygen. Gaseous properties of the new body, its effect on blue litmus paper, which oxygen and sulphur have not. Its composition is 50·00 parts of sulphur and 50·00 parts of oxygen, and it is called sulphur dioxide. Water dissolves nearly fifty times its volume of this gas, and then turns blue litmus strongly red and has an acid taste. The combination of the gas and water to form sulphurous acid. Another compound of sulphur and oxygen can be made, in which the same weight of sulphur is combined with more oxygen. One hundred parts contain 40 of sulphur and 60 of oxygen, and it is called sulphur trioxide. Sulphur trioxide has properties differing from the dioxide. If the dioxide and oxygen be mixed they do not combine, but if they are passed over hot platinum dense white fumes are formed, which are the trioxide. Combination of the trioxide with water to form sulphuric acid (oil of vitriol).

Experiments.—Show roll and flowers of sulphur and specimen of native sulphur, also iron pyrites and other native sulphides. Powder iron pyrites and heat it in a tube held horizontally over a lamp to show the sulphur obtained from the pyrites. Show the

melting of sulphur by heating flowers of sulphur in small flask. Heat sulphur on a piece of tin plate till it catches fire, show the colour of the flame and observe the smell of burning sulphur. Prepare sulphur dioxide by heating copper turnings in sulphuric acid, and show that it extinguishes flame, is very soluble in water, and that the water dissolving it becomes very acid, turning blue litmus red. Bubble air through a strong solution of sulphur dioxide, and then over platinised asbestos; demonstrate that when the platinised asbestos is hot dense white fumes are formed of the sulphur trioxide. Pour some sulphuric acid into 20 or 30 times its volume of water and prove its acid taste, its action on litmus, and its power of causing effervescence if dropped on sodium carbonate. Show that sulphuric acid is a colourless liquid, that bulk for bulk it is much heavier, more than $1\frac{3}{4}$ times, than water. Show by a thermometer or by immersing a test tube with spirit in it that a large amount of heat is evolved when this acid is poured into water. Pour so ne on sugar or shake it up with oil to show its action on organic bodies.

CHLORINE.—The gas obtained by the action of hydrochloric acid on the black oxide of manganese. So called on account of its colour. Its characteristic smell. Is $2\frac{1}{2}$ times heavier than air and $35\frac{1}{2}$ times heavier than hydrogen. Soluble in water. Many substances take fire in chlorine gas, *e.g.*, phosphorus, and form chlorides. Ignition of oil of turpentine in chlorine with separation of carbon and formation of hydrochloric acid. Bleaching power of chlorine. Bleaching-powder.

Experiments.—Samples of common salt, rock-salt. Prepare chlorine from (1) mixture of common salt, black oxide of manganese and sulphuric acid; (2) from mixture of black oxide of manganese and hydrochloric acid. Collect gas by downward displacement. Draw attention to its colour, and show that phosphorus spontaneously inflames in the gas to form chemical compound of phosphorus and chlorine. Show that oil of turpentine ignites spontaneously in chlorine. Show that sodium when strongly heated burns in chlorine and forms common salt. Show bleaching action of chlorine by dipping moistened Turkey red rag in bottle filled with gas. Show similar action with solution of bleaching powder and acid. Show that chlorine is soluble in water and that the solution has characteristic smell and colour of the gas.

ACIDS.—Are bodies which have sour taste, turn blue litmus red, and liberate carbon dioxide when added to solution of sodium carbonate. Sulphuric acid has these properties. Its specific gravity. Colourless when pure. Evolves heat on being mixed with water. There are two other common bodies which have strong acid properties like sulphuric acid, these are nitric acid and hydrochloric or muriatic acid; these are made of different constituents from sulphuric acid. All act on litmus, &c. in same way; all can be neutralised by potash forming potassium sulphate, or nitrate, or chloride. The compound formed by the union of an acid and alkali is called a salt. All three acids are colourless liquids, but, beside the properties possessed by all acids, each acid has properties which belong to it alone. Nitric acid attacks most metals. Poured on copper the metal is dissolved and red fumes are formed. Hydrochloric acid does not dissolve copper, is not so heavy as sulphuric acid; when mixed with manganese dioxide gives off a yellow irrespirable gas known as chlorine.

Experiments.—Samples of both nitric and hydrochloric acid. Show that they have all the properties belonging to acids and that by neutralising them common salt and nitre can be made. Show the action of nitric acid on copper, tin foil, &c. Show that it has no action on platinum or on gold. Copper placed in hydrochloric acid not attacked, but if mixed with manganese dioxide and warmed chlorine is given off.

ALKALIES.—Are another class of bodies which turn red litmus blue; have soapy taste and absorb carbon dioxide. If potash be added gradually to sulphuric acid the properties of both bodies gradually disappear, and at last a liquid is obtained that has no action on litmus. The combination of acid and alkali and the body formation of sulphate of potash or potassium sulphate; sulphate of soda or sulphate of ammonia can be formed in a similar manner.

Experiments.—Show that solutions of potash, soda, and ammonia turn reddened litmus blue, and that when a tube containing carbon dioxide is inverted in any of these solutions the gas is absorbed. The taste of these bodies is soapy not sour. Add gradually to dilute sulphuric acid one of these bodies, and see that the acid character of the dilute sulphuric acid disappears. Neutralise exactly sulphuric acid with potash, then evaporate and crystallise out the salt formed.

AMMONIA.—A gas with a very pungent smell. Solution in water. One volume of water dissolves 800 volumes of ammonia. This liquid has the pungent smell of the gas, and it can neutralise the strongest acids. Formation of ammonium chloride or sal ammoniac by ammonia with hydrochloric acid. Ammonium chloride a white solid, soluble in water, with no smell of ammonia. Ammonium chloride a volatile body. The effect of boiling a solution of ammonium chloride with lime or potash. Ammonia is composed of 82·3 parts of nitrogen and 17·7 parts of hydrogen. The pungent odour of smelling salts is due to ammonia. Animal matters, such as horn, dried flesh, glue, cheese, isinglass, heated so as to decompose these bodies, yield ammonia. The formation of ammonia in large quantities by heating coal to make coal gas. Production of ammonia when animal matters containing nitrogen putrify.

Experiments.—Prepare ammonia by treating ammonium chloride with an equal weight of slaked lime and enough water to make the whole into a thick mud; and demonstrate its smell, its action on red litmus, and its great solubility in water. The gas passed into water, the increase of volume of the liquid. Its properties and their identity with those of the gas. Volatility of ammonia shown by the liquid leaving no residue on evaporation. Show that ammonium chloride is formed by neutralising a solution of ammonia with hydrochloric acid, and is obtained as a solid on evaporation, and that on further heating it is volatilised. Heat coal in a coarse powder in a glass tube, and show that the liquid obtained is very alkaline. Show the formation of ammonia by the addition of potash and lime to a solution of ammonium chloride.

LIME AND CLAY.—Limestone, marble, oyster-shells, chalk, all contain a metal known as calcium. The oxide of this metal known as lime. Lime and carbon dioxide are together present in limestone, marble, shells, and chalk. When these are strongly heated, especially in a current of air, the carbon dioxide is evolved and the lime is left. Action of water on lime. Its use in making mortar. Lime slightly soluble in water. On blowing carbon dioxide into a clear solution of lime (lime-water), liquid becomes turbid, owing to combination of carbon dioxide and lime to form chalk. Same effect on breathing through lime-water. Other important salts of lime are gypsum or plaster of Paris (sulphate of lime) and phosphate of lime, which exists largely in bone. Clay is a combination of a body called silica, which is the chief constituent of sand and flint, with the oxide of a metal known as aluminium, so called because it exists also in alum. Glass is a compound of silica with lime and an alkali, potash or soda. Varieties of clay; their use in manufacture of bricks and pots. The metal of clay (aluminium), a white body with a brilliant lustre, $2\frac{1}{2}$ times heavier than water; may be rolled out into thin sheets and drawn into fine wire. Not oxidised in the air.

Experiments.—Samples of limestone, marble, oyster shells. Show that these substances effervesce with dilute hydrochloric acid, and that a gas carbon dioxide is evolved. Heat a piece of limestone or marble to redness in a fire, and show that after heating it no longer gives off carbon dioxide on treatment with an acid. Describe process of lime-burning. Properties of lime as distinguished from limestone. Show that a piece of moistened red litmus paper pressed against limestone is not affected, but that when pressed against lime it is turned blue. Show slaking of lime; draw attention to heat evolved. No such result on treating limestone with water. Show that lime is soluble in water, whereas limestone is not. Add carbon dioxide to the solution of lime, and show that white powder is formed which on treatment with acid evolves carbon dioxide again. Explain that white powder thus formed is identical in chemical composition with limestone, and hence that limestone is a compound of carbon dioxide and lime. Explain use of lime in making mortar. Various samples of clay are used in manufacture of bricks and pots. Show plasticity of clay and exhibit one or two specimens of ware before being baked. Show that a vessel of kneaded or "puddled" clay will hold water. Explain chemical nature of clay, and show specimens of silica and alumina. Show alum and demonstrate that alumina is contained in it by heating ammonia alum. Show specimen of aluminium and explain that this metal is contained in alumina and therefore in clay.

METALS, INTRODUCTORY.—About 70 different elementary subjects known. Almost all the common metals are elements. For instance, iron, lead, copper, zinc, mercury, silver, gold, tin, are elements. All combine with oxygen to form oxides, with chlorine to form chlorides, and with sulphur to form sulphides.

Experiment.—Specimens of metallic and non-metallic elements and of oxides and sulphides.

LEAD.—Its colour; a fresh surface bright, but soon tarnishes in the air. Is heavy. Lead is $11\frac{1}{2}$ times heavier than water. Can be beaten or rolled into thin sheet or drawn into wire. Melts at temperature 633° F. Can be cast in a mould. Its combination when liquid with oxygen. Formation of lead oxide. The oxide has entirely different properties from lead. Removal of the oxygen when heated with carbon and the formation of metallic lead. Formation of red lead by heating the oxide. Solution of lead by nitric acid and the formation of lead nitrate. Solution of lead oxide by nitric acid and the formation of lead nitrate. Similarly to potassium nitrate this is to be termed "a salt." Its solution in water. Other salts of lead, chloride, sulphate. Formation of sulphate and chloride of lead, their insolubility in water. Galena, or lead sulphide, one of the ores from which lead is obtained.

Experiments.—Piece of lead to scrape and show it is then bright and has "metallic" appearance. Show by balance that compared with water it is bulk for bulk much heavier. Show the metal beaten out into thin sheet, also as wire. Melt lead in an iron spoon, and cast in a mould. Show formation of oxide by blowing air on to the melted metal. Contrast the properties of the oxide with those of the metal. Convert the oxide again into metal by strongly heating an intimate mixture of it and charcoal powder. Heat the oxide to show its further oxidation and the formation of red lead. Show the action of nitric acid on lead, also on lead oxide, and the formation of lead nitrate. Show this is "salt," and prove that it is soluble in water. Demonstrate that the first is very slightly soluble, and the last almost insoluble in water. Show the formation of chloride and sulphate of lead by the addition of the respective acids to a solution of lead nitrate. Collect on filter the salts so formed, wash and dry them. Show specimen of Galena (lead sulphide).

IRON.—Not used in a pure condition, always obtained united with carbon. Three kinds of iron; wrought iron, cast iron, and steel. Wrought iron the purest and used if the body is to be formed by hammering. Cast iron contains most carbon. Steel used for cutting instruments; can be made into a magnet; can be "annealed." Solubility of all three forms of iron in sulphuric, nitric, and in hydrochloric acids, and the formation of iron sulphate, nitrate, and chloride. Their solubility in water. Melting point of iron is at much higher temperature than that of lead. Comparison of the weight of iron with that of water. Its colour. The ready action of air on it. Formation of rust. Oxidation by heating. The action of steam on iron when red hot. Oxide of iron heated with hydrogen or with carbon parts with its oxygen, and iron is left. Oxide of iron found in the earth. Hæmatite. A carbonate of iron mixed with clay used as a source of iron. Heating the ore the iron is converted into oxide. Removal of the oxygen by heating it to a very high temperature with carbon. Formation of slag from clay and lime.

Experiments.—Specimens of the different kinds of iron, wrought iron, cast iron, and steel. Dissolve cast iron in hydrochloric acid diluted with equal volume of water, show carbon which remains, filter and evaporate the liquid to show the chloride of iron formed. Heat iron wire by the blowpipe to show the high temperature required to fuse it. Iron acted on by air and moisture to show its rusting. Heat iron oxide in a tube and pass hydrogen over it to show formation of water and metallic iron. Show specimens of iron ores, clay iron stone. Hæmatite magnetic iron ore, and slag.

COPPER.—Its colour. Does not rust in air at ordinary temperatures. Thin wire melts in flame of Bunsen burner. When heated in air becomes black, owing to formation of an oxide. Oxide heated in hydrogen gas yields up its oxygen, water is formed, and the red-coloured copper is obtained. Action of acids on copper. With dilute nitric acid evolves a colourless gas, which turns red in contact with the air, and the metal dissolves, forming a green solution of copper nitrate. Heated with sulphuric acid copper yields sulphur dioxide, the same gas which is formed when sulphur burns in air or in oxygen. Substance formed when copper dissolved in sulphuric acid is when crystallised from water of a fine blue colour, known as copper sulphate or blue vitriol. Action of vegetable acids on copper. Verdigris. Use of copper In alloys. A penny composed of 95 parts of copper, 4 parts of tin, and 1 part of zinc. Bell metal and gun metal contain copper and tin.

Experiments.—Show specimens of copper in bar, sheet, and wire. Point out characteristic colour of metal. Heat piece of sheet copper over flame of Bunsen burner. Show formation of black film. Explain its origin. Take black oxide of copper and heat in hydrogen gas. Show that metal is again formed and that water is produced. Show action of nitric and sulphuric acids upon copper. Exhibit specimen of copper sulphate (blue vitriol). Show that on placing a knife blade in a solution of copper sulphate metallic copper is formed on the steel. Show sample of verdigris and explain how formed. Show various alloys of copper, bell-metal; brass, gun-metal, &c., a penny-piece.

MERCURY.—A liquid metal, but if it be cooled to − 40° Fahrenheit it is solid. Its metallic appearance. Its weight; heaviest liquid known; 13·6 times heavier than water. Use in the barometer and thermometer. Does not rust or tarnish in the air at ordinary temperatures, oxidation if heated to about 600° F. in the air, and the formation of red mercuric oxide. Is readily attacked and dissolved by nitric acid. It dissolves many metals, —*e.g.*, tin, lead, &c.; amalgams. Mercury in combination with sulphur, as cinnabar. Mercury can be obtained from any salt of mercury by heat, volatilization of mercury, and the condensation of the vapour.

Experiments.—Specimen of mercury. Show that to balance a given volume of mercury $13\frac{1}{2}$ volumes of water are necessary. Boil a little mercury in a tube to show it vaporizes. Treat mercury with nitric acid and show its solution. Show that tin foil is dissolved by mercury, which becomes less fluid. Heat mercuric oxide in a tube and collect both the oxygen and the mercury. Heat mercuric chloride in tube sealed at one end with dry sodium carbonate and show the metallic mercury condensed on the side of the tube.

SODIUM.—Common salt contains a metal combined with chlorine known as sodium. 100 parts of common salt contain 39·3 parts of Sodium and 60·7 parts of Chlorine. Carbonate of soda (washing soda) contains sodium. Sodium obtained on strongly heating carbonate of soda with charcoal. Sodium one of the lightest solids known. Swims on the surface of water and decomposes that liquid with evolution of hydrogen and formation of the alkali soda. Other properties of the metal sodium: its low fusibility and softness. Its tarnishing in air. Preservation of sodium from action of air by being kept in same liquid lighter than water and free from oxygen.

Experiments.—Samples of common salt and rock salt; also washing soda and bicarbonate of soda. Recall experiment showing that chlorine is constituent of common salt. Show that washing soda and sodium bicarbonate evolve carbon dioxide on treatment with an acid. Common salt a compound of chlorine with a metal called sodium; bicarbonate of soda and washing soda compounds of carbon dioxide and sodium. Sodium can be made by strongly heating sodium carbonate with charcoal. Exhibit specimen of portions sodium. Show that it can be cut with a knife, and that the so cut can be pressed together again. Exhibit metallic lustre of sodium; show that it quickly tarnishes in the air. Show that sodium is lighter than water and decomposes that liquid with evolution of gas (hydrogen). Collect hydrogen from water by thrusting small piece of sodium beneath test-tube filled with water and standing in basin of water.

CARBON COMPOUNDS.—Large numbers of substances are met with in plants and animals which are not found in the earth. Most of these bodies contain carbon. The other elements united with the carbon are hydrogen, oxygen, nitrogen; some bodies are composed of all these elements; others of only two of them. Many of these bodies when heated leave black residue of carbon; when this is more strongly heated it burns away. The great number of these carbon compounds, and the great difference in their properties. Some are acids, *e.g.*, vinegar (acetic acid), and tartaric acid. Some are salts, *e.g*, fats, tallow, butter. Some are neutral bodies, *e.g.*, sugar, starch, spirit.

Experiment.—Show that on heating any ordinary vegetable or animal substance carbon is left behind.

ACETIC ACID.—One form of dilute acetic acid is known as vinegar. Formation of acetic acid when beer or wine exposed to the air becomes sour. The spirit present combines with oxygen of the air and forms acetic acid. The presence of a kind of fungus called *mycoderma aceti* necessary to cause this oxidation. Large amount of vinegar is made from poor kinds of wine and beer. Action of vinegar on blue litmus, and on sodium carbonate. Vinegar is also made by heating wood in a retort; a great many bodies distil over, among them acetic acid. The pure acid has very pungent smell, and has all the properties which are characteristic of the acids. Boils at 246° F. Dissolves in water. It is composed of carbon, hydrogen, and oxygen in the proportion of 40·0 parts of carbon, 6·7 parts of hydrogen,

and 53·3 parts of oxygen. It is neutralised by alkalies like sulphuric acid. Iron put into it is slowly dissolved, hydrogen being given off. Oxide of lead dissolves in it, forming a salt, and if the clear solution be evaporated a white crystalline body called "sugar of lead" is formed, which is lead acetate. The vinegar smell belongs only to the acid, not to the salts. Sodium acetate has no smell; add to it sulphuric acid and warm, when the smell shows the acid has been liberated and that it is volatile.

Experiments.—Show that vinegar has the properties of an acid, and that a salt is formed on neutralising it. Show a specimen of the commercial acetic acid, and point out its colourless appearance and strong smell and acid reaction. Show that iron is acted on and dissolved by acetic acid. Make sugar of lead by dissolving lead oxide in acetic acid, and crystallise out the salt. Point out disappearance of the pungent odour of the acid on neutralisation by potash or soda. Demonstrate the liberation of the acid as indicated by the odour on addition of sulphuric acid to sodium acetate, and show that it can be separated from the liquid by distillation.

TARTARIC ACID.—Occurs in many fruits; especially in grapes. Is obtained from "argol," an impure potassium salt of tartaric acid, deposited when grape juice ferments. Tartaric acid is a crystalline solid, and dissolves easily in water. Has no smell. Is composed of carbon, hydrogen, and oxygen, *i.e.*, the same elements as are in acetic acid but in different proportions, viz.: 32·0 parts of carbon, 4·0 parts of hydrogen, and 64·0 parts of oxygen. Its action on sodium carbonate. Effervescing draughts; seidlitz powders. Tartrates.

Experiments.—Specimen of argol and of crystals of tartaric acid. Show solubility of the solid acid in water, and that the solution has acid properties and is without odour. Demonstrate the presence of carbon in the acid by ignition.

FAT AND OILS.—Are neutral bodies made up of an acid and a base, the base in all cases is glycerine, the acid varies in different oils and fats. They are all insoluble in water. Oils are liquid; fats are solid. Many of the oils are obtained from vegetables, either from the seed or fruit. Most of the fats are from animals. Melting of tallow (fat of the ox, sheep, &c.) put in boiling water. Its non-solution in water. Its lightness as compared with water. If a solution of caustic potash be added, and the solution of the liquid boiled, the fat disappears and the liquid becomes slightly milky, and nearly the whole dissolves. Combination of the potash with the acid (stearic) of the tallow and formation of potassium stearate. Previously the stearic acid was combined with glycerine. To the solution of potassium stearate hydrochloric acid is added. The potash is again separated from the stearic acid, and the stearic acid, as it cannot dissolve in water, separates out. Stearic acid dissolves in alcohol and in ether and separates out in crystals. Used in making candles, and is better than tallow because it melts at a higher temperature. Tallow distilled with steam of temperature 600° F. (high pressure steam) separates into stearic acid and glycerine, and when cold these bodies remain separate. All oils and fats are decomposed by potash in the same way as tallow.

Experiments.—Tie beef or mutton fat up in muslin bag, and melt to separate the fat from membranous matter. Show that fat is insoluble in water, that it floats on water, and melts at a temperature below boiling water. Show that oil has very similar properties to melted fat. Boil oil or fat with caustic potash. Prepare a solution of potassium stearate, and precipitate stearic acid from it by the addition of hydrochloric acid. Show the solubility of the acid in alcohol and ether, and the insolubility of the lime salt of stearic acid.

GLYCERINE.—A thick colourless liquid with a sweet taste. Dissolves readily in water. When quite pure becomes solid at a low temperature. If heated alone it is destroyed, but if heated with water in a retort it distils over with the steam. Heated with acids it combines with them, and bodies similar to fats are formed.

Experiment.—Specimen of glycerine. Demonstrate its solubility in water and its sweet taste.

SOAP.—By boiling fat with caustic soda sodium stearate is formed. On adding salt to the liquid the sodium stearate, which is soap, separates out and solidifies on the surface of the liquid. Soft soap is potassium stearate. Action of soap in washing. Action of soap on hard and on soft waters.

Experiment.—Shake distilled water up in bottle with soap. Show action of solution of salts and acids on the solution. Add soap solution to distilled water, also to common water, and explain the difference of action. Show the presence of stearic acid in soap by adding hydrochloric acid to a solution of soap.

SUGAR.—Exists in many plants. Is obtained from the sugar-cane; also from beetroot. The juice of these plants yields the sugar. When pure it is white, crystalline, sweet, and very soluble in water. Sugar candy. If heated with very little water to 365° F., on cooling it is no longer crystalline and is "barley sugar." Does not combine with acids, but even a very little acid boiled for a long time with a solution of sugar changes it to another kind of sugar. Composition of cane sugar. The several different kinds of sugar, *e.g.*, the solid part of honey is a sugar which differs from the sugar in the sugar-cane; the same found in all sweet fruits and is called grape-sugar. Grape-sugar not so sweet nor so soluble as cane-sugar.

Experiment.—Specimens of ordinary white and brown sugar; also sugar candy and barley sugar. Show its great solubility in water; also that its solution is neutral. Heat it and point out the peculiar odour it gives out, and that on further continuing the heat it leaves a residue of carbon. Wash honey with spirit, and show the residue is sugar, but that it is not sweet as ordinary sugar, and not so soluble.

STARCH.—A neutral substance, composed of carbon, hydrogen, and oxygen. Composition. Peculiar structure; not crystalline. Is found in all parts of a plant. Is obtained from wheat, rice, potatoes, arrowroot, &c. Starch in its ordinary condition insoluble in water. When starch powder is boiled with water, the membrane of starch cells bursts, and the starch is partially dissolved. Strong solutions form a jelly when cold. Used for stiffening linen. Starch recognised by its forming a blue compound with iodine. Undergoes no change in the air at ordinary temperatures; if heated to about 300° F. it becomes slightly discoloured and is changed into a soluble body, known as British gum (dextrin). If small amount of nitric or hydrochloric acid be added to the starch this change is more rapid. Extract of malt also changes starch into soluble compounds. Starch as a food.

Experiment.—Specimen (of starch), point out its peculiar structure and absence of crystalline form. Demonstrate that it does not dissolve in cold water, but on boiling some does dissolve. Show that starch both solid and in solution gives a blue colour when iodine is added to it. Moisten starch with very dilute hydrochloric acid, and heat to convert it into a gum, which is thus soluble in water.

GLUTEN.—If flour is tied up in a calico bag and well kneaded in a basin of water, the water becomes milky, and on standing starch sinks to the bottom. All the starch in the flour can thus be removed, and then a sticky substance remains in the bag called gluten. About 70 per cent. of flour is starch and 10 per cent. is gluten. Gluten contains nitrogen, starch does not. These bodies represent two most important constituents of food. The gluten exposed to the air soon decomposes and smells very disagreeably (putrifies).

Experiment.—Tie some flour up in a piece of calico and knead it for some time in a vessel of water; the starch comes through, and will settle to the bottom of the vessel, and can be collected and examined; the gluten remains in the bag.

SPIRIT.—Alcohol, spirits of wine. A colourless, light liquid. Neutral to test papers. Has pleasant odour, boils at 173° F. Burns with a flame, which gives very little light, without leaving any black residue of carbon. A large number of different bodies dissolve in it. It is the intoxicating principle in wines and spirits. In beer there is 3 to 5 per cent. of alcohol. In light wines about 8 per cent. In spirits 60 to 75 per cent. The different flavours of wines and spirits depend on very small quantities of other bodies present. Alcohol dissolves in water, giving out heat.

"Proof spirit" contains 50·76 parts of water, and 49·24 parts of alcohol. If more water be present the spirit will not set fire to gunpowder when burning. Alcohol obtained from grape sugar. Fermentation grape sugar converted into alcohol and carbon dioxide by presence of some ferment which exists in yeast. Cane sugar on the addition of yeast is first converted into grape sugar, then into alcohol and carbon dioxide. Use of yeast in brewing. Not necessary for making wine, as there is already a ferment in expressed juice of grape.

Experiment.—Show it is neutral liquid dissolving in water, that it burns with nearly colourless flame, and leaves no residue of carbon. Show that it can be made to boil at much lower temperature than water by placing test-tube of it in hot water. Distil beer and collect the alcohol and water which comes over;

add quicklime to this. Allow it to stand some hours, and distil again. Show that this is much stronger, catches fire readily, and tastes more burning. Make a solution of common sugar in a large flask; add yeast, and fit a cork with a bent tube to the flask. Let the tube dip into lime water. Place it in a warm place, and after some days show that spirit has been formed in the flask by distilling the liquid and collecting the portion coming over first.

All the substances and experiments mentioned above are to be shown to the class. This does not preclude such other experiments and illustrations as may suggest themselves to the teacher.

NOTES

WE trust that it is not in the least likely that the proposal "From a Correspondent" in Saturday's *Times* to remove the Science Museum to make way for a permanent Colonial Museum will receive serious attention in any influential quarter. For this proposal really involves the monstrous step of shunting collections which have been brought together with so much trouble and at so much expense. Their value was recognised by the Duke of Devonshire's Commission. As to the Colonial Museum we shall be in a better position to express an opinion upon it when its nature and objects are further developed. We wish in no way to disparage it; but there is room for it elsewhere. Why should its founders try to build it on the ruins of an existing and valuable collection?

THE thirty-fifth meeting of the American Association for the Advancement of Science will be held at Buffalo, from Wednesday, August 18, until Tuesday evening, August 24, 1886. For the third time, at intervals of ten years each, the Association has accepted an invitation to hold a meeting in Buffalo. The Local Committee intend to make the meeting a great success: and members who were at the meeting of 1876 need only to recall it in order to form an idea of what the coming meeting promises to be. The facilities which the city offers are all that can be desired, both in regard to rooms for the several Sections and in hotel accommodation, while the health and comfort of the city in the month of August are well known. The headquarters of the Association will be at the High School, and all the offices and meeting-rooms will be in that building or in one of the schoolhouses near by. The hotel headquarters will be at the Genesee House. A special circular in relation to railroads, hotels, and other matters, has been issued by the Local Committee. Arrangements for excursions and receptions will be announced by the Local Committee. The officers of Sections D and H have issued special circulars relating to the meeting, which can be had by addressing the respective secretaries. Special information relating to any of the Sections will be furnished by their officers. In Section E special attention will be given to the problems connected with the Niagara Falls and its gorge.

WE have only just received the *Proceedings* of the American Association for the Advancement of Science at the Philadelphia meeting of 1884. The volume is particularly well printed and fully illustrated.

THE Institution of Naval Architects is holding a summer meeting at Liverpool this week.

THE recent elections have done nothing to alter the comparatively small but distinguished band of men of science in the House of Commons. Sir John Lubbock retains his seat for London University. The electors of South Manchester have remained faithful to Sir Henry Roscoe, and those of South Leeds to Sir Lyon Playfair. Mr. Story-Maskelyne returns from North Wiltshire, and Sir Edward Reed, after one of the principal contests of the election, from Cardiff.

M. JANSSEN is continuing, at Meudon, his researches on the influence of gases on the rays of the spectrum. He is building tubes, which can be loaded with 1000 atmospheres of hydrogen, oxygen, or carbonic acid. In this last case the real density of the gas will be superior to the density of water. The filling of the tubes to these high pressures is not directly obtained by pressure; they are loaded by a sort of step-by-step or cascade process. This is a very long affair. After the filling of these tubes some time must be allowed for the settling down of the dust which has been raised by compression. As long as the cloud of minute particles is floating, the colour of the light traversing longitudinally the tubes is blood-red. This effect can be shown with a far lessened pressure.

MR. J. M. HORSBURGH has been appointed Secretary of University College, London, to enter upon his duties on October 1.

DURING the last ten years M. Marcel Deprez has been engaged in experiments connected with the transmission of force by means of electricity. The Rothschilds some time since provided him with an unlimited credit to prosecute his researches at Creil, under the inspection of a commission of thirty-eight men of science. On Friday the commission met to hear a report on the results at present obtained, drawn up at their request by M. Maurice Lévy. This report was unanimously approved. It appears from it that we can now, with only one generator and only one receiver, transport to a distance of about 35 miles a force capable of being used for industrial purposes of 52-horse power, with a yield of 45 per cent., without exceeding a current of 10 ampères. When the amount of force absorbed by the apparatus used to facilitate the recent experiment, but not required in the applications to industrial purposes, is added, the yield will be nearly 50 per cent. The commission certifies that the machines now work regularly and continuously. The maximum electromotive force is 6290 volts. Before the construction of the Marcel Deprez apparatus the maximum force did not exceed 2000 volts. The report states that this high tension does not give rise to any danger, and that no accident has occurred during the past six months. The commission is of opinion that the transmitting wire may be left uncovered on poles provided it be placed beyond the reach of the hand. It estimates at nearly 5000*l.* the probable cost of the transmission of 50-horse power round a circular line of about 70 miles. This price would, however, be much diminished if the machines were frequently constructed. The commission, in the name of science and industry, warmly congratulated M. Deprez on the admirable results which he had obtained, and expressed thanks to the Rothschilds for the generous aid extended to the undertaking.

THE eighth congress of the French Geographical Societies will meet at Nantes on the 4th proximo, and will continue until the 9th.

IT is stated that Baron de Miklouho-Maclay is now busy getting printed at St. Petersburg, by command of the Czar, the result of his scientific researches in New Guinea from 1870 to 1883.

A CONFERENCE was held by the National Fish Culture Association on Monday last at the Colonial and Indian Exhibition. Sir Albert K. Rollit, M.P., presided. The chairman, in delivering the presidential address, stated that the Association had made a great impression upon the public as to the necessity for remedial, protective, and other measures in the interest of our fishing industries and population. The Association was doing work which many other nations and colonies thought it expedient and economical to do upon a much larger and more expensive scale. He therefore thought the public ought to support it liberally in order to enable it to carry out the work which could

not be left undone without serious danger to one of the greatest industries of this country. Mr. Oldham Chambers then read a paper upon "Carp Culture," which was followed by the Rev. C. J. Steward with a paper upon "Marine Temperatures and their Influence upon Fishes," and Mr. Willis-Bund with a paper upon "The Influence of the Weather upon the Migration of Fish." After the several papers had been fully discussed, the proceedings terminated with a vote of thanks to the chairman for presiding.

IN a recent article the *Ceylon Observer* refers to the power of the coco-nut palm to conduct lightning. Sir Emerson Tennent long ago pointed out that this tree acts as a conductor in protecting houses from lightning, and in one instance 500 palms were struck in a single plantation during a succession of thunderstorms in April 1859. But the trees themselves suffer terribly in the process, for however slightly they may be touched by the electric fluid, they die. Sometimes only the edges of the branches are singed, at others a few leaves turned brown alone show where the tree was touched, yet however slight the apparent effect, in course of time the tree withers gradually and dies. In conclusion the journal quoted inquires why it is that coco-nut palms which have merely had their external parts, their foliage, almost imperceptibly singed should be as much doomed to death as those which have had their vital parts permeated by the lightning, the fatal result being only protracted in the one case, while it is instantaneous in the other.

AT Nottingham the University College, Technical Schools, and a Museum are under the same committee as the Free Library; and from the not very carefully edited Report of that committee the convenience of combination is further shown by special classes being opened at the first of them for elementary school teachers, while the expense is defrayed by the School Board. The Technical Schools, whose evening class students we trust will far exceed in number the regular daily pupils, are largely supported by voluntary contributions from the Drapers' Company; so natural and appropriate a use to put such money to, that it is to be hoped that the reforming spirit of the age will lead to the same commendable action voluntarily on the part of all such old trade guilds. As might easily have been foreseen, commercial classes held during the working hours of all those who felt their need have failed; just like the free library at the Guildhall, London, so discreetly opened at first from ten till four —just the hours when the pressure of business was greatest upon every one to whom it could be of any use! An experience of the Nottingham Committee seems to be that the highest working power of their money may be obtained by opening district branches, modestly termed reading rooms, yet each the germ of a branch library; and also that the most economical size for a free library, as far as supplying literature to the working classes is concerned, is from three to four thousand volumes; their libraries of that size having a greater circulation in proportion than either the larger or the smaller ones.

WE have received the first number of a new technical journal named *Industries*, published in Manchester. As its name implies it is the intention of the publishers that it shall cover all the ground of the manifold manufactures of the country, and include the dissemination of technical education in its widest and most useful form. A new departure is taken in technical journalism, inasmuch as the publishers propose to offer a series of substantial rewards to those of their readers who may bring forward some new and useful advance in practical science. These awards will be granted on certain conditions being fulfilled, and will be made by the editors, assisted by two or more gentlemen eminent in the particular science. As a commencement it is the intention to arrange rewards in each of the following subjects, viz. engineering, electricity, and chemistry. In order to increase the value of the reward, and should the successful reader desire it, a patent will be obtained by the publisher for the invention or process, which will be presented to the successful reader before the invention is described in the paper, and further, if a model is necessary, this also will be added to the reward. This inducement ought to add considerably to the subscribers' list, and ensure the success of the paper; at the same time it will without doubt increase, the number of workers and probably add to the already large list of inventions. The paper is nicely printed, and the woodcuts are well executed. We find an illustrated Patent record at the end of the journal, and arrangements are to be made so as to include the more important American and German Patent Specifications. Journals of this class add greatly to the general advancement of technical education, and we wish the new venture every success in its youth and a strong and sturdy future.

A MOSCOW journal states that it is contemplated establishing a university for women in that city, founded on private capital. It is to have three faculties—a mathematical, a natural history one (with medicinal studies), and a philological. Doubt is, however, expressed whether the Government will sanction the scheme.

FROM several parts of Western Norway complaints are being received of the great destruction of fir and spruce cones by a little unknown insect.

FROM the report of the Swedish Academy of Sciences for last year, it appears that the National Museum—which is under the authority of the Academy—succeeded in acquiring some splendid specimens of topazes from Brazil, containing fluids on which experiments are now in progress in the Academy's chemical laboratory. Some specimens of argyrodite, containing the new element germanium, were also acquired, with which Profs. Nilsson and Petterson have been experimenting. The museum for lower invertebrates acquired from Lieut. Sandberg a very valuable collection of lower marine animals from the shores of Northern Russia, collected by this gentleman during his extensive journeys in these parts. At the Academy's biological station on the coast of the province of Bohus, aquaria were kept in perfect working order from June till October. The station was visited by many *savants* for zoological studies. Through Dr. Carlson's researches on the former existence in Sweden of *Trapa natans*, the National Museum has become possessed of more than a thousand sub-fossil trapa fruits from Southern Sweden. In addition to the sums granted by the Government towards scientific researches on the recommendation of the Academy (p. 201), several others were made out of the funds administrated by that institution. Among them are the interest on the large sum left by the late Dr. Regwell for zoological studies, distributed for the first time; a sum of 250*l.* to Dr. S. Arrhenius for the study of the galvanic conductive force of electrolytes, and their relation to physics, at certain institutions in Russia, Germany, and Holland. Prof. Agardh was awarded the yearly Lettersted prize for his celebrated work "Fresh contributions to the Systematic of the Algæ." The Lettersted fund for scientific research amounted at the end of the year to 30,000*l.* A number of smaller sums were also granted by the Academy towards researches on a variety of scientific subjects.

MR. OTIS T. MASON sends us a reprint of his valuable paper on the progress of anthropology in 1885, which was originally embodied in the Smithsonian report for that year. Here anthropology is used in a very wide sense, giving scope to remarks on comparative psychology, biology, archæology, and sociology, as well as to the more closely-connected subjects of general and special ethnology. Some of the more important recent works

on these several branches are noticed in greater or less detail, and there is appended an ample "Bibliography of Anthropology, 1885," which is arranged in alphabetical order, and which will be found most useful for purposes of general reference. It includes not only independent works and memoirs, but also special papers bearing on the subject, which have been contributed in the specified period to various English, American, French, German, and other scientific journals. Amongst the essays more fully noticed are M. Gabriel de Mortillet's work on "The Precursor of Man," advancing the theory that the flints of Thenay were the workmanship, not of man as fully developed, but of his immediate predecessor, the anthropopithecus; Dr. Lissauer's paper on human craniology, introducing the sagittal suture as a new element in obtaining anthropological measurements; Dr. Hermann Welcker's treatise on the capacity of the cranium in connection with the three diameters, with classifications of races according to their skull capacity; Dr. Topinard's masterly work on general anthropology, from which copious extracts are made; Dr. Otto Stoll's contribution to the comparative philology of Central America, embodying a scientific classification of the eighteen languages still current in Guatemala. Here the Maya family is specially dealt with and divided into four distinct groups: Tzental (Chendal), Pokonchi, Quiché, and Marne. It is incidentally mentioned that in 1885 the Woman's Anthropological Society was organised in Washington under the presidentship of Mrs. Tilly Stevenson. The object of this association is stated to be "to conduct investigations to which the avenues are especially open to women, and to encourage the sex in the prosecution of scientific work."

THE catalogue of the Library of the Chemical Society, arranged according to subjects with indexes containing authors' names and subjects, will be useful to chemists.

VOL. I. of "Studies from the Biological Laboratories of the Owens College" (Manchester: Cornish) is mainly a reprint of papers that have appeared in various journals.

WE have received the last (19th) Report of the Peabody Institute of Baltimore. There is no marked advance over past years in any department, but all have been prosperous and the results attained have been satisfactory. The attendance at the courses of lectures was exceptionally large, but the use of the library has been somewhat reduced owing to the opening of another free public library in Baltimore. Amongst the lectures during the year were courses on Arctic Explorations and Life in the Arctic Regions, on Mexico, Ancient and Modern, on the Mound Builders of Ohio, and on the Poetry of Science.

MR. C. G. ROCKWOOD, jun., of Princeton, N.J., writes that the shock of earthquake at Sandy Hook, New York, of June 11, noticed in NATURE of June 17 (p. 153) is an error. The tremor which was felt in that vicinity at the time stated, and which was at first reported as an earthquake, was afterwards traced to the firing of heavy guns on board the U.S.S. *Juniata*, at that time approaching Sandy Hook.

THE additions to the Zoological Society's Gardens during the past week include a White-handed Capuchin (*Cebus hypoleucus*) from Brazil, presented by Madam Sangiorgi; a Levaillant's Cynictis (*Cynictis penicillata* ♂), five Suricates (*Suricata tetradactyla* ♂ ♂ ♀ ♀ ♀), two Triangular Spotted Pigeons (*Columba guinea*), three Vinaceous Turtle Doves (*Turtur vinaceus*), two Cape Turtle Doves (*Turtur capicola*) from South Africa, presented by Mr. R. A. Fairclough; two Red Foxes (*Canis fulvus* ♂ ♀) from North America, presented by Messrs. Enson, Weber, and Co.; a Masked Paradoxure (*Paradoxurus larvatus*) from Hong Kong, presented by Mr. J. Orange; five Forster's Milvagos (*Milvago australis*) from the Falkland Islands, presented by Mr. James Moore; a Tawny Owl (*Syrnium aluco*), British, presented by Master C. G. Gregory; five Common Toads (*Bufo vulgaris*) from the South of France, presented by Mrs. F. Walker; a King Vulture (*Gypagus papa*) from Brazil, deposited; three Lions (*Felis leo* ♂ ♀ ♀) from Africa, a Grey Squirrel (*Sciurus cinereus*), a Mink (*Putorius vison*), three Hudson's Bay Squirrels (*Sciurus hudsonius*), a Virginian Eagle Owl (*Bubo virginianus*) from North America, purchased; two Mule Deer (*Cariacus macrotis* ♂ ♂), born in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *AUGUST* 1–7

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on August 1

Sun rises, 4h. 26m.; souths, 12h. 6m. 4·9s.; sets, 19h. 46m.; decl. on meridian, 17° 59′ N.: Sidereal Time at Sunset, 16h. 27m.

Moon (one day after New) rises, 5h. 59m.; souths, 13h. 19m.; sets, 20h. 26m.; decl. on meridian, 11° 23′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury ...	6 43	13 26	20 9	7 52 N.
Venus	1 45	9 54	18 3	22 29 N.
Mars	10 55	16 23	21 51	7 0 S.
Jupiter	9 27	15 31	21 35	0 5 N.
Saturn	2 21	10 28	18 35	22 10 N.

Occultations of Stars by the Moon (visible at Greenwich)

Aug.	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
5	95 Virginis	6	21 54	22 53	108 297
5	94 Virginis	6	22 3	near approach	202 —

Aug.	h.	
1	15	Mercury stationary.

Variable Stars

Star	R.A. h. m.	Decl. ° ′		h. m.
U Cephei	0 52·2	81 16 N.	Aug.	2, 22 30 *m*
λ Tauri	3 54·4	12 10 N.	,,	3, 22 12 *m*
U Monocerotis ...	7 25·4	9 32 S.	,,	6, *M*
S Virginis	13 27·0	6 37 S.	,,	1, *m*
V Coronæ	15 45·5	39 55 N.	,,	5, *M*
U Ophiuchi	17 10·8	1 20 N.	,,	2, 0 38 *m*
R Scuti	18 41·4	5 50 S.	,,	1, *M*
η Aquilæ	19 46·7	0 43 N.	,,	5, 21 30 *m*
δ Cephei	22 24·9	57 50 N.	,,	4, 0 0 *m*

M signifies maximum; *m* minimum.

Meteor Showers

Showers have been observed at this season from the following radiants:—The *Andromedes* (I.), from R.A. 8°, Decl. 36° N.; from Camelopardus, R.A. 12°, Decl. 70° N.; near η Persei (the *Perseids*), R.A. 45°, Decl. 56° N.; near η Herculis, R.A. 254°, Decl. 37° N.; near λ Aquarii, R.A. 342°, Decl. 9° S.; from Lacerta, R.A. 342°, Decl. 40° N.; and one near Fomalhaut, R.A. 342°, Decl. 34° S.

Stars with Remarkable Spectra

Name of Star	R.A. 1886·0 h. m. s.	Decl. 1886·0 ° ′	Type of spectrum
D.M. + 44° 3877 ...	21 31 42	44 51·9 N. ...	III.
249*a* Schjellerup ...	21 37 13	34 59·4 N. ..	IV.
μ Cephei	21 40 1	58 15·4 N. ...	III.
254 Schjellerup ...	21 40 41	2 44·4 S. ...	III.
258 Schjellerup ...	21 59 1	27 47·8 N. ...	III.
18 Cephei...	22 0 26	62 33·8 N. ...	III.
D.M. + 56° 2821 ...	22 34 8	56 12·2 N. ...	III.
β Pegasi	22 58 14	27 27·8 N. ...	III.

THE VOLCANIC ERUPTION IN NEW ZEALAND

FURTHER details have been received of the volcanic eruption in the Hot Lake district of New Zealand, which has been the scene of a terrible and unexpected catastrophe which

occurred early on the morning of June 10. At Ohinemutu, on Lake Rotorua, the earth began to tremble at midnight. At ten minutes past 2 a.m. there was a heavy quake and a loud roaring noise which startled the inhabitants and caused them to flee from their houses. From this point it could be seen that Mount Tarawera, about fourteen miles distant, had suddenly become an active volcano, belching out fire and lava to a great height. At 4 a.m. a dense mass of ashes poured down, accompanied by suffocating smells. A large black cloud, which extended in a line from Mount Tarawera to the Pairoa Mountains, was filled with flame and electricity. The thunder-like roar from the crater, the sulphurous smells and constant quaking of the earth, caused many of the inhabitants to leave their homes and proceed to Tauranga, a distant township on the east coast. At 8 a.m. the aspect of affairs was as bad as it was all night, and hundreds of boiling springs had broken out around Lake Rotorua. At 11 a.m. the eruptions were going on continuously, and all the country down to Tauranga was in total darkness, with thick clouds of dust and sulphurous fumes in the air. At one o'clock the darkness had all cleared away and the principal centres of eruption had subsided.

At Lake Taupo, about forty miles to the south of Tarawera, the spectacle presented was most extraordinary. At 2 a.m. the outbreak was heralded by terrific reports, which resembled the roar of artillery, while a pillar of flame shooting hundreds of feet into the air was observed in the locality of the eruption. A great black cloud hung over this pillar, whilst flashes of electricity shot out from the cloud in every direction, shedding an unearthly bluish light. Loud reports, accompanied by heavy shocks of earthquake, followed in quick succession, and kept on until six o'clock, when the daylight and clouds of ashes rendered the sight invisible.

At Tauranga, forty-seven miles distant to the north-east, loud reports of heavy earthquakes began at 2.15 a.m., and very severe shocks were experienced at 3 a.m., while in the direction of the outbreak the country was illuminated for hours with flames and lightning. In the neighbourhood of Maketu, fifty miles to the east of the principal centre of eruption, atmospheric disturbances caused darkness till 10 a.m., and the shocks of earthquake were accompanied with strong lightning and earth currents, while at Hamilton, eighty miles to the west, the volcanic discharges are said to have resembled the firing of great guns at sea.

The devastation caused by the eruption is very widespread, and it is believed that all the natives round Rotomahana and Tarawera Lakes must have died. About a hundred Maoris are known to have perished. Mr. Haszard, the schoolmaster at Wairoa, and his four children and niece, and an English tourist named Bambridge are the only Europeans known to be killed. The country for fifty miles around the vicinity of the disturbance has been covered by the discharge of stones and ashes, and large craters have risen up. Mount Tarawera is elevated 300 feet higher than before. Lake Rotomahana has subsided, and has been transformed into an expanse of seething mud, and the renowned terraces are reported to be destroyed. Large areas are covered with volcanic dust and mud. During the disturbance the wind blew from the east, heavy snow fell on the ranges, and there was intense cold.

The wide area forming the scene of the eruption has been the chief centre of volcanic activity in New Zealand since the country has been known to Europeans, and in fact since the earliest period of Maori tradition. The region forms of itself a distinct volcanic zone remarkable for its picturesque hot lakes, boiling geysers, and numerous thermal springs. For many years it has been the resort of tourists from all parts of the world, and who reached it by way of Tauranga, a picturesque town on the east coast of the North Island, with a fine harbour opening into the Bay of Plenty. It is in this bay, about 30 miles from the mainland, that the first indication of volcanic activity presents itself in the form of Whakari, or White Island, a cone-shaped mountain which rises abruptly from the sea to an altitude of 860 feet. The crater, about $1\frac{1}{2}$ miles in circumference, is in a condition of a very active *solfatara*, whose numerous geysers and boiling springs evolve at all times dense volumes of steam and sulphurous gases.

From Tauranga the traveller proceeds in a southerly direction through a fern-clad country interspersed with broad belts of primæval forest presenting the most luxuriant and varied vegetation. In a distance little short of 40 miles the land rises gradually to an altitude of 800 feet, when the great table-land of the Lake Region is reached. Here, in a depression which appears to have formed at some remote period the area of an immense lake-basin, is situated the township of Ohinemutu, where there are several good hotels and a small white population. Just beneath the township the blue surface of Lake Rotorua, with the picturesque Island of Mokoia in its centre, spreads itself out in a circle of nearly 25 miles. The area in the immediate vicinity of the lake, where the action of the thermal springs is most active, extends from Whakarewrewa on the one side to Te Koutu on the other, and inland to Tikitere and Ariki Kapa, celebrated for its big holes of black boiling mud. Hot springs occur on its southern shore, while still further to the east of it again are the warm lakes known as Rotorua and Rotoehu. The native settlement is situated on a long peninsula stretching out into the waters of Lake Rotorua. Every part of this strip of land is dotted and riddled with thermal springs, some of which shoot out of the ground from small apertures, while others assume the forms of large steaming pools. They are of all degrees of temperature from tepid heat to boiling-point. Here the *whares* or huts of the natives are clustered promiscuously about the springs, and in situations where a few inches below the surface the soil is sufficiently hot to cook an egg in a few minutes.

It is this region which may be said to contribute the first link in the chain of active thermal action, extending from Whakari in the Bay of Plenty, through the Lake Country, to the active volcano of Tongariro, in the centre of the island, a distance in a direct line of about 130 miles.

At a distance of about nine miles still southerly from Lake Rotorua, lies Lake Tarawera, with its cluster of minor lakes, which constitute the second and most important connection in the volcanic belt. It was here the recent volcanic disturbance first declared itself in the sudden activity of Mount Tarawera, an extinct volcanic cone which had remained quiescent since time immemorial.

No place in the world could boast of scenery so unique and thermal phenomena so marvellous as could Tarawera and its surroundings. It was reached from Tarawera by a delightful route fringed by fern-clad mountains, and through the Sikitapu Forest one of the grandest gardens of primæval vegetation in New Zealand, but which is now uprooted by the force of the subterranean devastation. At the southern exit of the forest the traveller was charmed by Sikitapu, the Blue Lake, and Rotokakahi, the Green Lake, whose calm picturesque beauty formed one of the grandest sights of this singularly gifted region. A few yards from this point nestled the native settlement of Wairoa, now covered with 10 feet of ashes. Here were two hotels for the accommodation of tourists, who came from places far and wide to visit the wonders of the Terraces. From time out of mind it had been one of the principal homes of the great Arara tribe, who claim to be the pioneers of the race in New Zealand.

Down a wild gorge from Waira Lake Tarawera lies embosomed in a circle of tall forest-clad mountains, whose pointed peaks and serrated ridges betoken at once their Plutonic origin, while on the southern shore of the Lake rises Mount Tarawera, in the form of a colossal truncated cone, with pointed peaks like a spiked crown. It was out of this giant mountain *tapu*, and sacred in Maori song and legend, that the recent subterranean fires first shot forth, enveloping the whole mountain in a sheet of flame.

A glance at this mountain and the surrounding region was sufficient to show that at some remote period it must have been —as now—the chief centre of a widely-extended volcanic action. The mountain itself formed one of the principal volcanic cones to be found dotted over the country. A range of volcanic hills sloped down on its western side to Lake Rotomahana, which was connected with Lake Tarawera by a small warm stream known as Te Ariki. Before the eruption occurred the shores of the former lake formed the principal point of thermal activity in the district, and there can be little doubt that beneath its surface the forces which culminated in the outbreak of Mount Tarawera were evolved.

Lake Rotomahana, now said to be nothing more than a hole of seething mud and vaporous gases, formed in reality the wonderland of the region. Like Lake Tarawera, it was situated at an elevation of a little over 1000 feet above the level of the sea. It was one of the smallest of the group of lakes, being about a mile long by a quarter of a mile wide. It was, however, grandly picturesque, not only by reason of its unequalled features presented by the terraces, but likewise on account of

its steaming shores with their countless hot springs, boiling geysers, steaming cauldrons, and seething mud-pools, as well as by the bold, rugged scenery which surrounded it on every side. The name Rotomahana in the native language means literally "hot lake." The mean temperature of the water was about 80° F., while in the vicinity of the hot springs it rose frequently to 212° F.

It was on either shore of this lake that the marvellous terraces now unfortunately reported to be destroyed were situated. The largest of these singular formations was Te Tarata, or the White Terrace, the outline of which assumed a semicircular form and spread out at its base as it sloped gently down to the margin of the lake; the broad, flat, rounded steps of pure white silica rose tier above tier white and smooth as Parian marble and above them terrace after terrace mounted upwards, rounded and semicircular in form. All were formed out of a delicate tracery of silica, which appeared like lacework congealed into alabaster of the purest hue; crystal pools shaped as if to resemble the form of shells and leaves, and filled to their brims with water blue and shining as liquid turquoise charmed the eye, while around the edges bright crystals of silica formed incrustations which made them appear as if set with a margin of miniature pearls. At the summit of the terrace was a crater of 200 feet in diameter filled to overflowing with brilliant transparent water in the form of a boiling fountain, from which clouds of steam floated constantly upward. This boiling spring formed an intermittent geyser, which during its active intervals threw up a column of water to a height of over 100 feet. The crater, however, was always overflowing, and the water, which was highly charged with silica, had by a gradual process of deposition, extending probably over a vast period, formed the present system of terraces. The temperature of the water varied from boiling point to 70° F. at the foot of the terrace, the summit of which was over 80 feet above the level of the lake.

Immediately at the back of the White Terrace and bordering the lake was a rocky desolate gorge seamed and furrowed in every direction with streams of hot water, while jets of hissing steam bursting from its sides marked the sites of subterranean fires. The high hills on each side of the gorge rose up in quaint fantastic shape, and their rugged sides composed of shattered volcanic rock sent forth water and jets of steam from a thousand fissures. Here boiling geysers emitting clouds of steam lashed their hot waves about and foamed with a furious sound in rock-bound basins, while scattered over the greater portion of this fiery wilderness were innumerable fumaroles all hard at work shooting out steam and vomiting black streams of liquid mud. Some of these were round, some flat, and others cup-shaped, while not a few assumed the form of miniature volcanoes.

It was opposite to this spot on the further shore of the lake, that Te Otukapurangi, or the "Fountain of the Clouded Sky" of the Maoris, or the Pink Terrace, rose from the water of the lake to an altitude of nearly 100 feet. Here the deposits of silica assumed the same general formation, and each terrace of steps was gracefully and marvellously shaped with rounded edges which swept about in waving curves. The various buttress-like masses which supported the fringed edges of the terraces bent over and formed miniature grottoes resplendent with festoons of pink-tinted silica and rose-coloured stalactites which appeared to have been woven together by nature into an intricate network and then crystallised into their present shape. Here the successive deposits or layers of silica-rock did not assume, like those of Te Tarata, a wonderful combination of delicate lacework around the edges of the terraces, but the siliceous laminations appeared even thinner, and reminded one of the corrugated surface of pink satin rep. It was, however, the variegated tints of this wondrous structure which rendered it even more remarkable than the gracefully symmetrical proportions of its incomparable design. As the blue-tinted water came rippling and falling from terrace to terrace in miniature cascades, Te Otukapurangi looked radiant in its sparkling mantle of delicate pink, and as the golden rays of the sun shot far and wide, it changed with every shade of light, with brilliant hues of pink, amber, carmine, and yellow, which shone with a dazzling and metallic lustre as they flashed and palpitated as it were in the warm glowing air.

At the summit of the terrace was a circular platform, in the centre of which was a steaming cauldron formed by an alabaster-like basin about 100 feet in diameter. Here the deep dark-blue water within a few degrees of boiling-point lay without a ripple upon its surface, and shone with the brilliancy of transparent crystal, while beneath the siliceous deposits, which encrusted the sides of the crater, assumed all the varied designs of a coral grove tinted in glowing colours of yellow, blue, and pink.

From Lake Rotomahana the recent volcanic eruption extended to the Pairoa Mountains, which attain to an altitude of 1000 feet, and which, when visited by Mr. Kerry-Nicholls, were hot, and quaking with internal fires, boiling mud pools, and coiling jets of steam that burst with a hissing sound from the deeply-scarred hills. The base of this range, where the volcanic action was greatest, was formed of a burnt fiery-looking earth, broken here and there into enormous fissures, and dotted about with boiling pools and deep holes of hot seething mud, while clouds of vapoury steam burst forth from the highest peaks.

Following up the line of thermal activity across the island, as yet not known to be affected by the recent outbreak, hot springs and geysers are found at Orakeikorako on the banks of the Waikato and in various places along the whole valley of the rivers, and notably at Wairakei, where the thermal activity is both widespread and extraordinary in its variety. At Taupo, the great central lake of the island, geysers and other phenomena of the kind exist on its northern shores. From this point further across the lake the hot springs and geysers of Tokanu occur, while a short distance beyond rises the cone-shaped form of Tongariro, at an altitude of 7000 feet, the two craters which are in a state of very active *solfatara* constantly emit vast volumes of steam. Five miles to the south of the latter mountain rises the colossal form of Mount Ruapehu, which, with a base of over sixty miles, rises to an altitude of 6000 feet to the region of perpetual snow. This mountain, which was at one time the chief centre of volcanic activity of the north island, has been extinct from time immemorial, but it is reported that during the recent eruption steam was seen to issue from the crater. It is the highest point of the north island, and was ascended by Mr. Kerry-Nicholls and his interpreter, Mr. Turner, in 1883.

SCIENCE IN NEW SOUTH WALES

IN his Annual Address (on May 5) to the Royal Society of New South Wales, the President, Prof. Liversidge, referred to the death of Prof. Smith, the former President of the Society, and to the eminent services which he had rendered to the cause of science and of education in New South Wales, and also to other members of the Society who died during the past year. The President then expressed regret that the number of original papers contributed to the Society is so small. "It is not," he said, "from lack of subjects, for there are many questions which require investigation, but rather from the lack of competent investigators who can spare the necessary time. Up to the present but little original work has been done in working out the chemistry of the mineral and vegetable products, and but very little in many branches of biology. The descriptions, catalogues, lists, &c., of the flora and fauna, are making fair progress, but still very little has been published relating to the development and life-history of the fauna of Australia, even of forms of life peculiar to that part of the world. In matters of natural history, geology, and allied subjects it is apparent to every one that the materials for original work are in New South Wales abundant, and a considerable amount of very valuable work is being done in this direction by the Linnean Society of New South Wales, but the amount waiting to be done is far more than they can cope with at present. The Society, by offering a medal and a money prize, has done what it can to stimulate research; but the amount at its disposal is small. So many subjects if thoroughly worked out would be of economic value to the colony—such as the chemistry of Australian gums and resins, the tin deposits, iron ores, and silver ores of New South Wales —that the Government might with propriety assist the Society in undertaking these researches. Wealthy colonists might also, with advantage to the State and credit to themselves, encourage such original investigation." Speaking of biological work, the President said that one of the few facilities for scientific work possessed in Sydney, and which the Society assisted in founding, viz. the biological laboratory at Watson's Bay, has been closed, the Government having taken the house and grounds for defence purposes. The trustees will doubtless receive the cost of the buildings, and with this as a nucleus a fresh start can be made. It would be a great pity to allow such an undertaking to drop, especially as there is such an unlimited field for marine biological work in Australia. In regard to scientific education

in the colony, Prof. Liversidge said that notwithstanding the liberality of Parliament and the receipt of private endowments for improved instruction in science, many of the arrangements for this purpose of the Sydney University are of a very meagre and imperfect character. The Board of Technical Education is now doing good work in spreading elementary, scientific, and technical education over the colony by means of science classes and itinerant lecturers. The necessity of scientific education is also being recognised; there is a motion before the Legislative Assembly to place the sum of 10,000*l.* upon the estimates for the establishment of schools of mines in the various mining centres, while another motion to be brought proposes to make provision for the creation and endowment of twenty scholarships of the value of 200*l.* per annum, each tenable for three years, at the Sydney University. The President then referred to Prof. Huxley's remarks in his anniversary address to the Royal Society on scientific federation. Prof Huxley said:—"I have often ventured to dream that the Royal Society might associate itself in some special way with all English-speaking men of science; that it might recognise their work in other ways than by the rare opportunities at present offered by election to our foreign fellowship, while they must needs be deprived of part of its privileges." On this Prof. Liversidge remarks that though every one will agree as to the desirability of having closer bonds of union between the Royal Society and the men of science who are scattered over the wide areas of English-speaking countries, it does not appear easy to suggest a method of bringing it about. Good work in the Colonies, at any rate at present, is rarely overlooked by the Council of the Royal Society. Prof. Liversidge concluded his interesting address by suggesting a federation or union of the members of the various scientific societies in Australia, Tasmania, and New Zealand nto an Australasian Association for the Advancement of Science, on the lines of the British Association, with a view to holding the first general meeting in Sydney on the hundredth anniversary of the founding of the colony. A meeting of the kind during the centennial year would offer a unique opportunity for the exchange of ideas and information, and it would not only have an immediate and beneficial effect, but would permanently raise the high-water mark of thought in all the colonies, especially in connection with scientific matters. It would be an opportunity to correlate and correct all the scattered and fragmentary geological maps and memoirs relating to the various colonies, and to adopt a uniform system of nomenclature, colouring, &c., for all Australasian geological maps. It would, pursued the President, be beneficial if botanists were to prepare and revise the census of plants for each colony, especially to show their distribution, and similar questions could be discussed by zoologists for land and marine organisms.

ICE MOVEMENTS IN HUDSON'S BAY[1]

In my report last year I described the ice as consisting of three kinds, viz., icebergs, heavy arctic ice and ordinary field ice. The icebergs are stated to have come from Fox Channel. This conclusion was based on the report from No. 3 station made on the homeward voyage of the *Neptune*, that the icebergs passed the bluff from west towards east. This report was made on the strength of the few observations which the party had been able to make in the interval between the two calls of the *Neptune* at the inlet. Further and more perfect observations show conclusively that the current sets in the opposite direction and that the icebergs move from east to west. If further proof of the existence of this set were necessary, we have it in the drift of the *Alert* when fast in the ice off Ashe Inlet and invariably carried to the westward.

In considering the question of the sources from which the ice affecting Hudson's Straits navigation comes, we must first begin with the east Greenland ice. All those who have made the voyage from any port in Europe to Hudson's Straits seem to agree in the statement that Cape Farewell must not be approached nearer than seventy miles in order to keep clear of the east Greenland ice which sweeps round the cape in an almost ceaseless stream, after rounding which it turns to the northward, and passes up the south-west shore of Greenland, nearly as high as Gothaab, then turns over to the west side of Davis' Straits, and joining the stream of Davis' Straits ice runs south with the arctic current. The limits of the east Greenland ice field, when rounding Cape Farewell, vary greatly; in some years, it moves as far south as the parallel of 58° N. This ice field can be, and is of course always avoided, the rule in making the passage being to keep to the south of 58° N. till in longtitude 58° W., on which meridian the northing should be made.

The stream of Davis' Straits ice flows right across the entrance to Hudson's Straits, and varies in width with the season of the year. The first information which I have of it was derived from conversation with Captain Watson, of the whaling barque *Maude* of Dundee, owned by Captain Adams. Captain Watson had been for many years engaged in the Davis' Straits whale fishing, and for the last few years has commanded his present vessel. Their usual routine is to leave Dundee in March, and they arrive off the edge of Davis' Straits ice in the early part of April, cruising off the edge of the ice between latitudes 58° N. and 63° N. Captain Watson told me, that he made the ice in April of this year about 58° N. and 120 miles off the Labrador coast, and up to the date of our meeting with him, June 13, he had not been able to get nearer to Resolution Island than thirty-five miles, and as the average southerly set of the current is about twenty miles per day, this stream of ice must have been flowing uninterruptedly up to June 15, the date on which the *Alert* took the pack. An examination of the records of the stations at Port Burwell and Nachvak Bay shows that at Port Burwell the ice cleared out of the Straits on April 9. They remained clear up to the 14th, when the ice came in sight again, and was present almost constantly thereafter until its final disappearance in August. At Nachvak the ice swung on and off the shore with the winds and tide, but though sometimes out of sight from the ordinary observation point, it was always seen upon going to a higher elevation. It is therefore certain that during the months of May, June, and July, large fields of ice were present in the entrance of the Straits, and the question remains, at what date was this ice in such a condition as to permit the passage of vessels strengthened for meeting the ice, but which could be used as freight steamers. For in all questions as to feasibility of the navigation I am not considering the date at which one of the Dundee whaling or Newfoundland sealing steamers could be forced through, but when a strongly built iron steamer, sheathed and otherwise strengthened, could make the passage.

On June 15, when we went into the ice, it was certainly impenetrable by any vessel of the class referred to, and though the ice would slacken at the turn of every tide, and sometimes run abroad so that it would have been possible to work the ship to the westward, distances varying from two to five miles at each of these slack times, I only tried to hold my own, generally under canvas; as apart from any question of the injury which the ship had received, I deemed it more desirable to watch the ice at the entrance of the Straits than to force the ship through, when I could only have made at the most ten to twenty miles a day. I am of opinion that the Straits were passable at the eastern entrance about the date that we returned to St. John's for repairs, viz., July 5, but any ship going in at this date would still have been subject to these delays, but might have made from twenty-five to forty miles a day.

Proceeding westward, from this date, July 5, the observations at Ashe Inlet and Stupart's Bay show that on the north side of the Straits, and from eighteen to twenty miles out, the ice was present almost continuously, much as we found it in August; some of the sheets of enormous extent and of great thickness. Many of these were, in August, over half a mile long, and some which we measured were from twenty to thirty feet in thickness. In the middle of July, Mr. Ashe reports that open water is visible beyond the ice, and Mr. Stupart, fog-banks and water sky frequently to the north. The two stations at the western end of the Straits also report that in the middle of July the ice was loose and drifting with the tide. Everything goes to show that though there would have been very frequent delays still it would have been possible for a steamship to have got through the Straits by July 15 or 20.

Ice would have been met with again, doubtless, in the bay, but I do not think there would have been any serious delay in reaching either Churchill or York Factory.

Stations on shore for the purpose of watching the movements of the ice, though undoubtedly the best system which we can adopt, cannot tell us with any degree of certainty how soon a vessel might be able to push her way through the Straits, but they do tell when it is sufficiently run abroad, or

[1] From the Report of the second Hudson's Bay Expedition under the ommand of Lieut. A. R. Gordon, R.N., 1885.

when a sufficient amount of open water appears, to make the passage a reasonable certainty, and the date for this year I place at from July 5 to 15, as it is more than likely that a ship could have got through the Straits in ten days. The ice is, moreover, so sensitive to wind that even if telegraph stations were so placed as to be able to convey to ships news regarding the position of the ice ahead, long before the vessel arrived at the place the condition of affairs might, and probably would, be totally changed.

As to the closing of navigation in 1884, Mr. Laperrière reports, at Cape Digges, that on October 25 the ice was solid in every direction, and at Nottingham Island a similar entry is made on the 27th. A distinction must be made between the closing of navigation by the formation of young ice, and the presence of a large field of heavy old ice which is cemented together by the formation of young ice between the pans. In the first case any ordinarily powerful steamer could go through without risk, but in the second case the most powerful of the whaling or sealing steamers would be helpless. The western end of the Straits is always subject to incursions of this heavy ice, from Fox Channel, and especially so in the months of September and October, when strong north-easterly and north-westerly gales are frequent, and we have now evidence that in both seasons, 1884 and 1885, this heavy ice came down in October.

As to the length of season for practical navigation, if we regard the presence of field ice as the only barrier, the information which we have got would point to the months of July, August, September, and October as being the months in which the Straits are passable. As a rule, in July there will be delays, but to vessels strengthened and sheathed there would be no danger in making the passage.

All the inhabitants of the Labrador, the Straits, and the Bay, spoken to on the subject, agreed in stating that the ice movements this year were much later than the average; at Fort Churchill the season was fully a month late, and on the Labrador three weeks, so that I think it will be found that on the average four months will be the length of the season for practical navigation by steam vessels which would be freight-carriers. There have been, I am informed, seasons when the Straits were clear of ice in the month of June, but they are, according to the logs of the Hudson's Bay ships, quite exceptional. Captain Hawes spoke of such being the case only once in his experience of fourteen years, and the dates which I have seen of the arrival of the Hudson's Bay vessels at their ports of destination show no arrival earlier than August.

THE TRANSCASPIAN FAUNA

WE notice in one of the last issues of the *Bulletin* of the Moscow Society of Naturalists (1885, No. 2) a most valuable paper, by M. Zaroudnoi, on the birds of the Transcaspian region. His list contains an enumeration of 184 species, well determined on 600 specimens—doubts remaining only with regard to a very few species. The author distinguishes in the region the following chief zoological sub-regions:—(1) The Kara-kum desert, having a pretty well furnished flora, notwithstanding its immense sandy plains and salt clays. The Tamarix forests, now mostly destroyed, are well peopled with the *Atraphornis aralensis*, as also with a few *Podoces* (*Panderi?*) and *Passer* (*ammodendri?*), which make their nests further north in the *saksaul* forests. The *Houbara quennii*, Gray, is rare. The reptiles are represented by the *Phrynocephalus interscapularis* and *helioscopus*, *Agama sanguinolenta*, *Testudo*, *Naja oxiana*, Eichwald; the *Varanus scincus* extends much further south into the Akhal-Tekke plain, and even to the Kopet-dagh Mountains. (2) The Akhal-Tekke oasis, striking by the monotony of its landscape, diversified only by the gardens of the Tekkes, which remain green even during the hottest part of the summer, when all vegetation is scorched up by the sun. In the plain only the Tamarix, a few willows on the banks of the rivulets, and the dark-green bushes of the capers, adorned with pretty flowers, are to be seen. The great areas covered with bushes of *Alchagi camelorum* and wormwood increase the monotony of the landscape. Pretty *Julodis variolarius*, *eufraticus*, and sometimes *globicollis* are often found flying around these bushes; in July the *Fisheria baetica*, Ramb., several Irises, as also *Empus pennicornis*, Pall., several kinds of *Ateuchus* and *Copris*, and numerous species of *Melanozomatæ* are met with. The stone-chatters (traquets) and larks are so numerous as to become troublesome. The *Phrynocephalus helioscopus* and *Agama sanguinolenta* fly at the approach of man. From time to time a *dscheiran*, or a fox, may be perceived. The nights are sultry and hot, and one hears the shrivelling of the *Grillus cerisyi*, Serv., and *G. capensis*, Fabr., the barking of the jackals, and the cries of *Caprimulgus arenicolor*, Sev. The banks of the few rivers, covered with brush and reed-grass, are the refuge of the wild cat and the *Lagomys*. The high summer temperature of the oasis is well known: 40° Cels. in the shade being not uncommon; and M. Zaroudnoi is inclined to ascribe to the great heat the intensity of the moulting of birds. The lark loses so much of its feathers that the body remains in many parts quite naked; with the stone-chatters only the base of the feathers remains on their heads. Most of the birds met with in the oasis during the summer belong to the Aral-Caspian fauna, the others come from the mountains; these last have followed the courses of the rivers and have taken possession of the Akhal-Tekke gardens; such are the *Salicipasser montanus*, *Passer indicus*, *Sylvia mystacea*, *Butalis grisola*, a great number of *Salicicariæ*, and several others. Some, like the griffons, the ravens, the *Cypselus apus*, the *Chelidon urbica*, the *Merops apiaster*, inhabit the mountains, and descend to the plain only for hunting. The *Galerita magna*, *Calandrella pispoleta*, and *Saxicola isabellina*, may be considered as representatives of the Akhal-Tekke fauna owing to their considerable numbers. (3) The mountain-region is much more interesting, especially when the traveller reaches the upper valleys covered with forests, where the vines grow wild. Wild cats and jackals are the usual inhabitants of these valleys; but the *Cynailurus jubatus* and the *Leopardus pardus* are rare; *L. irbis* is never met with in the region. *Hyæna striata* is occasionally met with. *Ellobis talpinus*, several *Erinaceus* and *Platycercomys*, as also *Histrix hirsutirostrix* are common. The dreadful *Vipera eufratica* is a source of continual danger during the grape-harvest. *Eremias velox* and *Agama sanguinolenta* are worthy of notice. As to the birds, we must merely refer to the list of M. Zaroudnoi, where notes as to their distribution are given in French. The zoological determinations have been revised by M. Menzbier.

SOCIETIES AND ACADEMIES

London

Royal Society, May 27.—"Researches in Stellar Photography." By the Rev. Prof. Pritchard, F.R.S.

The objects of these researches are:—(1) To ascertain, if possible, by means of definite and accurate measurement, as distinguished from impressions and estimates, what is the relation between the diameter of a star-disk impressed on a photographic plate with a given exposure, and its photometric magnitude, instrumentally determined. With this view, five plates of the Pleiades were taken with different exposures, on different nights. The diameters of the star-disks on each of the plates were then measured with a double-image micrometer, checked by measurement also with the macro-micrometer in the Oxford University Observatory. Curves were then drawn for each of the plates, taking the magnitudes as given in the "Uranometria Nova Oxoniensis" as abscissæ, and the measured diameters as ordinates. The result was a satisfactory coincidence in the case of all the plates, leading, when treated in the usual manner, to the final result—

$$D - D' = \delta \left\{\log M' - \log M\right\} \quad . \quad . \quad . \quad (1)$$

where D, D' are the measured diameters of any two stars on the plate, and M, M' the corresponding photometric magnitudes; δ being a definite constant depending on the physical circumstances of the particular plate.

It was observable that, out of twenty-eight stars examined, three stood out from the rest, indicating, as might have been expected, some peculiarity in the spectra of these stars. In the memoir itself the tabular relations of all the measures are exhibited. The similarity of the symbolical form above to the relations existing between "magnitude" and intensity of light is obvious and interesting.

(2) Another branch of the inquiry is still more important, and it is this. Seeing that in the modern use of the dry plates the times of exposure are so considerable, and the processes of development and drying, &c., so suspiciously dangerous to the stability of the films, it becomes a matter of great importance to ascertain

whether the photographic plate remains an absolutely accurate picture of the actual relative positions of the stars in the sky itself, and, moreover, whether these are measurable with that extreme degree of precision which is attainable with the best instrumental means. To ascertain this, the same plates for a portion of the Pleiades were taken which gave rise to the formula already obtained. The distances of some twenty-five of the stars from Alcyone were measured on each of these plates, the number of repetitions of the measures being made the same as those adopted by Bessel in his measures of the same distances with his heliometer. The resulting accordances of these individual measures on each of the plates was very satisfactory, and a trifle better than the accordances in the case of Bessel's measures; and the accordance of each of the means of the distances of each of the stars from Alcyone on each plate was at least quite equal to the results obtained with the heliometer. The average deviations from the mean for all the measures was, in the case of the photographic plates, 0''·24, and, of the heliometer measures, 0''·29. These satisfactory accordances of the resulting measures on each of the plates (corrected for refraction, and where necessary for aberration), afford a sure indication of the reality of the pictures, as well as of their accurate measurability.

An interesting circumstance occurred in the course of the work. On one of the plates the distances of three stars from Alcyone exhibited a slight discordance of from 0''·75 to 1''·5, when compared with those stars on the other plates. These three stars all occupied a small area on the plate; no discordances occurred on this plate with respect to any of the other stars. Here is an indication of a slight disturbance of the film on one small portion of the plate, but on no other portion. Hence the necessity of the precaution of taking at least three plates for the purpose of security of measurement. The plates were exposed variously for about 8 to 12 minutes in the focal plane of the de la Rue reflector of 13 inches aperture.

(3) A few stars were examined on the same plate with different exposures, varying from 1 second to 120, with the view of ascertaining, if possible, the relation between the areas of the impressed star-disks and their time of exposure. As far as at present appears, these areas vary as the square root of the time. This result differs widely from that obtained by Bond in 1858. That astronomer considered that these areas varied directly as the time; the investigation, however, is not yet complete, and will be resumed at Oxford. It is well known that these photographic disks are not sharply and definitely cut circles on the negative plate, when examined with the higher powers of the microscope, such as 100 and beyond; but they are fringed with a number of discreet black dots extending to some distance beyond the hard photographic images. Nevertheless, these images, when printed in the form of position, lose this fringe, and present the appearance of well-defined sharply-cut circles; the light appears to have penetrated through the interstices of the discrete fringe, and leaves a very definite outline. Encouraged by these results of the measurements of the stars from Alcyone, I propose to test this photographic method still further by applying it, not without hope of success, to the question of stellar parallax.

These measures are now well advanced, and afford good hope of success.

Chemical Society, June 17.—Dr. Hugo Müller, F.R.S., President, in the chair.—The following were elected Fellows of the Society:—Thomas Akitt, James Blake, M.D., Alfred Chaston, A. W. H Chapman, Augusto Cæsar Diojo, Charles A. R. Jowitt, Charles Alexander Kohn, John Temple Leow, William Ray, Joseph Price Remington, William Richards, Forbes Rickard, William Saunders, Charles A. Smith.—The following papers were read:—The electrolysis of aqueous solutions of sulphuric acid, with special reference to the forms of oxygen obtained, by Prof. H. McLeod, F.R.S.—Essential oils (Part III.): their specific refractive and dispersive power, by Dr. J. H. Gladstone, F.R.S.—The formation and destruction of nitrates and nitrites in artificial solutions and in river and well waters, by J. M. H. Munro, D.Sc.—Water of crystallisation, by W. W. J. Nicol, M.A., D.Sc., F.R.S.E.—A method of investigating the constitution of azo-, diazo-, and analogous compounds, by R. Meldola, F.R.S., and F. W. Streatfeild.—The estimation of free oxygen in water, by Miss K. I. Williams and Prof. W. Ramsay.—Note as to the existence of an allotropic modification of nitrogen, by Miss K. I. Williams and Prof. Ramsay.—The presence of a reducing agent, probably hydrogen peroxide, in natural water, by Prof. Ramsay.—Evaporation and dissociation (Part IV.); a study of the thermal properties of acetic acid, by W. Ramsay, Ph.D., and Sydney Young, D.Sc.—Note on the vapour-densities of chloral ethyl-alcoholate, by William Ramsay, Ph.D., and Sydney Young, D.Sc.—The nature of liquids as shown by a study of the thermal properties of stable and of dissociable bodies, by William Ramsay, Ph.D., and Sydney Young, D.Sc.—The electromotive forces developed during the combination of cadmium and iodine in presence of water, by A. P. Laurie, B.A., B.Sc.—Detection and estimation of iodine, bromine, and chlorine, by M. Dechan.—The analysis of alloys and minerals containing the heavy metals, selenium, tellurium, &c., by Thomas Bayley.

Entomological Society, July 7.—Mr. J. Jenner Weir, F.L.S., Vice-President, in the chair.—Mr. S. H. Scudder, of Cambridge, Mass., U.S., was elected a Foreign Member of the Society.—The Rev. H. S. Gorham exhibited specimens of *Eucnemis capucina*, Ahr., a species new to Britain, discovered in June last in an old beech tree in the New Forest. He also exhibited specimens of *Cassida chloris*.—Dr. Sharp exhibited larvæ of *Meloe*, and read notes on their habits, and Mr. Saunders exhibited a specimen of *Halictus* infested with about thirty *Meloë* larvæ. Mr. Billups remarked that he had recently found forty-seven larvæ of *Meloë* on the body of a species of *Eucera*. Dr. Sharp said that he was of opinion that the operations of these larvæ were not the result of instinct, but were more like reflex actions: the instant the larvæ touched a suitable surface they clung to it. The discussion was continued by Prof. Riley, who disagreed with Dr. Sharp, and believed these larvæ were guided by instinct, as they showed a decided preference for particular hosts.—Mr. Jenner Weir exhibited a male of *Lycæna bellargus* and a female of *L. icarus*, which had been captured *in copulâ* by Mr. Hillman, and shown to the exhibitor at the time of capture. Mr. Weir also exhibited some specimens of *Lycæna* which he believed to be hybrids between *Lycæna bellargus* and *L. icarus*; and he further exhibited, on behalf of Mr. Jenner, four specimens of *Phosphænus hemipterus*, taken at Lewes.—The Rev. W. W. Fowler exhibited two specimens of *Chrysomela cerealis*, lately taken by Dr. Ellis on Snowdon; and also two specimens of *Actocharis Readingii*, found at Falmouth by Mr. J. J. Walker.—Mr. E. B. Poulton called attention to the fact that the larvæ of some Lepidoptera, if fed in captivity on an unusual food-plant, subsequently refused to eat their ordinary food-plant. He stated that he had observed this with the larvæ of *Pygæra bucephala* and *Smerinthus ocellatus*. Mr. Stainton, Mr. Fowler, and Mr. Goss made some remarks on the subject.—Mr. Elisha exhibited a series of bred specimens of *Geometra smaragdaria*.—M. Alfred Wailly exhibited a long series of silk-producing moths, including some remarkable hybrids between *P. cecropia* and *P. ceanothi*; and Prof. Riley and Mr. Weir made some observations on these hybrids.—Dr. Sharp read a paper on *Eucnemis capucina* (Ahr.) and its larva.—Mr. Dunning read a report on the subject of the importation of humble-bees into New Zealand, from which it appeared that the efforts of Mr. Nottidge, of Ashford, and the Canterbury (N.Z.) Acclimatisation Society had been successful, and that the long wanted clover-fertiliser had at length been established in New Zealand.—M. Peringuey communicated notes on some Coleopterous insects of the family *Paussidæ*.—Mr. J. B. Bridgman communicated additions to the Rev. T. A. Marshall's Catalogue of British Ichneumonidæ.—Prof. Riley read notes on the phytophagic habit, and on alternation of generation, in the genus *Isosoma*. In this paper Prof. Riley described, from direct observation, the phytophagic habit in two species of the genus. He also established the existence of alternation of generation.

EDINBURGH

Royal Society, July 5.—The Hon. Lord Maclaren, Vice-President, in the chair.—In a paper on the electrical resistance of nickel at high temperatures, Prof. C. G. Knott, of Tokio University, gave an account of experiments on certain nickel wires, in which the temperature was carried to a fairly bright red head. The resistance at different temperatures was compared with the resistance of a platinum wire at the same temperatures; and, by the substitution of other metals for the nickel, further comparisons were established. Nickel, platinum, palladium, and iron were thus studied; and the general conclusions were as follows:—(1) The rate of growth of the resist-

ance of a given nickel wire with temperature is greater, on the average, than the corresponding quantity for platinum or palladium, and less than that for iron. (2) The "logarithmic rate"—that is, the rate of change per unit rise of temperature of unit resistance at any temperature—falls off more slowly for nickel as the temperature rises to 200° C. than for platinum or palladium. (3) At about 200° C. the rate of resistance growth for nickel increases markedly, and continues practically steady, till about 320° C., when a sudden decrease occurs, and thereafter the resistance steadily increases at this diminished rate. In other words, between the limits of temperature specified, the slope of the resistance curve is much steeper than for any other. The same peculiarity is probably possessed by iron between the temperatures of a dull red and a bright red heat. (4) The peculiarity occurs (in each case) between the limits of temperature within which the striking thermo-electric peculiarity discovered by Tait also occurs. This peculiarity, which is most briefly described as an abrupt change in the sign of the Thomson effect, is not known to be possessed by any other metal. (5) There is thus a strong presumption that the Thomson effect in metals has a close connection with the mutual relations of resistance and temperature—at any rate in metals in which the Thomson effect is proportional to the absolute temperature (according to Tait's theory) the "logarithmic rate" of change of resistance seems to be very approximately inversely as the absolute temperature. In nickel and iron, in which the law of the Thomson effect is peculiar, such a simple relation between resistance and temperature does not hold.—Prof. Tait discussed the effect of external forces on a system of colliding spheres. He gave a proof, much more simple than Maxwell's, of the fact that gravity has no effect in altering a uniform distribution of temperature throughout a vertical gaseous column. His proof is founded on the assumption that, in a horizontal layer of gas which has arrived at a steady state, all particles passing across the upper surface do so on the whole as if they had freely passed through the layer.—Dr. John Murray read a paper by Dr. H. B. Guppy on the mode of formation of the coral reefs of the Solomon Islands. In this paper the typical reefs were described with the various corals growing on them. In places exposed to the full sweep of the trade-winds the corals do not grow higher than to about 7 or 10 feet from the surface. In sheltered places they are found at a depth of from 4 to 5 feet. Dr. Guppy believes that the reefs never rise to the surface without upheaval. He gives a theory of the construction of barrier reefs, which corresponds to that formerly given by Le Comte to explain the reefs of Florida.—Mr. J. T. Cunningham, of the Scottish Marine Station, read a paper on the eggs and early stages of some teleosteans, and also a paper on the reproductive organs of *Bdellostoma*, and a teleostean egg from the West Coast of Africa.—Mr. Patrick Geddes gave a synthetic outline of the history of biology, and also read a paper on the theory of growth, reproduction, heredity, and sex.

Sydney

Linnean Society of New South Wales, May 26.—Prof. W. J. Stephens, M.A., F.G.S., in the chair.—The following papers were read:—Notes on some Australian Tertiary fossils, by Capt. F. W. Hutton. In this paper, which is based on the examination of a fine collection of Australian Tertiary fossils recently sent to the Canterbury Museum by Prof. Tate of Adelaide, Capt. Hutton enumerates seventeen species of molluscs and echinoderms which are common to the Tertiary strata both of Australia and New Zealand, and deals with their synonymy.—On some further evidences of glaciation in the Australian Alps, by James Stirling, F.L.S., communicated by C. S. Wilkinson, F.G.S. After reviewing what has been written on the subject of glacial action in Australia, the author adduces fresh evidence in favour of such action, obtained by himself and Dr. Lendenfeld during a recent visit to Mount Bogong, the highest mountain in Victoria, where erratics, perched blocks, smoothed surfaces, and old moraines were met with.—Jottings from the Biological Laboratory, Sydney University, by W. A. Haswell, M.A.: (No. 7) On a method of cutting sections of delicate vegetable structures; (No. 8) on the vocal organs of the Cicada.—Mount Wilson and its ferns, by P. N. Trebeck. Mr. Trebeck describes the position, geology, soil, and vegetation generally of Mount Wilson, and gives details of 15 genera of ferns, including 38 species, which were growing there in the greatest luxuriance from the very summit to a considerable distance down the slopes and gullies of the mountain.—List of the Rhizopoda of New South Wales, by Thomas Whitelegge. The list contains 24 species, with exact localities, and notes on collecting, preserving, and mounting Rhizopods. The species are mostly identical with those found in Europe, America, and India. Amongst those of interest the following may be mentioned:—*Arcella dentata*, Ehr., *Pelomyxa palustris*, Greeff, *Raphidiophrys elegans*, Hert. and Less., *Clathrulina elegans*, Ceinkowski, and *Biomyxa vagans*, Leidy.

Paris

Academy of Sciences, July 19.—M. Jurien de la Gravière, President, in the chair.—Remarks accompanying the presentation of M. de Saint-Venant's important manuscript memoir on "the resistance of fluids," by M. Boussinesq. This unpublished work, begun in 1847, and not completed till the year 1885, a short time before the author's death, embodies historical, physical, and practical considerations regarding the problem of the mutual dynamic action of a fluid and a solid, especially in the state of permanence supposed to be acquired by their movements. It comprises three parts, the first dealing with the researches of previous physicists on the impulse of fluids in motion on solid bodies encountered by them; the second showing theoretically that this impulse is connected exclusively with the "imperfection of the fluid," that is, the development of friction, which to be surmounted requires a higher pressure on the upper than on the lower surface of the submerged body; the third containing a practical calculation of the impulse experienced by a body in any indefinite fluid current.—On the displacement of ammonia by other bases, and on its quantitative analysis, by MM. Berthelot and André. It is shown that in the presence of soda the double salts yield their ammonia far less readily than the sal ammoniacs unassociated with another base; also that in the ordinary conditions of analyses magnesia is powerless to entirely displace the ammonia. With certain salts, such as ammoniaco-magnesian phosphate, the displacement is extremely slight or nil. These results must henceforth be taken into account in the analysis of earths and of other products containing organic matter associated with the phosphates or with magnesia.—On a reindeer's antler embellished with carvings found by M. Eugène Paignon at Montgaudier, by M. Albert Gaudry. This relic of the reindeer age ranks among the most interesting animal and human remains in recent years discovered by M. Paignon in the Montgaudier Caves, Tardoire Valley, Charente. It is pierced with a large hole and covered with carvings executed with such a sure hand and sentiment of form that it shows even to greater advantage under the magnifying glass than when viewed with the naked eye. One face shows two seals (*Phoca vitulina* and a larger perhaps of different species), a fish (a salmon or trout), and three twigs of plants. On the other side are two long slender animals, apparently eels, three other animal figures exactly alike but indeterminable, and an insect. This specimen of prehistoric art, of the authenticity of which there can be no doubt, has been presented by the finder to the Museum, together with several other objects from his valuable collection.—On the real position to be assigned to the fossil flora of Aix in Provence (continued), by M. G. de Saporta. The conclusion already arrived at on stratigraphic grounds, that this flora belongs to the triple series of the Upper Eocene, Tongrian, and Aquitanian, is here confirmed by the Palæontological indications themselves.—On the development in series of the potential of a homogeneous revolving body, by M. O. Callandreau.—On the variations of the absorption-spectra in non-isotropic mediums, by M. Henry Becquerel. Apart from certain anomalies here described, it may be generally assumed that for each absorption-band there is a single system of three principal rectangular directions, of such a nature that the intensity of a luminous vibration proceeding from a crystal parallel to the direction of the incidental vibration may be represented by the form $I = (a \cos^2 \alpha + b \cos^2 \beta + c \cos^2 \gamma)^2$, where α, β, γ indicate the angles of direction of the vibration with the principal directions, and a^2, b^2, c^2 the principal intensity of the radiation in question. This hypothesis seems to be confirmed by photometric measurements executed with plates of epidote.—On the decomposition of hydrofluoric acid by an electric current, by M. H. Moissan.—Note on urethane regarded from the standpoint of chemical analysis by M. Georges Jacquemin.—Action of some organic chlorides on diphenyl in the presence of the chloride of aluminium, by M. P. Adam.—On the normal propylamines, by M.

C. Vincent. The results of the author's researches are: (1) The separation of the three normal propylamines; (2) the discovery of nitrosodipropylamine; (3) the determination of the physical constants of di- and tripropylamine and of nitrosodipropylamine.—On a new creatinine (ethylamido-acetocyamidine), and on the formation of the creatinines and creatines, by M. E. Duvillier. From the author's experiments it follows, so far, that the action of cyanimide on the starch acids consists essentially in the formation of creatinines, that of creatines taking place only in a very few cases.—On a combination of stannic chloride with hydrochloric acid (chlorostannic acid), by M. R. Engel.—On the alcoholate of potassa, by M. E. J. Maumené. Referring to M. Engel's note in the last issue of the *Comptes Rendus*, the author points out that he had already determined and announced an alcoholate of potassa so far back as the year 1872 (*Les Mondes*, December 19, 1872).—Note on the antennæ of the Eunicians, by M. Et. Jourdan.—On the effects of pollinisation in the orchid family, by M. Léon Guignard. A series of experiments is described which the author has carried out for the purpose of determining the varying interval which intervenes between pollinisation and fertilisation in this group of plants.—On the amphibolic schists and gneiss, and on the limestones of Southern Andalusia, by MM. Ch. Barrois and Alb. Offret.—Fresh experiments with balloon photography: ascent of MM. A. and G. Tissandier and P. Nadar, by M. G. Tissandier. During this ascent, which took place on July 2, and lasted nearly six hours, the altitude never exceeding 1700 metres, M. Nadar took no less than thirty instantaneous photographs; of these about a dozen constitute undoubtedly the finest series of negatives yet obtained from a balloon. Amongst them were two views of Versailles at 800 metres; one of Sèvres at 600 metres; one of Ballème (Orme) at 900 metres; several perspectives of Saint-Remy (Sarthe), some at 1200 metres. During a second ascent the following week, M. Nadar secured three good views of Champigny and the banks of the Marne. These experiments place beyond all doubt the success of aërial photographic operations.

BERLIN

Physical Society, June 4.—Dr. Pringsheim spoke on a new application of the telephone for the measurement of electrical resistances, a purpose for which it had already been brought into use by Prof. Kohlrausch in cases in which the resistances were measured by means of alternating currents—in cases, that is, of fluid conductors and also in the case of wires. Dr. Pringsheim had, however, observed that in the measurement of wire-resistances by means of alternating currents the determinations by the telephone did not always concur with those of the galvanometer, and varied very much with repeated measurements. He therefore applied the telephone for measurement by means of a constant current, and that in the following manner. In the Wheatstone bridge the circuit usually occupied by the galvanometer was of constant resistance. The four sides of the wire arrangement contained the wire the resistance of which required to be measured, and the rheostat. The two free angles of the square were connected by a wire circuit in which was placed a telephone. So long as the resistances of the two sides of the bridge were not perfectly equal, a part of the current flowed through the telephone circuit, and each time this was opened a snapping was heard in the telephone. The rheostat resistance was then changed till nothing was heard on opening the telephone circuit. The sensitiveness of this method was equal to 0·04 per cent. of the total resistance.—Prof. von Helmholtz reported on his most recent investigations, which respected the "doctrine of the maximum economy of action," and communicated the interesting history of the understanding of this principle. The doctrine was first propounded by Maupertuis in 1744 in a treatise laid before the Paris Academy. This treatise contained, however, no general statement of the proposition, nor did it define the limits of its applicability, but only adduced an example. This example was, in accordance with the present state of our knowledge, not pertinent, and had no relation to the principle of the *actio minima*. Two years later Maupertuis propounded his principle before the Berlin Academy, proclaimed it to be a universal law of nature and the first scientific proof of the existence of God. On this occasion, too, he did not prove the proposition nor determine the limits of its applicability, but only supported it by two examples, one of which alone was correct. This principle, propounded with such grand solemnity, but so weakly supported, was violently attacked by König of Leipzig, and just as keenly defended by Euler. This mathematician likewise failed to furnish the proof, which was not possible till after the investigations of Lagrange. The form in which the principle of the *actio minima* now existed was given to it by Hamilton, and the Hamiltonian principle for ponderable bodies was in complete harmony with the Lagrange propositions. The elder Neumann, Clausius, Maxwell, and the speaker had already extended the Hamiltonian principle to electro-dynamics. For this purpose, and in order to be able to subordinate to it all reversible processes, the speaker had undertaken some transformations of it, and had introduced into it the conception of the "kinetic potential." In the form it had thus attained the Hamiltonian law—the old principle of the *actio minima*—had in point of fact universal validity. It had just as wide an application as had the law of the conservation of energy, and revealed a whole series of mutual relations between the different physical processes. In his communication Prof. von Helmholtz gave only a quite general view of his investigations.

BOOKS AND PAMPHLETS RECEIVED

"Studies from the Biological Laboratories of the Owens College," vol. i. (J. E. Cornish, Manchester).—"Exterior Ballistics," by J. M. Ingolls (Van Nostrand).—"Essays relating to Indo-China," 2 vols. (Trübner).—"Fifth Annual Report of the United States Geological Survey," by J. W. Powell (Washington).—"The Chemistry of Wheat Flour and Bread," by W. Jago (Brighton).—"Annual Report of the University College and Free Library Committee of the Borough of Nottingham," 1885-86.—"Report on the Progress and Condition of the Botanic Garden and Government Plantations of South Australia, 1885," by R Schomburgk (E. Spiller, Adelaide).—"Longman's School Geography," by G. G. Chisholm (Longmans).—"Annuaire Geologique Universel et Guide du Geologique, 1886," vol. ii., by Dr. Dagincourt.—"Proceedings of the American Association held at Philadelphia," 2 parts (Salem).—"The Law of Storms," second edition, by W. H. Rosser (Norie and Wilson).—"Fourth Report of the U.S. Entomological Commission" (Cotton Worm and Boll Worm), by H. Riley (Washington).—"Progress of Astronomy, 1885," by W. C. Winlock (Washington).—"Bulletin de la Société Impériale des Naturalistes de Moscou," Nos. 3 and 4 (Moscou).—"Original Mittheilungen aus der Ethnologischen Abtheilung der Königlichen Mus. zu Berlin," Erster Jahrgang, Heft 1, 2, 3 (Spemann, Berlin).

CONTENTS

THURSDAY, AUGUST 5, 1886

THE BOOK OF DUCK DECOYS

The Book of Duck Decoys; their Construction, Management, and History. By Sir Ralph Payne-Gallwey, Bart. (London: John Van Voorst, 1886.)

IN the year 1812 appeared the first instalment of what its eccentric author intended to be a sketch of the local history of the Lincolnshire Fens; therein "Fen-Bill Hall" declares his set purpose of devoting a portion of the work to the "life of a low Fen-man," and of descanting largely upon the subject of decoys, adding that he had never seen but one rational writer on the subject, and that he (the said writer) manifested that he knew "nothing of the theory." Hall's book came to an untimely end with the third part, and the author therefore had not the opportunity of writing "rationally" upon a subject which would have proved so interesting in the present day and upon which so much irrational writing has been lavished. Failing "Fen-Bill Hall" so well did the decoymen keep their secret and so securely were the decoys guarded from intrusion that in all the numerous subsequent so-called descriptions, with one or two partial exceptions, the writers showed an utter want of acquaintance with both the theory and practice of decoying, and in the exceptional cases named such experience was of a very limited character, the deficiency being in all probability supplied by intentionally misleading information on the part of the guardian of the decoy. It was not till the year 1845, when the Rev. Richard Lubbock published his well-known "Fauna" of Norfolk, that the first really reliable account of the art of constructing and working a duck decoy, the result of actual experience acquired by the writer, was given to the public. Since that time various more or less accurate papers on the same subject have appeared, but it remained for Sir Ralph Payne-Gallwey (than whom no more competent authority could be found) to collect the literature of the subject, and with his own practical experiences added, to publish the first "Book of the Duck Decoy" in the form of the handsome volume now before us.

From very early times tunnel-nets appear to have been used for taking water-fowl, and there is no doubt much importance was attached to the privilege of using such engines, as appears from frequent litigation on the subject, dating back even as long ago as the reign of King John. These tunnel-nets were used for the purpose of securing young birds yet unable to fly, which with the old birds when moulting were *driven* into them. Sir Ralph Gallwey gives a curious woodcut of such an arrangement, as used in the sixteenth century, which represents in a somewhat diagrammatical way a phalanx of boats driving the birds before them into the nets fixed at the head of a bay, much as the Orkney fishermen drive a "school" of ca'ing whales on to the beach. This method of driving was found so destructive that it was prohibited by law, and was probably succeeded by a device called a cage-decoy, into which the ducks were enticed by feeding, and then secured by dropping a framework of netting which closed the entrance. Such a decoy is still worked at Hardwick Hall in Derbyshire, but it seems probable that decoys proper as used in the present day were not introduced into England till the commencement of the seventeenth century,—whether by Sir William Wodehouse as so often stated, on the authority of Sir Henry Spelman, Sir Ralph Gallwey appears to think doubtful. The decoy erected by Charles II. in St. James's Park in the year 1665 is doubtless the first arrangement on the Dutch plan for *alluring* ducks of which we have any exact account, and it is even possible that a curious old woodcut which occurs in a copy of "Æsop's Fables," dated 1665, and which Sir Ralph Gallwey reproduces at p. 9 of his book, may have been taken from this identical decoy; but should it, as seems more probable, have been sketched from a still earlier decoy, it would tend to prove that Sir William Wodehouse really had the honour of taking precedence of Charles II. in adopting the Dutch method of decoying.

For the successful working of a decoy a site must be chosen far from the busy haunts of men, secluded by a screen of trees, all the approaches to which should be under the control of its owners; at the commencement of the present century such spots were easy enough to find, and decoys abounded yielding large profits to those who worked them. Unfortunately very little is now known of the results of the working of these decoys, but we have it on the authority of Pennant that one celebrated group of ten decoys in Lincolnshire produced in a single year 31,200 ducks. There are also some published statistics of the Ashby decoy, showing that in thirty-five years 95,836 fowl were there taken; and a famous decoy in Essex produced in thirteen years 50,787 birds. Sir Ralph Gallwey estimates that 100 decoys which formerly existed in the Eastern Counties averaged 5000 ducks each yearly, or half a million of birds, and this without firing a shot, and adds, "during the last dozen years I have waged constant warfare against wildfowl, with all imaginable contrivances in the way of yachts, punts, and guns, in various parts of the world, as well as at home, at a cost of—I should be afraid to say how much time and money—yet I can account for but six or seven thousand ducks. Now, in *one* winter alone, it was in our grandfathers' days a usual thing for a decoyman to catch from five to ten thousand birds, at an annual outlay of perhaps 50*l.* spent in keeping up a pond and its netting, its pipes, and its reed screens." The price realised by the fowl at the commencement of the present century seems trivial enough as compared with that produced in the present day, but many decoys then made a return of from one to four hundred pounds per annum, perhaps even more, could the secrets of the decoymen be ascertained. In 1714 about 9*s.* 6*d.* per dozen birds seems to have been the price (it must be remembered that the smaller species of duck were counted as "half-birds," and went at twenty-four to the dozen); from thence the price appears gradually to have increased to 16*s.* per dozen in 1726. In 1765-66, 13,160 "whole-birds" (representing 18,000 fowl), captured at the Dowsley decoy, sold for 385*l.* 18*s.* 10*d.*

As might be expected of men exercising such an exceptional calling, the decoyman was clannish in the extreme; and Sir Ralph Gallwey gives some very interesting particulars of a family of typical decoymen named Skelton, who originally migrated from Firskney in Lincolnshire, where they had long followed the same occupation, into

Norfolk, in which county they remodelled most of the then existing decoys and constructed others. Descendants of the same family, having since removed to other counties, are some of them still celebrated for the skill with which they exercise the talent inherited from their forefathers. When "Old George Skelton" first came into Norfolk he found the decoy-pipes simply grouped around the margins of the "Broads," a plan which did not at all accord with his ideas of propriety, the great extent of water rendering it difficult if not impossible to have the fowl under what he considered proper control. He therefore selected a small piece of water of about two acres in extent on the banks of which to construct his decoy, much to the amusement of the local decoymen; but their derision was soon changed to amazement when in one week he captured 1100 teal in his "two-acre puddle" as they derisively termed it. The son of this man, also known as "Old George Skelton," was equally celebrated for his skill as a decoyman, and left his mark upon many of the Norfolk decoys. This man, says Sir Ralph Gallwey, is described as a "very peculiar man, short of stature, web-footed like a duck, very strongly built, particularly kind in disposition, perfectly indifferent to cold and hardship, well-informed, and unequalled in skill in the construction and management of decoys."

In the space at our disposal it would be impossible even to epitomise the full and elaborate instructions for erecting and working a decoy, given so clearly and precisely that the thirty-two plans and illustrations are scarcely necessary for their elucidation. But with such assistance there should be no difficulty in erecting the decoy, and by following the ample instructions experience would be gained in a season or two sufficient to enable almost any one to work the pipes with tolerable success; but the art of decoying is only to be acquired in perfection by careful and continued study of the habits of the frequenters of the decoy pond with practice added. We quite agree with Sir Ralph Gallwey that there cannot be a more interesting adjunct to an estate than a duck decoy, if even it be only worked on occasions to obtain a supply of fowl for the table of the proprietor and as acceptable presents to his friends: but should he be a naturalist and fond of the study of birds, a peep through the screen of his decoy at the fowl disporting themselves in a state of perfect unconsciousness under his very eye and almost within his grasp will go far towards repaying him the trifling outlay the decoy will entail. Nor need the fact of the decoy being worked preclude the proprietor from the occasional use of the gun: if not persistently disturbed the fowl will speedily return, and although it is undoubtedly to the advantage of the decoy to be perfectly secluded, a very successfully worked decoy is known to the writer in so exposed a situation that the fowl on the water may be seen from a public road which passes close by; it is astonishing how soon wildfowl become accustomed to sounds and sights which are not sudden or unexpected.

Sir Ralph Gallwey enumerates forty-four working decoys, and traces with more or less success the history of 149 others which have ceased to be used in England, and three active and nineteen disused decoys in Scotland; the sister island, so far as he can ascertain, never having possessed a decoy. Of this large number Lincolnshire possessed thirty-nine, only one of which is still worked; Essex thirty, three of which are still worked; Norfolk twenty-six, with five still worked; and Yorkshire fourteen, with two only still in use. The history of these decoys as given by Sir Ralph Gallwey will be found replete with antiquarian interest as well as with abundant matter for the consideration of the naturalist, and his chapter on the Lincolnshire Fens is especially interesting.

A short account is given of the decoys existing in Holland, from which country enormous numbers of fowl are exported annually, and which probably indicates the state of affairs which existed in this country in the palmy days of the duck decoy. A small woodcut on p. 200 shows a form of nesting basket used by the Dutch for their tame decoy ducks, and which would probably prove an excellent contrivance for inducing wild birds to nest in our own shrubberies and pleasure-grounds.

We cannot speak too highly of the plates and plans with which this handsome volume is illustrated, and we cordially recommend it to the perusal of all lovers of field-sports.

COMETARY AND PLANETARY ORBITS

Traité de la Détermination des Orbites des Comètes et des Planètes. Par le Chevalier Théodore d'Oppölzer, &c. Édition Française. Par Ernest Pasquier. (Paris: Gauthier-Villars, 1886.)

THIS is a translation from the second edition of the first volume of Prof. Oppölzer's laborious and truly classical work in German, on the theory and practical determination of the orbits of comets and planets. It has been made with the full assent and co-operation of the author, and with the assistance of Dr. Schram and others who greatly aided in the production of the original work. The volume comprises nearly 500 pages of text and 200 pages of tables, and is an excellent specimen of typography throughout. Oppölzer's first volume is divided into two parts, the first termed *preparatory*, the second treating of the determination of orbits in the various conic sections In the preparatory part we have chapters on the transformation of co-ordinates; on co-ordinates in their relation to the time and the relation between the position of the celestial body in its orbit and the corresponding epoch; likewise on the relation between a number of positions in the orbit. There is a chapter on aberration, and an important one on the theoretical determination of the formulæ of precession and nutation. The second part commences with the treatment of parabolic orbits, of which the numerous cometary discoveries of the present day necessitate so frequent application, and there are fully-worked numerical examples referring to the comets 1869 III. and 1881 III. This section is followed by a chapter, which will have much interest, on the determination of the orbit of a swarm of meteors by means of its radiant point, a problem which is reduced within a very small expenditure of time and calculation: a numerical example is worked out for one of Prof. Weiss's radiants. The next section treats of the calculation of the orbit where no assumption is made with respect to the excentricity: (1) from three observations only, as is more usually the case; (2) where four observations are introduced. The well-known general method of Gauss was published early in the present

century, and has been used in determining the orbits of a large number of the minor planets, and of the comets of short period. Oppölzer substitutes for it in his second edition one of his own, which, from extensive application he has found to be much superior to all other methods, both as regards the precision of the results and the rapidity with which the computations may be performed. In the case of the planet *Ceres* he obtained results on a first approximation more exact than those given by the method of Gauss after three approximations. Further, it is pointed out that, where four observations are employed, Gauss's method is not applicable, except when the excentricity is small. There is a chapter on the modifications of Oppölzer's method necessary in the determination of cometary orbits; also a numerical example for the orbit of the minor planet *Eudora*, and one for the first comet of 1866, or the comet of the November meteors, as well as a comparison of the new method with that of Gauss, by an example taken from the *Theoria motûs*. So far, three observations are employed. Similar examples follow for the case of four observations. A succeeding section deals with the calculation of circular orbits, and it is shown that an ephemeris deduced from a circular orbit, which admits of comparatively rapid and easy calculation, may be made of service in following for a time a newly-discovered minor planet. In an appendix are collected all the formulæ usually required in the first determinations of orbits, with reference to those parts of the volume where the analysis and other details are to be found—a *résumé* that possesses great value in so extensive a work. The tables which follow are on a greatly extended and refined system, more especially that for the calculation of the true anomaly in the parabola

The great work of Oppölzer, of which Prof. Pasquier has presented astronomers with so admirable a translation, is not one suited to a beginner; but the student with a certain knowledge of the differential and integral calculus, and of analytical mechanics, may initiate himself with its aid, as the translator remarks in his preface, "à l'un des problèmes les plus hardis que se soit posés l'intelligence humaine."

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Silver-Blue Cloudlets again

FROM your last week's issue, p. 264, it would seem that the silver-blue clouds and cloudlets seen at midnight low down over the northern horizon both in this, and last, year's July are attracting much attention among your correspondents; but have not yet had the spectroscope directed to them.

Now there was a remarkable display of those bright blue clouds on the night of Tuesday, July 27, though with some variations on their exact mode of appearance in the *earlier* part of the month; but not necessarily removing them into a different category. The day had been cold but clear, especially in the northern direction, from which the wind was blowing, bar. = 29·60, night temp. = 48° F., depression of wet-bulb = 4°·0 F It was therefore just such a night as at this season of the year and in this high latitude is certain to show a coloured twilight over the sun's place beneath the northern horizon, if ordinary thick fogs, and low cloud-banks do not interfere.

On issuing, then, that night, close upon twelve o'clock, from the Observatory computing-room, upon the Calton Hill, I was surprised and even startled, not at seeing a low-down coloured twilight in the north, but at the excessive strength, and glittering brightness of its colours. You might indeed have, at first sight, imagined that some great city, spread abroad over the plains of Fife was in a fierce state of extensive conflagration, so burning red was the first and lowest stratum extending along nearly 20° of the horizon. But that awful kind of redness passed quickly into lemon-yellow clouds in the stratum next above the red; and then came the silver-blue cloudlets just above the lemon-yellow, and even brighter still; but with an innocence of colour and gentleness of beauty, which at once exorcised the horrid idea of malignant flames devouring the works of man; and showed it must be something very different.

But still what was it, that made that low level strip far away in the north, just then so brilliant in its light and intense in its colours,—that it, and it alone seemed for the time, to be illumining the otherwise pitchy darkness of night? At the same time a few stars were faintly visible; while a long streamer, of apparently white cirrus cloud, trailed over half the sky from west, to east-north-east, and passed across the Polar region at a considerable altitude, having the silver-blue cloudlets and their gorgeous red basement far below, but within, its wide-inclosing sweep.

On reaching home, I got a large spectroscope to bear on the brightest part of the low level streak of richly coloured light, its red, and yellow, and light blue, both collectively, and separately; but with no other decided effect than a short continuous spectrum in the green; which, as I have elsewhere long ago shown, is the spectrum of ordinary twilight always. For even though red and yellow be present to the eye at large, these colours rapidly fade out in any slit-formed spectrum, leaving the maximum of faint twilight placed by the prism as above described.

On this occasion, however, I did remark that that short continuous spectrum began in its citron, or commencing, region rather abruptly: in fact I even imagined a bright line there; and after several independent measures of spectrum-place, duly tested by reference both to a hydrogen tube, and the micrometer readings,—made out, that it was in the very position of the aurora line; or that, in fact, aurora was at that moment assisting, though to a very small extent, in that low streak of merely, but yet so intensely coloured, solar and Scottish, midsummer-midnight, northern, twilight.

Going next to the window, with a hand spectroscope, and examining the long ribbon of supposed white cirrus at some immense elevation,—it was startling as well as delightful to find it to consist of hardly anything but aurora; and to see aurora's chief line thin, sharp and positively brilliant along its whole extent; even appearing, if that could be, several times brighter, than its parent white streamer itself looked to the naked eye.

Nor did the identification, as aurora, of this fair white arc (transverse to a line leading to the magnetic pole), depend on the spectroscope alone: for, about 1 a.m., it began to form luminous, and rather yellowish, abutments to both its western and eastern terminations. Then its original singleness of curvature began to mould itself in the north-west into several curves of shorter radius; and after that, many thin arrows and shafts of light began to shoot out at right-angles from some parts of the great arc, and towards the zenith; and then, after a few minutes, died away. In fact it was to the eye a very fair auroral display, though the papers next morning said nothing about it.

But luminous manifestations were by no means the whole of what the aurora was doing; for presently I could conceal from myself no longer that the whole space below that long and high vaulting, white, upper arc was darkened, as compared against the sky elsewhere, with a brown-black hue; which moreover darkened still further and deepened in obscuration as it descended, until it suddenly ended sharply above, and quite close to, the silver-blue cloudlets of the low coloured twilight on the northern horizon.

Here then was a key at once to the apparently supernatural brilliance of the silver-blue cloudlets and the other colours below them; viz., all the broad expanse of ordinary further,

or outer, twilight, extending in reality to far beyond and above the place of the said cloudlets, was on this occasion painted, or blocked, out by dark dun colour. Nearly half the heavens were so obscured, and the earth below was as dark. No wonder then that the residual strip of untouched twilight shone so conspicuously in contrast.

But what is that darkness below an auroral arc?

It has been compared to the dark space under the negative pole of a highly rarefied gas-tube, when an electric spark is passed through it. And if we add in idea that it extends downwards to a certain angular distance from the sun, say 20°, and keeps to that,—the suggestion may explain why the silver blue cloudlets were seen higher over the northern horizon in the end of June and beginning of July, than at the end of the latter month; and also why they are never seen in the winter. But a still greater instrumental curiosity manifested itself in this, that the bright auroral citron-coloured line was also given in the spectroscope out of every part of that large expanse of auroral *shade*; and almost, though not quite, as well as from the bright track along its outer and upper edge; just as if, however dark to the unassisted eye, the black-brown space was yet somewhat luminiferous to the peculiar power of the prism.

On the next night after this interesting midnight experience, there was no aurora, and the twilight extended faintly to many degrees higher than the position of the blue clouds of the previous night, and in fact spread into and over the region which was before so decidedly "aurora blackened."

But the next night after that again, viz. two nights after the display, there was a wet drizzling mist which continued through the early hours until more than a quarter of an inch of rainfall had been gathered. Admirably confirming therefore the late Sir Robert Christison's often strongly expressed opinion that 48 hours after a great aurora, abundant rain is sure to follow,—an opinion too which I have only just heard was formed quite independently in Canada by my friend Mr. R. S. Haliburton, who is even now introducing it into his theory of "the aqueous origin of the aurora," so far as that can be carried; but without explaining either the citron line in the spectrum; or the effect on the magnetic needle. C. PIAZZI SMYTH

15, Royal Terrace, Edinburgh, July 31

The Bright Clouds

THE bright cirrus-like clouds are very common here this summer. I have seen them here on the nights of the 12th, 18th, 20th, and 24th of last month, and on the 1st inst., also at Gilsland on the 27th ult., in fact I do not know that in the last fortnight there has been a single night on which the northern sky has been quite free from lower clouds on which they have not appeared more or less; sometimes, however, they appear but for a short time, and in a very limited area of the sky. I have tried to keep a watch to see them in the day-time, but have not succeeded as yet; the nearest approach to success was on the 20th ult., when I saw them as early as 9.22 p.m., at which time they were visible over the greater part of the sky, but in the south-east were not strikingly bright.

There is one peculiarity with respect to them that I have not seen mentioned in NATURE, and that is their motion; on the above dates, except the 20th, I took notice of this, and in every instance the motion was from a northerly or easterly direction, whereas I have not noticed any ordinary cirrus moving from that quarter lately. Last night at from 10 to 10.15, when there were small patches of these curious clouds, there was also at the same time a great deal of ordinary cirrus moving from the west. This circumstance appears to indicate that there is quite a different current of wind in the upper atmosphere from that blowing at the lower elevation of ordinary cirrus.

I have no hesitation in saying that these extraordinary clouds do not shine with their own light, but with the direct light of the sun. T. W. BACKHOUSE

Sunderland, August 2

Aurora

THE following is a record of aurora observed on July 27 at Ramelton, co. Donegal:—

9.30 p.m.—From west to east there were occasional pencils of reddish lights shooting up, while from east to west there were continuous pencils of yellowish and reddish lights, with intermittent clouds and columns of reddish light, rising between the north-east and north-north-west. The pencils were very steady, but increasing and decreasing in length, at times assisted by the clouds of red forming a corona at the zenith round the star Capella (?), at such times as pencils shot up from the southward, on a rude irregular cross. The corona and cross appeared and disappeared quite rapidly. The clouds and columns of reddish light were succeeded by flashes and pencils of bright silver light, they being most frequent and brilliant between the north and west, the flashes being sometimes in long narrow wavy clouds that rapidly ascended, or narrow sheets that appeared and disappeared nearly instantaneously. They became more and more brilliant, especially to the north-north-west, till the display was greatest between 10.30 and 10.45.

10.45 p.m.—About this hour the continuous pencils of yellow and reddish lights between the east and west disappeared with the other lights, but about five minutes afterwards, to the northward, silver pencils and sheets appeared, veering from thence gradually towards the north-east and east. Some of the sheets hung at times in clouds that formed small arcs, that slowly rose obliquely, and moving eastward till about 11 o'clock, when all the lights disappeared, except that at long intervals faint pencils or flashes might shoot up on a small arc of silver clouds; but at 11.25 there was another brilliant display. First there appeared an arch of silver light, its centre being about north-north-west, then two arches that began sending up horns and pencils of light. The upper arch was a little below the North Star, while the lower one went through the Pointers of the Plough; these two arches were succeeded by one at 11.35. The crown of the arch was very unsteady, moving from north to north-north-west and back again, its shape and the accompanying horns and pencils continually changing, the most brilliant and highest pencils being those that shot up to the north-westward. This display continued more or less brilliant till 11.45, when clouds came up and prevented further observations; but at midnight the position of an arch was distinctly defined behind the clouds. At 1 a.m. the clouds had cleared away and no lights were visible. The night was not favourable for seeing an aurora, as it was very light and clear, yet at times the lights were very brilliant. Although the arches were of the same class of silver light as those seen from the North Atlantic or the Canadian Lakes, yet they were not steady like those, as they were continually shifting their positions or disappearing and reappearing. During the previous day there was a northerly wind with at times intensely cold squalls of misty rain.

G. H. KINAHAN

ABOUT 11.15 p.m. July 27, 1886, I observed an incipient stage of the aurora borealis, and about 0.10 a.m. of the 28th a considerable display of auroral colour commenced; but between 0.30 a.m. and 1 there was a vivid display of huge auroral sheets and columns; indeed, it did not require much to entitle the golden scene to the epithet—magnificent. There was a prismatic arc, not unlike a rainbow, which spanned from the north-west to the east, and measured about 70° from its centre to the horizon. All under this arc was a flood of white light, which the aurora did not in the least degree invade. From this arc developed a brilliant aurora borealis to a few degrees south of the zenith; and, with other constellations, Cygnus, Lyra, and the Northern Crown were overwhelmed in a golden flood. In this part of our northern latitude there are at present highly favourable conditions for phenomenal refraction of solar light. On the morning of the 27th and 28th the earth-shine on the moon was very bright, and I have no doubt but that these atmospheric conditions are also favourable to auroral displays. Every vestige of the aurora borealis disappeared about 2.30.

I do not think that an aurora as early as July is on record; in the Culloden meteorological records there is not one recorded so early as July from 1841 to 1880: indeed August is reckoned unusually early for an aurora borealis. I recollect a most magnificent one about the beginning of August in 1882, in the upper reaches of Lanarkshire; the huge vivid sheets and columns reached from the west all along the horizon to the east, and up the vault of heaven to the zenith, and with their fleet shifting flashes and bursts of prismatic coruscations, they lighted up the earth with ineffable glory.

DONALD CAMERON

The Academy, 22, Argyll Street, Paisley, July 28

Halos and Mock Suns

On Tuesday, July 20, about 5.15 p.m., I saw from this neighbourhood a most remarkable series of halos and parhelia, the general appearance of which is represented in the accompanying figure. (The parhelia at 120°, P^{iii} and P^{iv}, cannot be represented in the figure.)

As I happened to have a theodolite near at hand, I measured the altitudes and azimuths of the parhelia and contact arches, and also of two points on the larger halo, with the following result :—

	Azimuth by back angle	Altitude.	
Sun	320°	25°	
Parhelia	°	°	Angular distance from sun
P^{i}	347	25	27
P^{ii}	293	25	27
P^{iii}	80	25	120
P^{iv}	200	25	120
Contact-arch to inner halo	°		Radius of large halo from mean of two observations
Left extremity (A^{ii})	285	39	63° 30′
Right extremity (A^{i})	353	39	

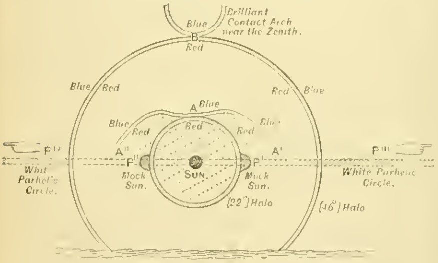
The positions of the two parhelia P^{iii} P^{iv} (or more properly speaking, *anthelia*) at 120° on either side of the sun, exactly accord with what is given in the text-books, but the solar longitude of the parhelia on the primary halo 27°, and the dimensions of the larger halo $63\frac{1}{2}°$, usually given as 46°, are greater than those usually recorded.

The following features were observed :—

(1) The parhelion, P^{ii} 27°, to the left of the sun, was very brightly visible before that on the right appeared at all.

(2) The parhelic circle appeared to encircle the entire sky, and to be everywhere of the same altitude—25°—as that of the sun.

(3) The contact arch, B, at the top of the larger halo, was remarkably brilliant, being red on the side adjacent to the sun,



and blue on that furthest from it, and appeared to be almost exactly at the zenith, thus supporting the somewhat rough measurements of the outer halo, which made it considerably larger than the traditional 46°.[1]

(4) The contact arches A^{i} A^{ii} were also very brilliant, and the space within them as well as that within the inner halo *h h* was much darker than that outside.

(5) The colours of the outer halo, H H, were similar to those of the inner halo, *h h*, viz. red inside and blue outside, but fainter.

(6) The parhelia attached to the inner circle P^{i} P^{ii} were similarly red inside and blue outside, while those at 120° were perfectly white.

The whole phenomenon lasted about twenty minutes, and was one of the most beautiful sights I ever saw. I was experimenting with a captive balloon at the time, or should have been able to make more detailed observations. I hear that on Monday night a deluge of rain of a tropical character fell at Dieppe. The cloud which caused these unusual optical phenomena appeared to be of the type termed by Poey globo-cirrus. I shall be glad to hear if any corroborative measurements were made by other observers.
E. Douglas Archibald

Tunbridge Wells, July

P.S. No parhelia were visible at the junction of the larger halo H H with the parhelic circle. Also there were no signs of the rare 90° radius halo. The radius of the inner halo was not measured, but as the lateral deviation of the parhelia P^{i} P^{ii} from the points in which it intersected the parhelic circle for a solar altitude of 25° should be about 2° 7′, this would make the radius of the inner halo 24° 53′ instead of 22° 30′ as is generally the case.

[1] Sun's altitude = 25°, radius of halo = $63\frac{1}{2}°$, which would make the lower extremity of the top contact-arch $88\frac{1}{2}°$ above horizon.

On Tuesday afternoon, 20th inst., while sketching near Cranbrook, in the Weald of Kent, I saw a magnificent example of mock suns and solar rainbow circles.

From an early part of the day the sky had been, I think, more splendid in its cloud arrangements of cirro-cumulus than I have ever seen in this or any other country, though I have always been a delighted student of these phenomena. From 10 a.m. to about 4 p.m. there was an incessant change of loveliness in the forms and positions of the clouds and the remarkable perspectives thereby produced, to the intense admiration of myself and wife. But about 4 o'clock one half of the heavens from the horizon to the zenith became nearly covered with a thin stratum of dark clouds, which resembled more than anything else innumerable long bundles of cotton fibre, placed in every possible direction. The other half of the sky was of the richest and most delicate ultramarine as a background, and the fleecy *mare's tail* and *flocks of sheep* cloudlets as the subjects. On the dark strata of clouds the mock suns made their appearance, the real sun shining through the clouds with great intensity.

The whole phenomenon did not fade out till nearly 6 o'clock. As I saw it for some time reflected in a large sheet of water, I had good opportunities of studying it.

The setting of the sun that night was the most gorgeous pageant—myriads of golden streamers, in groups, being sent up from purple and scarlet clouds. Robert H. F. Rippon

Jasper Road, Upper Norwood, July 28

N.B. The clouds in the vicinity of the sun were slightly opalescent.—R. H. F. R.

A Singular Case

On March 2 last a small fishing-boat engaged in trawling at about 20 kilometres from the coast, off Monte Argentaro (Tuscan Maremma), captured a specimen of the Mediterranean Red Mullet (*Mullus barbatus*) tightly incased in a large colony of *Pyrosoma atlanticum*. The head of the fish had reached the bottom of the social cylinder, which fitted it to a nicety. The *Pyrosoma* measures 0·112 millimetre in length and 0·032 millimetre at its greatest transverse diameter; the mullet is 0·152 millimetre long, so that only 0·040 millimetre of its tail projects beyond the tightly fitting *Pyrosoma!* The fish was taken *alive*, but how it could have lived in such conditions or how it got into its tight jacket is to me most enigmatical. Even admitting a certain amount of elasticity in the tight-fitting tube in which its head, body, and fins are incased, its movements could only have been very limited, and a very incomplete respiration and perhaps nutrition might have come to it through the orifices of the zooids.

Young fish, especially Scomberoids, are often found under the shelter of Medusæ and *Physalia*—the case of *Fierasfer* getting into the visceral cavity of *Holothuria* is well known; but it is the first time I have seen or heard of so singular a case of imprisonment as the one related above, and I therefore thought it worthy of the attention of the readers of Nature. The specimen is preserved in alcohol in the rich ichthyological series of the collection of Italian Vertebrata in the Florence Royal Zoological Museum. Henry H. Giglioli

Florence, July 29

The Weather at Caracas

The following notes on the weather at Caracas during the remarkable storm from May 11 to 15 may not be void of interest :—

We had a rather low barometer on May 8 (10 a.m., 682·93 mm.; 4 p.m., 681·99 mm.), but then it rose gradually till May 18 (685·42 and 683·17 mm. respectively). There had been no rain in the first twelve days of the month, but from

May 13 to 16 there fell 106 mm. rain, about *one-ninth* of our total yearly quantity; on the 13th, 26 mm.; 14th, 27·6; 15th, 22·4; 16th, 30. These heavy rains were undoubtedly due to the northern storm, although they came two days later.

Caracas, June 29 A. ERNST

The Indivisibility of Certain Whole Numbers

ANOTHER exception has been found to Fermat's assertion regarding the indivisibility of whole numbers of the form $2^{2^m}+1$ (see several notices in NATURE, vols. xviii. and xix.). The matter now stands as follows:—

$2^{2^5}+1$	divisible by	$5\cdot2^7+1$	(Euler)
$2^{2^6}+1$	,,	$1071\cdot2^8+1$	(Landry)
$2^{2^{12}}+1$	,,	$7\cdot2^{14}+1$	(Pervouchine)
$2^{2^{23}}+1$	,,	$5\cdot2^{25}+1$	(Pervouchine)
$2^{2^{36}}+1$	,,	$5\cdot2^{39}+1$	(Seelhoff).

M.

A Quadruped Duck

IT may interest some readers of NATURE to hear that there is at present living in Bardsea a duck which has four feet. The two abnormal feet, which are webbed like the others, and of the same shape and size, spring from one leg, which is about the same length as the normal legs, but rather thicker. This leg grows from a point just beneath the tail. Its bone does not seem to be directly connected with the other bones of the bird, as it can be freely moved in any direction. This duck is more than a month old, and is healthy. EDWARD GEOGHEGAN

Bardsea, August 3

PHYSIOLOGICAL SELECTION: AN ADDITIONAL SUGGESTION ON THE ORIGIN OF SPECIES[1]

I.

THERE are three cardinal difficulties in the way of natural selection, considered as a theory of the origin of species.

(1) The difference between species and varieties in respect of mutual fertility. Many of our domesticated varieties differ from one another to an extent greater than that which distinguishes many natural species: yet they continue perfectly fertile *inter se*, while the natural species are nearly always more or less sterile. The difficulty is not met by pointing to the fact that sterility between natural species is neither absolutely constant nor constantly absolute; for the question still remains, Why are the modifications of organic types supposed to have been produced by natural selection, so generally attended with some more or less pronounced degree of mutual sterility, when even greater modifications of such types produced by artificial selection so generally continue mutually fertile? That this question does not admit of any answer by the theory of natural selection Mr. Darwin himself acknowledges, and therefore suggests a wholly independent hypothesis by which to explain the fact. This hypothesis is, that varieties occurring under nature "will have been exposed during long periods of time to more uniform conditions than have domesticated varieties, and this may well make a wide difference in the result." Now, whatever we may think of this hypothesis, it is certainly quite distinct from the theory of natural selection; and, therefore, any one who adopts the supplementary hypothesis is, so far, confessing the inadequacy of that theory, considered as a theory of the origin of species. For my own part, I deem the hypothesis wholly insufficient to meet the facts. When we remember the incalculable number of species, living and extinct, we immediately feel the necessity for some much more general explanation of their existence than is furnished by supposing that their mutual sterility, which constitutes their most general or constant distinction as species, was in every case due to some incidental effect produced on the generative system by uniform conditions of life. To say nothing of the antecedent improbability that in all these millions and millions of cases the reproductive system should happen to have been affected in this peculiar way by the merely negative condition of uniformity, there remains what seems to me the overwhelming consideration that, at the time when a variety is first forming, the condition of prolonged exposure must necessarily be absent as regards that variety: yet this is just the time when we must suppose that the infertility with its parent form arose. Because, if not, the incipient variety would have been reabsorbed into its parent form by intercrossing.

(2) For the swamping effects of free intercrossing upon an individual variation constitutes the next, and perhaps the most formidable, difficulty with which the theory of natural selection is beset. The only answer which Mr. Darwin has to make in this case is that a number of individuals inhabiting the same area may vary in the same way at the same time. Of course, if this assumption were granted, there would be an end of the present difficulty; for if a sufficient number of individuals were thus similarly and simultaneously modified, there need no longer be any danger of the variety being swamped by intercrossing. But the force of the difficulty consists in the very fact of this assumption being required to meet it. The theory of natural selection trusts to the chapter of accidents in the matter of variation; and in this chapter we read of no reasons why the same beneficial variation should arise in a number of individuals simultaneously. Moreover, if it does so, the fact of its doing so cannot be attributed to natural selection, which thus again fails as a theory of the origin of species. Lastly, as will immediately be shown, a very large proportion, if not the majority, of features which serve to distinguish species from species, are features presenting no utilitarian significance; and, therefore, even if it be conceded that they each arose in a number of individuals simultaneously, their reabsorption by intercrossing could not have been in any degree hindered by natural selection.

(3) The difficulty just alluded to of the inutility to species of so large a proportion of specific distinctions, is one which Mr. Darwin frankly acknowledges in the later editions of his works. In other words, he allows that a large proportion of these distinctions resemble the more general distinction of sterility in not admitting of any explanation by the theory of natural selection. They consist of small and trivial differences of form and colour, or of meaningless details of structure, which, being of no service to the plants or animals presenting them, cannot have arisen through the agency of natural selection. If it be suggested that all such distinctions are of disguised utility, the answer is that to offer this suggestion is to reason in a circle. For the only evidence we have of natural selection as an operating cause in any case is derived from the utility of the observed results: therefore, in cases where utility is apparently absent, we may not assume that it must be present only because, if it were not present, the results must be due to some cause other than natural selection. Observe, the case would be different if the great majority of specific distinctions—like the great majority of higher distinctions—were of obvious utilitarian significance; for in this case we might reasonably set down the exceptions as proof of the rule, or hold that they appear to be exceptions only on account of our ignorance. But it is certainly too large a demand on our faith in natural selection to appeal to the argument from ignorance when the facts require that the appeal should be made over so very large a proportion of instances. But it is needless further to insist upon this

[1] Abstract of a Paper read before the Linnean Society on May 6, by George J. Romanes, M.A., LL.D., F.R.S. &c.

point, since, as I have already observed, its force has been fully recognised by Mr. Darwin and his followers. Here again, therefore, the theory of natural selection fails as a theory of the origin of species.[1]

In view of these three grave disabilities under which the theory of natural selection lies, I feel entitled to affirm that the theory has been misnamed. Natural selection is not, properly speaking, a theory of the origin of *species*: it is a theory of the origin—or rather of the cumulative development—of *adaptations*, whether these be morphological, physiological, or psychological; and whether they occur in species only, or likewise in genera, families, orders, or classes. These two things are very far from being the same; for, on the one hand, in an enormously preponderating number of instances, adaptive structures are common to numerous species, while, on the other hand, the features which serve to distinguish species from species are, as we have just seen, by no means invariably—or even generally—of any adaptive character. If once it is thus clearly perceived that the theory of natural selection is not a theory of the origin of species, but a theory of the development of adaptive structures—whether these happen to be distinctive of species or of higher taxonomical divisions—if once this is clearly perceived, the theory is released from all the difficulties which we have been considering. For these difficulties have beset the theory only because it has been made to pose as a theory of the origin of species, whereas in point of fact it is nothing of the kind. In so far as natural selection has had anything to do with the genesis of species, its operation has been, so to speak, incidental: it has only helped in the work of originating species in so far as some among the adaptive variations which it has preserved happen to have constituted differences of merely specific value. Many other such differences there are with which natural selection has had nothing to do—particularly the most universal of all such differences, or that of mutual sterility—while, on the other hand, by far the larger number of adaptations which have been the work of natural selection are now the common property of genera, families, orders, or classes. Let it, therefore, be clearly understood that it is the office of natural selection to evolve adaptations: not necessarily, or even generally, to originate species.

Let it also be clearly understood that in thus seeking to place the theory of natural selection on its true logical footing, I am in no wise detracting from the importance of that theory. On the contrary, I am but seeking to release the theory from the difficulties with which it has been hitherto illegitimately surrounded.

Enough has now been said to justify the view that there must be some cause or causes other than natural selection operating in the evolution of species. And this is no more than Mr. Darwin himself has expressly and repeatedly stated to have been his own view of the matter; nor am I aware that any of his followers have thought otherwise. Hitherto the only additional causes of any importance that have been assigned are use and disuse, sexual selection, correlated variability, and yet another principle which I believe to have been of much more importance than any of these. Yet it has attracted so little attention as scarcely ever to be noticed by writers on evolution, and never even to have received a name. For the sake of convenience, therefore, I will call this principle the Prevention of Intercrossing with Parent Forms, or the Evolution of Species by Independent Variation.

First let us consider how enormous must be the number of variations presented by every generation of every species. According to the Darwinian theory it is for the most part only those variations which happen to have been useful that have been preserved: yet, even as thus limited, the principle of variability is held able to furnish sufficient material out of which to construct the whole adaptive morphology of nature. How immense, therefore, must be the number of unuseful variations! Yet these are all for the most part still-born, or allowed to die out immediately by intercrossing. Should such intercrossing be prevented, however, there is no reason why unuseful variations should not be perpetuated by heredity quite as well as useful ones when under the nursing influence of natural selection—as, indeed, we see to be the case in our domesticated productions. Consequently, if from any reason a section of a species is prevented from intercrossing with the rest of its species, we might expect that new varieties (for the most part of a trivial and unuseful kind) should arise within that section, and that in time these varieties should pass into new species. And this is exactly what we do find. Oceanic islands, for example, are well known to be extraordinarily rich in peculiar species; and this can best be explained by considering that a complete separation of the fauna and flora of such an island permits them to develop independent histories of their own, without interference by intercrossing with their originally parent forms. We see the same principle exemplified by the influence of geographical barriers of any kind, and also by the consequences of migration. When a species begins to disperse in different directions from its original home, those members of it which constitute the vanguard of each advancing army are much more likely to perpetuate any individual variations that may arise among them than are the members which still occupy the original home. For not only is the population much less dense on the outskirts of the area occupied by the advanced guard; but beyond these outskirts there lies a wholly unoccupied territory, upon which the new variety may gain a footing during the progress of its further migration. Thus, instead of being met on all sides by the swamping effects of intercrossing with its parent form, the new variety is now free to perpetuate itself with comparatively little risk of any such immediate extinction. And, in the result, wherever we meet with a chain of nearly allied specific forms so distributed as to be suggestive of migration with continuous modification, the points of specific difference are trivial or non-utilitarian in character. Clearly this general fact is in itself enough to prove that, given an absence of overwhelming intercrossing, independent variability may be trusted to evolve new species. The evidence which I have collected, and am collecting, of the general fact in question, must be left to constitute the subject of a future paper.[1]

Were it not for the very general occurrence of some degree of sterility between even closely allied species, and were it not also for the fact that closely allied species are not always separated from one another by geographical barriers, one might reasonably be disposed to attribute all cases of species-formation by independent variability to the prevention of intercrossing by geographical barriers, or by migration. But it is evident that these two facts can no more be explained by the influence of geographical barriers or by migration than they can by the influence of natural selection. The object of the present paper is to suggest an additional factor in the formation of specific types by independent variability, and one which appears to me fully competent to explain both the general facts just mentioned.

[1] Of the three cardinal objections to the theory of natural selection thus briefly stated, Mr. Darwin himself appears to have attributed most importance to the first, seeing that its consideration occupies so large a portion of his writings. The objection from intercrossing, on the other hand (which was first rendered with much force and clearness by the late Prof. Fleeming Jenkin of Edinburgh, in an anonymous article, *North British Review*, 1867), is the only difficulty in the way of his theory which Mr. Darwin can fairly be said not to have sufficiently treated. The objection from inutility was first prominently raised by Bronn. It was afterwards developed by Nägeli, Broca, Mivart, and many other writers.

[1] So far as I am aware, the first writer who insisted on the importance of the prevention of intercrossing in the evolution of species, both by isolation and migration, was Moritz Wagner. Since then Wallace, Weismann, and others have recognised this factor. The most recent contribution to the subject is an admirable collection of facts published by Mr. Charles Dixon in a work entitled, "Evolution without Natural Selection," which was recently reviewed in these columns But I cannot find that any of these writers allude to the principle which it is the object of the present paper to enunciate, and which is explained in the succeeding paragraphs.

Of all parts of those variable objects which we call organisms, the most variable is the reproductive system; and the variations may be either in the direction of increased or of diminished fertility. Having, regard, therefore, to all the delicate, complex, and for the most part hidden conditions which determine this double kind of variation within the limits of the reproductive system, there can be no difficulty in granting that variations in the way of greater or less sterility must frequently occur both in plants and animals in a state of nature. Probably, indeed, if we had the means of observing this point, we should find that there is no one variation more common. But, of course, whenever it arises—whether as a result of changed conditions of life, or, as we say, spontaneously—it immediately becomes extinguished, seeing that the individuals which it affects are less able (if able at all) to propagate the variation. But now, if the variation should be such that, while showing some degree of sterility with the parent form, it continues to be perfectly fertile within the limits of the varietal form, in this case the variation would neither be swamped by intercrossing, nor would it die out on account of sterility. On the contrary, this particular variation would be perpetuated with more certainty than any other variation, whether useful or unuseful. An illustration will serve to render this more clear.

Suppose the variation in the reproductive system is such that the season of flowering or of pairing becomes either advanced or retarded. Whether this variation be, as we say, spontaneous, or due to any change of food, climate, habitat, &c., does not signify. The only point we need here attend to is that some individuals, living on the same geographical area as the rest of their species, have varied in their reproductive systems so that they are perfectly fertile *inter se*, while absolutely sterile with all other members of their species. By inheritance there would thus arise a variety living on the same geographical area as its parent form, and yet prevented from intercrossing with that form by a barrier quite as effectual as a thousand miles of ocean: the only difference is that the barrier, instead of being geographical, is physiological

From this illustration I hope it will be obvious that wherever any variation in the highly variable reproductive system occurs, tending to sterility with the parent form without impairing fertility with the varietal form—no matter whether this be due, as here supposed, to a slight change in the season of reproductive activity, or to any other cause—there the physiological barrier in question must interpose, with the result of dividing the species into two parts. And it will be further evident that when such a division is effected, the same conditions are furnished to the origination of new species as are furnished to any part of a species when separated from the rest by geographical barriers or by migration. For now the two sections of the species, even though they be living on the same area, are free to develop distinct histories without mutual intercrossing, or, as I have phrased it, by independent variation.

To state this suggestion in another form. It enables us to regard many, if not most, natural species as the records of variation in the reproductive systems of ancestors. When accidental variations of a non-useful kind occur in any of the other systems or parts of organisms, they are, as a rule, immediately extinguished by intercrossing. But whenever they happen to arise in the reproductive system in the way here suggested, they must inevitably tend to be preserved as new natural varieties, or incipient species. At first the difference would only be in respect of the reproductive system; but eventually, on account of independent variation, other differences would supervene, and the new variety would take rank as a true species.

The principle thus briefly sketched in some respects resembles, and in other respects differs from, the principle of natural selection, or survival of the fittest, as I will show later on. For the sake of convenience, therefore, and in order to preserve analogies with already existing terms, I will call this principle Physiological Selection, or Segregation of the Fit.

Before proceeding to state the evidence of the particular kind of variation on which this principle depends, let it be noted that we are not concerned either with its causes or its degrees. Not with its causes, because in this respect the theory of physiological selection is in just the same position as that of natural selection: it is enough for both that the needful variations are provided, without its being incumbent on either to explain the causes which underlie them. Neither are we concerned with the degrees of sterility which the variation in question may in any particular case supply. For whether the degree of sterility with the parent form be originally great or small, the result of it will in the long run be the same: the only difference will be that in the latter case a greater number of generations would be required in order to separate the varietal from the parent form.

(*To be continued.*)

TROPICAL FRUITS [1]

THE present Colonial and Indian Exhibition has developed interest in tropical fruits to an extent not previously known in England; and whatever may be the individual merits of many of the fruits displayed in the colonial market attached to the Exhibition, no one can deny that they afford proof of numerous undeveloped resources of our colonial possessions in a direction hitherto very much undervalued or entirely overlooked. Sir Joseph Hooker, in one of his journals, has remarked that "most tropical fruits are edible, but few are worth eating." But, after all, the merits of many fruits like those of certain wines are only properly appreciated under a concourse of local circumstances which materially affect our verdict. In the tropics the desire for refreshment and for something cooling and piquant is met by a fruit which, at the moment, completely answers the purpose. Transferred to a cooler climate, the same fruit may be entirely out of place, and indeed may be condemned as valueless. As a case in point, the water of a young cocoa-nut, when clear and fresh, taken from the fruit after a long ride in the sun, is most refreshing and wholesome. The same thing tried in the climate of England, and with fruit imported from the tropics, would be nauseous and wholly unpalatable. Similar remarks would apply to the fruit of the mangosteen, the durian, and many others where it is necessary that the fruit should be eaten when just ripe, and where a long journey affects the quality and impairs the delicacy—both being of an evanescent character.

Again, it is necessary to bear in mind how to use certain tropical fruits in order to appreciate them to the best advantage. Owing to the loose manner in which tropical fruits are termed, apples, plums, pears, peaches, &c., when they are neither botanically nor intrinsically anything of the sort, there has arisen considerable confusion respecting them. Again, many tropical fruits are suitable only for salads or curries, and should not appear at the dessert table at all. Others are better when preserved or cooked, and they are then both wholesome and well adapted to the wants of the country. There is no good pear (*Pyrus communis*, L.), as known in England, grown in the tropics, yet we have the name applied to the Alligator or Avocado pear (*Persea gratissima*, Gart), the anchovy pear (*Grias cauliflora*, L.), the prickly pear (*Opuntia ficus-indica*, Webb), and the wooden pear of Australia (*Xylomelum pyriforme*, Knight). Again, the English apple, although grown in the hills in the tropics, is practically of little value, but the name is as loosely applied as in the case of the pear, and hence fruits as widely apart as the poles in their botanical classi-

[1] Lecture given in the Conference Hall, Colonial Exhibition. Revised by the Author.

fication are grouped together under the general term of apple. To select a few out of many such names, we have the sugar or custard apple (*Anona reticulata*, L.), the Kei apple (*Aberia caffra*, H. and S.), the Mammee apple (*Mammea americana*, L.), the star apple (*Chrysophyllum Cainito*, L.), the rose apple (*Eugenia Jambos*, L.), and the golden apple (*Spondias lutea*, L.). In plums there are the Caffre plum (*Harpephyllum caffrum*, Bernh.), the coco plum (*Chrysobalanus icaco*, L.), the hog plum (*Spondias mangifera*, Pers.), the Chinese date plum (*Diospyros kaki*, Lin. fil.), the blood plum (*Hæmatostaphis Barteri*, H. K.), and the gray plum of Sierra Leone (*Parinarium excelsum*, Sabine); and so with the gooseberry, essentially a cold temperate fruit, English colonists have applied the name to such widely-diverging fruits as those of *Physalis Peruviana*, L. (Cape gooseberry), *Peirescia aculeata*, Mill (Barbados gooseberry), and *Cicca disticha*, L. (Otaheite gooseberry).

The so-called Nuts of the Tropics are in a worse state of confusion as regards the common names, and hence it by no means follows that what are imported as nuts belong at all to that category of fruits. The most familiar of tropical nuts is the cocoa-nut (*Cocos nucifera*, L.), a true nut; but the Para or Brazil nut (*Bertholetia excelsa*, H. B.) is simply the seed, not nut, which is inclosed in a large globular fruit, almost as large as a 36-pound cannon-ball; exactly the same occurs in the case of the Sapucajo nut (*Lecythis Zabucajo*, Aub.) as well as in the best of these so-called tropical nuts, viz. the souari or butter-nut of British Guiana (*Caryocar nuciferum*, L.). The cashew nut (*Anacardium occidentale*, L.) is a fruit borne upon a swollen pear-like peduncle, and presents one of the most remarkable instances of growth met in the tropics. The fruit when roasted is esteemed at dessert, and passes in India under the name of "promotion nut." The Jamaica walnut (*Aleurites moluccana*, Willd) is the seed of an Euphorbiad, as also the cob-nut (*Omphalea triandra*, L.).

From the above remarks it will be noticed from what various sources, and from what a diversified range of plants tropical fruits are derived. Also that little dependence can be placed on the common English names applied to these fruits. Where obtainable little objection can, however, be made against adopting the native or aboriginal names of tropical fruits, as in many cases they are sufficiently distinct, and at the same time are associated with circumstances of a local character, which render the name of permanent value. On this account a native or aboriginal name is quite as good, locally, as the scientific name; but of course it loses its value outside its own country. We have incorporated the aboriginal name in the case of the pine-apple (*Ananas sativa*, L.), and many instances of a similar character are found in the nomenclature of Indian and Chinese fruits.

Although not strictly speaking a tropical fruit, the orange is found in both the eastern and western tropics, and it is the best known of any foreign fruit. Large increase has occurred in the importation of this fruit to England within the last few years, and the present consumption is at the rate of $4\frac{1}{2}$ million bushels per annum, or equal to a consumption of sixteen oranges per head of population. Of fruits belonging to the same natural order as the orange there are the lemon (*Citrus medica*, var. *Limonum*, Brand), not largely grown in the tropics; the shaddock, or pumelo (*Citrus decumana*, L.), a fruit often 24 inches in circumference; the citron (*Citrus medica*, Riss.), chiefly used for "candied peel;" and the lime (*Citrus medica*, var. *acida*, Brand), which is chiefly known as the West India lime, and largely grown for making lime juice, raw and concentrated, for the manufacture of citric acid. This West Indian lime should take the entire place of the lemon in the English market, and when used in cooking gives a most piquant and refreshing flavour, not obtainable from any other source.

The banana generally found in the English market is the dwarf Chinese fruit (*Musa Cavendishii*, Paxt.). This is smaller and not so good as the true banana (*Musa sapientum*, L.), of which there are numerous varieties. The banana chiefly grown for export purposes in the West Indies is called the Martinique banana, a large yellow fruit about 6 to 8 inches long. The Cuban banana is a red-skinned variety, much coarser than the Martinique fruit, and only eaten when very "full," or in the ripe state of an English medlar. A variety of the banana with short thick fruits of very choice quality is called the fig banana, and this is esteemed as the best of the family, and consumed locally. The plantain (*Musa paradisiaca*, L.), is practically the tropical potato, and is used roasted, boiled, or fried, exactly as we use potatoes in England. At the present time the consumption of bananas in the United Kingdom is comparatively small. Some of the steamers trading with the West Indies are, however, being fitted with refrigerating chambers for carrying tropical fruit, and the banana should soon become as plentiful and as cheap as the orange. It has the merit of being wholesome and nutritious, and is suitable for invalids and young children as well as for dessert purposes.

The pine-apple we have already noticed. This is a deliciously refreshing fruit with healthful juices, and the demand for it is increasing daily. Hitherto, foreign pine-apples, which are gradually driving the home-grown fruit out of the market, are obtained in large quantities from the Azores. The further supply will no doubt come from the West Indies, where pine-apples can be grown in the open air as readily and as cheaply as the English farmer can grow beets or turnips. Unfortunately the choice varieties of pine-apples possess, as the growers say, "bad keeping" qualities; but with increased facilities for packing and stowing the fruit, there should be no difficulties of an insurmountable character in the way of bringing over pine-apples from the West Indies in excellent condition. At the colonial market attached to the Colonial and Indian Exhibition pine-apples from Antigua are sold at sixpence each, and we find from the Blue Book Report that the Bahamas export annually, either in a fresh or canned state, pine-apples to the value of 50,000*l*. annually. The mango (*Mangifera indica*, L.) is the apple of the tropics, and the mango tree is grown and occupies orchards as do apple-trees in England. Although an East Indian tree, it has become thoroughly naturalised in the west, where it spreads by self-sown seedlings over all waste places. Its introduction into Jamaica is thus described by Lunan ("Hortus Jamaicensis," 1814, p. 486):—"This beautiful tree was one of those brought to this island in June 1782, and taken in a French ship, bound for Hispaniola, by Capt. Marshall, of His Majesty's ship *Flora*, one of Lord Rodney's squadron. Capt. Rodney, with the approbation of Lord Rodney, deposited the mango plants and a great many others taken in the same vessel in Mr. East's garden (at Gordon Town), where they were cultivated with great assiduity and success, and have now become one of the commonest fruit-trees in Jamaica, in a great number of its varieties." As the mango ripens in July and August, and would come into the English market at a time when our own fruits are plentiful, it is possible it will not be greatly in demand. The best varieties are the Bombay sorts, while in Jamaica one of the plants deposited by Lord Rodney and having a number only, is is still known there and highly esteemed as "No. 11."

The passion fruits, of which there are several species, are highly prized. Of these there are the granadilla (*Passiflora quadrangularis*, L.), the pomme d'or, or water lemon (*P. laurifolia*, L.), the sweet cup (*P. edulis*, L.), and the calabash sweet-cup (*P. maliformis*, L.).

From the East Indies few if any fruits can be imported to England with success. In fact Ceylon and India have very few fruits which they can easily spare capable of bearing a long sea voyage, and the choice

fruits of Singapore and the Malay States are still further removed by time and distance.

At the Exhibition there is shown, from India, a small dried apricot (*Prunus armeniaca*, L.), an important article of food in the Punjaub Himalayas and in the North-west Provinces, which deserves attention as a probable source of an import trade for the English market. This fruit is known in India as the mish-mush, or "Moon of the Faithful." Dr. Watt remarks that it is largely eaten by all classes, fresh or dried, but chiefly fresh, and sometimes in preserve by Europeans. Sometimes the apricots are pressed together, and rolled out into thin sheets or "moons," 2 or 3 feet in diameter, like a blacksmith's apron. From Afghanistan large quantities of the dried fruit are imported into India, and distributed by trade far into the plains of Bengal.

Kew D. MORRIS

MICROSCOPIC ORGANISMS IN AIR AND WATER[1]

THIS Report is part of the "Annuaire de l'Observatoire de Montsouris" for the year 1885, and is worthy of careful study at the present time, when bacteriology is recognised as a special and important department of science. These investigations have been carried on at Montsouris since the year 1875, and through them Dr. Miquel has been enabled to throw much light on the meteorological aspect of the subject—an aspect that has received but little attention from investigators, as compared with the pathological. Every one will acknowledge that in entering upon a new field in scientific investigation it is extremely important that the line of research should proceed upon as broad a basis as possible, and that the work of experimentation and observation should not be confined to one aspect of the new study, however important it may be. Fallacies are sure to arise when any department of science is too narrowly specialised, from want of that more general knowledge which would prevent the adoption of erroneous views. This is especially liable to be the case in bacteriology, in which the objects of study are so minute and yet so widely distributed in nature. Dr. Miquel's researches—important as they are in themselves—are doubly welcome at the present time, as tending to popularise a field in which workers are urgently needed, as well as contributing largely to our knowledge. The example of Paris—the only city in which systematic investigations of the sort are now undertaken—should stimulate other towns which possess properly equipped meteorological laboratories, to conduct observations on the bacterial organisms contained in air, rain, and soil. The results obtained at Montsouris could then be confirmed or confuted by the results obtained at other laboratories under widely different climatic and meteorological conditions, and the enunciation of general laws and principles would in time become possible. We shall endeavour to place before our readers in this article some of the more important results and deductions made from them by Dr. Miquel, from the observations at Montsouris; but it should be distinctly recognised that any conclusions arrived at by Dr. Miquel are applicable only to Paris and its neighbourhood, and cannot at present be accepted as true for other places where the climatic conditions are different.

Tables are given in the Report, showing for each week of the years 1883-84 (*a*) the average number of bacterial micro-organisms present in a cubic metre of air, (*b*) the average barometrical pressure, (*c*) the average temperature of the air, (*d*) the average state of humidity of the air [percentage of saturation], (*e*) the amount of rainfall, (*f*) the electrical state of the air, (*g*) the direction and average velocity of the wind, (*h*) the average amount of ozone present in the air. From the observations recorded in these tables, Dr. Miquel has arrived at the following conclusions:—(1) An increase in the number of bacterial organisms contained in a cubic metre of air generally takes place when the barometrical pressure is high: this rule is not absolute, but the exceptions are rare. (2) Temperature does not cause such sudden increments; very often, it is true, a large increase in the number of microbes present in the air takes place in summer, but it is important to note that a sustained high temperature causes a manifest lessening in their number. The thermometer is capable of explaining certain seasonal variations, but not the weekly variations. (3) The maximum number of bacterial organisms present in the air corresponds almost always with a low hygrometric condition of the atmosphere; this is explained by the fact that the degree of humidity is always very high during rain, and when the superficial layers of the soil are soaked in water, periods during which the air is always very poor in bacteria. (4) It would appear *a priori* that the number of bacteria should increase with the strength of the wind, but observation negatives this assumption. A maximum number of microbes is found frequently during periods of calm—when the velocity of the wind is only 5-10 kilometres per hour—and minima have been observed during periods when the velocity of the wind was more than 30 kilometres per hour. (5) The direction of the wind exercises a considerable influence at Montsouris. The greatest number of maxima are noted when the wind is N.E., and the greatest number of minima when the wind is S.W. (6) When the amount of ozone in the air is large, the number of microbes present is small. The north winds blow over from Paris and contain but little ozone. They are rich in microbes. The presence of ozone in the air appears to have the power of destroying bacterial organisms, and, on the contrary, absence of ozone and humidity of the air—unless rain is falling—allow of an increase taking place in their number.

From observations at Montsouris, extending over a period of five years—1880-84—the average number of bacterial organisms in a cubic metre of air is stated to be: in winter 260, in spring 495, in summer 650, in autumn 380; the mean annual number being 445. In February the air is poorest in bacteria [the average of these five years is 165]. Towards the middle of summer the maxima present themselves [July 700].

Observations have also been conducted for a period of four years—1881-84—on the state of the air, as regards bacteria, in the centre of Paris. These observations were made on the air of the Rue de Rivoli, and afford a marked contrast in the number of micro-organisms to the far purer air of Montsouris, a suburb of Paris, and where, it is important to remember, the Observatory is situated in the centre of a park. The average of these four years' observations shows that the air of the Rue de Rivoli contains 3480 bacteria per cubic metre. The seasonal fluctuations are nearly the same as at Montsouris, the minimum being in February (1700) and the maximum in July (5010). The average number of bacteria present in a cubic metre of air, for the year 1881, was 6295, whilst the average number for 1884 was only 1830. This enormous decrease—which is observed in the intervening years to a slighter extent—is attributed by Dr. Miquel to the better drainage and scavenging of the city, and to the better cleansing of the gutters and watering of the streets in dry dusty weather, in 1884 than in 1881. The death-rate from zymotic diseases—in which are included typhoid fever, small-pox, measles, scarlatina, whooping-cough, diphtheria, dysentery, erysipelas, puerperal fevers, and choleraic diarrhœa of infants—has also fallen very considerably—27 per cent., if increase of population is taken into account—during this period. The death-rate of

[1] "Septième Mémoire sur les Organismes Microscopiques de l'Air et des Eaux," par M. le Dr. Miquel, Chef du Service Micrographique à l'Observatoire de Montsouris.

phthisis or consumptive diseases has, however, increased during the same period, although those of acute bronchitis and pneumonia have decreased. Acute pneumonia is now considered by many to be propagated by infection from specific organisms occasionally present in the air. The curves for the year 1883-84, representing the average weekly number of bacteria present in a cubic metre of the air of the Rue de Rivoli, A, and the weekly deaths from zymotic disease, B, are shown in Fig. 1. The curves are seen to present somewhat similar fluctuations except at the end of July and the first half of August, when the number of bacteria suddenly decrease—owing to the hot weather and sustained high temperature—whilst the deaths from zymotic disease undergo a large increase, owing to excessive mortality from infantile diarrhœa. The variations in the number of bacteria from week to week will be seen to be very much larger and more sudden than the variations in the zymotic death-rate. Very little can be deduced from comparisons extending over one year only, and although we are far from asserting that there can be no mutual relation between the number of micro-organisms present in the air, and the greater or less prevalence of epidemic disease among the community who breathe such air, still it is unsafe to found any arguments on such obviously inadequate data. It is only just to Dr. Miquel to say here that he recognises these difficulties, and is rightly cautious in drawing any conclusions except such as are founded upon an extended series of observations.

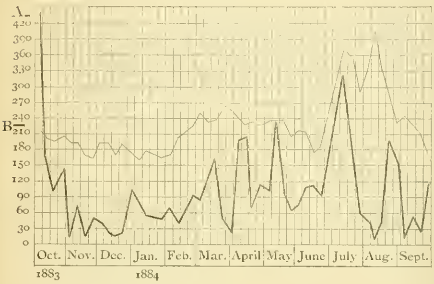


Fig. 1.

At the commencement of June 1884, Dr. Miquel, who was then in London, made some observations on the number of bacteria contained in the air of Ryder Street, St. James's. A cubic metre of this air was found to contain only 240 organisms, but this low result was probably due to the wet weather which prevailed on four out of the five days on which the experiments were conducted —the air being remarkably free from dust. In Paris at the same time the air of the Rue de Rivoli contained 360 organisms per cubic metre. Dr. Miquel would not, however, be surprised to find that the air of London was habitually fairly pure and free from organisms, owing to the proximity of the sea, and the fact that the houses of London being generally of no great height—unlike Paris—the streets are continually being swept by currents of air. The air of sleeping-apartments is very impure as regards the number of contained micro-organisms. One such room in Paris was found to contain on the average in the winter and spring of 1882, 73,540 bacteria per cubic metre, and the air of the Hôpital de la Pitié has been observed to contain 79,000 bacteria per cubic metre. In contra-distinction to these large numbers, the air over the Atlantic Ocean (Moreau and Miquel) has been found to contain from 0 to 6 bacteria per cubic metre, and the air of the higher mountains an average of only 1 bacterium per cubic metre (Freudenreich).

A considerable part of the Report is taken up with an account of researches conducted by M. Moreau into the number of organisms present in sea-air. These investigations—undertaken under circumstances of considerable difficulty on board ship, and conducted on an elaborate scale—are of much interest as bearing on the treatment of phthisis by high mountain altitudes or by sea voyages—in both cases the special object desired being to place the patient in an atmosphere free from all impurities. We will quote a few of M. Moreau's conclusions on this subject:—(1) Air taken on the coast, when the wind is blowing off the sea from a direction in which land is at a great distance, is in a state of almost perfect purity. (2) In the neighbourhood of continents, winds blowing from the land always bring an impure atmosphere; at 100 kilometres from the coast this impurity has disappeared. (3) During moderate weather the sea does not yield to the air any of its contained bacteria; during rough and stormy weather sea-air is charged with a minute quantity of bacteria. (4) The air of ships' cabins is also charged with a number of microbes incomparably greater than that of the open air at sea, but the purity of the air of these cabins increases rapidly during the first days of the voyage; later on, an equilibrium appears to be established, depending on the amount of purification of the air by ventilation and the number of occupants. (5) The air of ships' cabins is relatively very poor in bacteria; these probably are one hundred times less in number than the air of an occupied room in Paris.

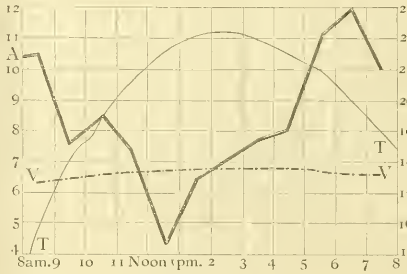


Fig. 2.

Observations have been made at Montsouris on the hourly variations in the number of bacteria contained in a cubic metre of air. These observations go to show that, contrary to the generally held opinion, the air is less pure—*i.e.* contains larger numbers of bacteria—during the morning and evening than at midday. In Fig. 2 are shown curves corresponding to proportional figures which illustrate this phenomenon, as ascertained by forty experiments. A is the bacterial curve, T is the curve representing the temperature, and V is the velocity of the wind. The lowest point of the bacterial curve is between noon and 1 p.m., two hours before the maximum temperature is reached. From 8 o'clock in the evening until midnight the number of microbes generally remains high, but decreases rapidly from midnight to 3 a.m., two or three hours before the lowest temperature is reached, and rises rapidly from 4 a.m.—when the ground and vegetation are covered with dew—until 6 a.m., when the maximum is reached. These night observations, however, are too few in number to be depended upon to give a very correct average. Rain, as has been before remarked, rapidly

purifies the air. But when the rain first commences to fall, the number of bacteria increases. This Dr. Miquel explains by supposing that many of the first drops of rain evaporate—the atmosphere not being saturated with vapour—and deliver up the bacteria they hold to the air in the neighbourhood of the earth. Later on the air is saturated with vapour, and the bacteria floating in it are carried down to the ground in the drops of rain, and by this means the air is purified.

Investigation of the organisms contained in rain show that the rain which first falls in a shower and that which falls after a period of dry-weather contain far larger numbers of bacteria than that which falls at any other times. Under such circumstances 200,000 microbes per litre is not an unusual quantity. The rain which falls during the warm months of the year—in summer and autumn—contains more microbes than that which falls in winter and spring. During the year 1883-84 the lowest monthly average was 1000 per litre in November, and the highest 6980 per litre in September. As the rain derives its organisms from the air which it purifies in its descent, we should expect the seasonal variations in the number of contained organisms in air and rain to correspond closely—as in fact they do. It is important to note that the organisms exist in the rain to a larger extent in the form of germs than in the adult state. Of 100 bacteriform organisms found in rain, on an average 60 are micrococci, 25 bacilli, and 15 true bacteria. But the numbers here given are subject to great variation in different falls of rain: the bacilli may be more numerous than the micrococci, the true bacteria being almost always fewest in number. Dr. Miquel calculates that during a year at Montsouris 4,000,000 of bacteria are carried down in the rain to each square metre of surface. This number, though not probably representing anything like the real figure, demonstrates that rain is a powerful agent in diffusing aërial bacteria and fungi. We have yet much to learn as to the part these organisms undoubtedly play when diffused into the soil, in altering or rearranging its component parts or constituents so as to render it more fit for sustaining vegetable life and growth.

The methods and apparatus employed by Dr. Miquel in his researches are very fully explained, and contrasted with other methods—especially those employed by Dr. Koch and other German observers—in terms that are not exactly those which a strict regard for international courtesy would dictate. It is somewhat of a reproach to bacteriologists that their leading authorities in all countries appear unable to keep clear of controversies which are conducted with an acrimony and animus more instructive than seemly. International jealousy would appear to lie at the root of much of this evil, and is plainly discernible in the writings of some of the ablest masters of the science.

THE RECENT VOLCANIC ERUPTION IN NEW ZEALAND

UNTIL the report of a trained geologist has been received we must be content with the narratives, often conflicting, of the surveyors and of the Press correspondents who hurried to the scene of the great catastrophe that has recently devastated the wonderland of New Zealand. In the meantime, however, it is possible from the various accounts to trace the leading features of the eruption, and to note their resemblance to those of other recorded volcanic outbursts. It is impossible not to be struck with the analogy between the phenomena exhibited last June in New Zealand and those that accompanied the great Vesuvian eruption in the first century of our era. In both instances a mountain which had never been known to be an active volcano suddenly exploded with terrific violence, filling the air with ashes and stones. At each locality there were the premonitory earthquakes, the thick black pall of volcanic cloud hanging over the mountain, the descent of dust, sand, and hot stones, the discharge of mud, with, so far as known, no outflow of lava, and the overwhelming of an inhabited district under a deep covering of loose volcanic debris.

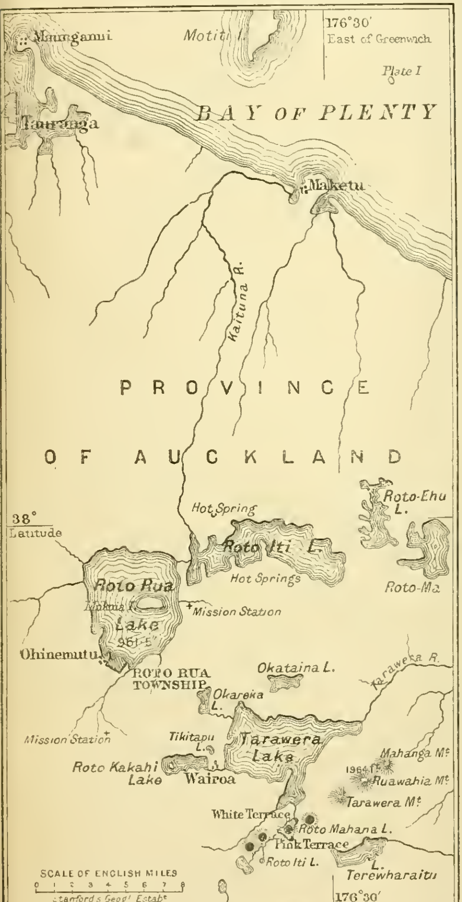
In a region so subject to earthquake shocks as that which crosses the centre of the North Island of New Zealand in a north-east and south-west direction, it was natural that no special attention should have been given to any greater frequency or violence of the shocks before the date of this volcanic eruption. But no doubt facts bearing on this subject have been noted by local observers and will in due course be published. From the newspaper accounts, indeed, there would appear to have been various precursory indications which in the light of subsequent events may not have been without importance. It is said, for instance, that the extinct volcano Ruapehu, the highest peak in the North Island, which since the discovery of New Zealand has never been known to manifest any activity, began to steam at the top some three weeks before the eruption. A fortnight previous to the catastrophe a wave 3 feet high suddenly arose on the Lake Tarawera, lying at the foot of the mountain of the same name, and in the very focus of the subsequent disturbance, and washed the boats out of the boat-houses. Doubtless there were other premonitory symptoms, besides earthquake activity, of the approaching event, though only a few days before their destruction, the famous White and Pink Terraces were visited by a party of tourists who observed no unusual vigour in the hot springs there, nor any indication whatever that these fairy-like deposits were so soon to be the theatre of violent volcanic energy.

About half an hour after midnight on the morning of June 10 the earthquake shocks that are familiar to the inhabitants of the Lake District assumed an altogether unusual vigour and frequency. At the settlement of Wairoa, which is about five miles from the warm lake and sinter terraces of Rotomahana, the ground shook violently for an hour or more, the more powerful shocks following each other at intervals of about ten minutes. The alarmed inhabitants, startled from sleep, ran out of their houses or clung to each other inside for mutual assistance and encouragement. At last, a few minutes after 2 a.m., a shock of exceptional severity was followed by a deafening roar, and suddenly what is described as a "pillar of fire" rose up from the crest of the mountain range some five or six miles eastward on the opposite side of Lake Tarawera. The top of Mount Tarawera (about 2000 feet high) had been blown into the air, leaving a huge chasm on the flank of the mountain. The glow of the white-hot lava in the interior ruddied the sky for miles around. Thousands of blocks of glowing lava described as "fire-balls" were shot into the air. The canopy of dark ashes that soon gathered over the mountain and spread out for miles around became the theatre of a violent electrical storm. It seemed to be torn asunder with incessant flashes of lightning, and the continuous peals of thunder, mingling with the bellowing of the volcano, increased the terror of the night.

That an eruption should ever take place from the three huge truncated cones that frown over Lake Tarawera was not regarded by geologists as a future probability. They had been extinct even from the times of early Maori tradition. To their solitary and mysterious summits the natives had probably for centuries been accustomed to carry their dead. The bones of many successive generations lay bleaching on that high lonely plateau, which had thus come to possess a peculiar sanctity in the eyes of the Maoris, who would not willingly allow a white man to approach it. Not only were these great cones to all appearance extinct, but the volcanic action of the whole district was of that type of waning energy which geolo-

gists have called the "solfatara stage." New geysers might break out, rivalling or even surpassing those already active in the district, and the orifices of eruption might shift from place to place, involving considerable local disturbance in their transference; but no one anticipated that in this district a great explosion, like the most gigantic outburst of Vesuvius, was likely to occur.

The magnitude of the explosion may be inferred from



NEW CRATERS, thus

several facts which appear in the newspaper reports. An observer at New Plymouth, on the west side of the island, 150 miles from the scene of the disaster, saw the column of ashes rising far into the air, and computed its height to be not less than 22,000 feet. The noise of the explosion is said to have been heard at Christ Church, a distance of some 300 miles. The ashes fell over a vast area of land and sea to the north and east of the vent of discharge. Vessels sailing even 130 miles away found the air thick with fine dust, which settled on their decks. Near the scene of the explosion the depth of accumulated ashes, still quite hot, was found by some adventurous explorers, seeking to succour the poor Maoris, to be not less than 20 feet. At a distance of 30 or 40 miles the deposit was still several inches thick, so stupendous must have been the amount of rock blown into powder by the great explosion of Tarawera.

The materials ejected from that mountain appear to have consisted mainly of loose fragments of lava, cinders, ashes, and fine dust, with vast quantities of steam, while not improbably hot water and mud issued from the flanks of the volcano. So far as can be gathered from the narratives, there was no emission of lava, though, from the "flames" and "fire-balls" so generally referred to, it may eventually be found that molten lava flowed out somewhere on the sides of the mountain.

Not far from the base of the volcanic cone of Tarawera lay the warm lake and sinter terraces of Rotomahana. The treacherous nature of that district has been often remarked—its steam-vents, boiling pools, hot steaming soil, and eruptive geysers, not always remaining in the same places, but apt with no warning to break out at fresh points. This weird locality has been involved in the volcanic disturbances of the region. The famous terraces have been blown into the air, and fragments of their sinter have been picked up among the dust and ashes of the surrounding country. The lake on the borders of which they stood has been ingulfed. On their site scores of mud-cones are vomiting forth stones and mud, and hurling clouds of steam into the air. New vents for the escape of steam and the outflow of volcanic mud have been opened all over the country, and the aspect of the landscape has been entirely changed. A scene of fairy-like beauty has been transformed into one of loathsome desolation. Even if the volcanic activity calms down and the sinter springs can recommence their work of deposition, many generations must pass away before they can build up again such terraces as have been destroyed. The new features of the country will no doubt still prove attractive to tourists, but the marvellous staircases of Te Tarata are a dream of the past.

Among the features of the eruption on which it is to be hoped that light will be thrown by the more detailed investigations of experts are the source and behaviour of the mud that overwhelmed the settlement of Wairoa. From the narratives of the survivors, showing that the houses were crushed in from above, the mud seems to have descended through the air upon the district, mingled with ashes and stones. Rain fell during the night, but the mud could hardly have been formed in the air by the mixing of the rain and dry ashes. It appears to have come down as liquid mud and was no doubt ejected as such from some neighbouring vent. The orifice of eruption could scarcely have been the great cone of Tarawera; more probably there were many vents not only at Rotomahana, but nearer to Wairoa, by which a large amount of mud was discharged over the surrounding district.

Another question that will no doubt receive careful consideration relates to the movements of the air during the time of the eruption. Barometric observations at Rotorua and at places on the opposite sides of the island will be of much interest. From the newspaper accounts it is clear that a great atmospheric disturbance accompanied the eruption. About an hour after the great explosion a gale suddenly arose in the Rotorua and Wairoa district, and blew with such fury as to uproot and prostrate immense numbers of trees, and to strip off leaves and branches from those that were left standing. At Rotorua the direction of the hurricane was towards the scene of volcanic activity, as if the air were being drawn into the vortex caused by the explosion. A few hours later the gale as suddenly ceased and then ashes began to fall, borne northwards by some upper current of air. We

have yet to learn how far these atmospheric movements were connected with or independent of the eruption.

There are some excellent geologists in New Zealand who have now a rare opportunity of investigation. No mud-eruptions at all comparable in magnitude to those of this summer in New Zealand have ever been known. The connection of these with the explosion of Tarawera, the relation of the latter to the lava-reservoir inside, the nature of the so-called "flames" and "pillar of fire" so conspicuous on the night of the eruption, the sources of the "fire-balls," and many other details, offer a wide and most interesting field for the colonial observers. Geologists all over the world will await with much interest the publication of their investigations. ARCH. GEIKIE

NOTES

THE honour of knighthood has been conferred on Mr. Philip Magnus, the head of the City and Guilds Technical Institute at South Kensington.

LAST Saturday a banquet was given to M. Chevreul by French students to celebrate the one hundredth anniversary of his birth.

THE death is reported on June 22 of Dr. H. F. Hance at Amoy, at which place he was Her Majesty's consul. Although no independent work bears Dr. Hance's name, he has done more than any other man to make us acquainted with the flora of China, both of the empire and of the British colonies. His contributions to botanical literature are to be found in periodicals, very largely in Trimen's *Journal of Botany;* and the number of species described by him for the first time is very great. He was a contributor to the herbaria at the British Museum and at Kew. It is to be hoped that his herbarium will be brought to London and deposited where it can be consulted, and his types readily compared with those of other authors. A full synonymic catalogue of all the known Chinese plants is now in course of publication by Messrs. Forbes and Hemsley, and is greatly needed.

UNDER the energetic management of Dr. Adolph Bastian the Administration of the Berlin Royal Ethnological Museum has commenced to publish a series of original communications explanatory of the varied contents of that superb collection. According to the present programme four parts of about 60 large octavo pages each, with two or more plates of illustrations, will be issued yearly by the Berlin publisher, W. Spemann, at the price of 16 marks, or 4 marks each. Judging from the three parts, which have already appeared for the year 1885–86, the series promises to develop into a vast encyclopædia of anthropological subjects. Many of the communications constitute in themselves more or less exhaustive essays on special branches of ethnology, and to some of them a peculiar value attaches, because contributed by the collectors or observers themselves. Such, for instance, is the paper on funeral rites in the Pellew Islands, contributed to the first number by the traveller, Kubary, who has spent many years in the Oceanic regions, and made a special study of the Polynesian and Micronesian islanders. The same number contains a report on Richard Rohde's expedition to the Paraguay and Brazil (Matto Grosso) in 1883–84, followed by the traveller's account of the Bororo and Guato tribes in the Upper Paraguay basin. Of great value is Dr. O. Finsch's paper in Part II. on the ethnological collections from the South Sea Islands, some 3000 objects, of which more than half found their way to the Berlin Museum. The importance of securing specimens from this region before it is too late is well shown by the experience of this traveller, who on revisiting New Britain in 1884-85 was no longer able to procure several objects which were readily obtainable three years previously. Others, such as wood-carvings, are now "manufactured" in New Ireland "for the trade," the natives finding good customers amongst the crews of passing ships. This paper is followed by the explorer Grabowski's description of nearly 200 objects from South and East Borneo, many extremely rare, if not quite unique. Dr. Bastian concludes a characteristic essay at the end of Part III. with the words: "So walte ein Jeder seines Amtes,—und uns ist die Aufgabe zugefallen,—Das rechtzeitig in Sicherheit zu bringen, was morgen schon verloren sein mag."

IN the July number of the *American Journal of Science*, Prof. Rockwood, jun., of Princeton, publishes a paper (the fifteenth of the series) on the earthquakes of North and South America for the year 1885. The writer again attempts to assign to each earthquake a grade of intensity, based where possible upon the character of the physical phenomena reported. Where no such information was available the estimate is based on the phraseology of the original report, taking into account the probable intensity of an earthquake in the locality in question. There is, as Prof. Rockwood recognises, a large element of uncertainty in these estimates, "but at least they are the best that can be made now, and better than any one else could make at a later date and without access to the original reports." This last consideration alone appears to us to justify the attempted classification. A large proportion of the American earthquakes of the year occurred in California, and accordingly a small outline earthquake map of that State for the year is given. The whole list contains 71 items: 34 belong to the Pacific coast of the United States, 9 to the Atlantic States, 8 to the Canadian provinces, 5 to New England, 3 to the Mississippi valley, 5 to South and 2 to Central America.

WE may also take this opportunity of referring to the same writer's annual account of the progress in vulcanology and seismology for 1885, published in the Smithsonian Report. Most of the incidents recorded, and papers and other publications referred to have from time to time been described or reported in our own columns, and it is therefore only necessary to say that in this as in previous years the summary shows great care and industry, and should be an invaluable *vade mecum* to all interested in the study of these phenomena.

THE United States Commissioners are still continuing their labours in the direction of acclimatising flatfish to American waters, which are nearly as well suited to the *Pleuronectidæ* as English waters. Several attempts have been made to transmit them from our shores alive, but hitherto with little success. Information has, however, reached us to the effect that fifty soles during last month were sent by the Derby Museum authorities at Liverpool, twenty-five of which reached their destination alive. Prof. Baird, the Chief Commissioner, states that the fish are in a thriving condition and feed well. They have been placed in a suitable habitat at Washington Station, and are being watched with special care. The soles, which are about three inches long, were transmitted in glass globes attached to the ceiling of the cabin of the s.s. *Britannic*. This method minimises the risk of injury to the fish through the oscillation of the vessel, as the receptacles, being swung, move backwards and forwards with the motion of the steamer. The National Fish Culture Association intend forwarding to America another consignment of flatfish next month in order to assist the operations of the Commissioners. The sole is a very delicate fish, and cannot withstand the strain of protracted journeys, which makes it very difficult to transmit.

MESSRS. J. B. LIPPINCOTT AND CO. have in the press a "Manual of North American Birds," by the eminent ornithologist, Prof. Robert Ridgway, Curator of the Department of Birds, Smithsonian Institution, Washington, D.C. The author has had

exceptional advantages for the preparation of a treatise of this character, arising from his own field experience, as well as his connection with the National Museum, and the free access which has been granted him to various other public and private collections of birds, both in this country and Europe. The work is to contain some 425 illustrations suitably executed, and will conform to the geographical limits, classification, numeration, and nomenclature adopted by the American Ornithological Union.

ACCORDING to an official notification of the trustees of the "Schwestern Fröhlich Stiftung" at Vienna certain donations and pensions will be granted from the funds of this charity this year in accordance with the will of the testatrix, Miss Anna Fröhlich, to deserving persons of talent who have distinguished themselves in any of the branches of science, art, or literature, who may be in want of pecuniary support either through accident, illness, or infirmity consequent upon old age. The grant of such temporary or permanent assistance in the form of donations or pensions is, according to the terms of the foundation deed, primarily intended for Austrian artists, literary men and men of science, but in which however foreigners of every nationality—English and others—may likewise participate provided they are resident in Austria. Donations will be granted to artists and men of letters and science for the purpose of completing their studies, and for the execution or publication of a certain specific work, and also in cases of sudden inability to work. Pensions are being granted to artists and men of letters and science who, in consequence of old age, illness, or other misfortunes, have been placed in a position of want. The applications addressed to the trustees (das curatorium) must be transmitted to the president's office of the Common Council of the City of Vienna (an das Präsidial-Bureau des Wiener Gemeinderathes Neues Rathhaus) before August 31, 1886, through the Austro-Hungarian Embassy in London, 18, Belgrave Square, S.W., where also further particulars of the terms and conditions of the foundation deeds, &c., can be obtained.

THE fifth Circular of Information for 1885, published by the United States Bureau of Education, is a very exhaustive treatise upon physical education. It directs attention to the four different ideas of manly excellence: the Greek, or æsthetic; the monkish, or ascetic; the military, or knightly; and the medical, or scientific. The recent war popularised the third in America, and reports of work of that class are accordingly to be found here. The teachings of the fourth, down to the most recent day, are of course the essence of the paper, and more than once attention is called to the great tendency of such teaching to return to the old standards of the first. In England, and still more in America, the oldest colleges and schools were founded with the principal object of educating a clergy, and accordingly were imbued with the spirit of the second—an important result of which has been that the games which occupy so much of the thoughts and the time of the students have hardly ever been recognised by the authorities, who have quite recently been forced to take them under their control to prevent abuse. It is felt now how important it is that games and exercises should be supervised and made a part of education, to be carefully controlled by a competent M.D., who shall be one of the Faculty on a par with the other masters. Rugby football is condemned by the great Harvard University as a "brutal and dangerous" game. Many strong objections to much that attends public games are recounted, and many careful restrictions on games generally have been agreed to by the highest authorities, both scholastic and medical. The code and the books of Mr. Maclaren, of Oxford—a prophet too little honoured in his own country—are highly praised, but Germany carries the palm for science and laborious thoroughness. The manual labour which has been so successful in the lower-grade schools is naturally not found popular in the colleges. A warning voice is raised against the high pressure at which girls live both at work and pleasure, and the necessity of more regular rest and exercise is insisted upon. A large part of the Circular consists of plans and elevations of gymnasiums, useful to both schoolmasters and architects.

THE labours of a curator who undertakes the first setting in order of the raw material of a museum in a new building are fully disclosed in the Report of the Nottingham Free Natural History Museum, which published the works of Mr. J. F. Blake, the Superintendent. And it can be no small labour to which he has devoted himself, even though extending over five years; comprising, as it does, the naming and re-naming of 11,400 specimens of every class of natural production hitherto kept with very little care; the supplementing them, as opportunity offered, with important desiderata till the total number of objects exhibited has increased to 21,960; the preparation of maps showing the geographical distribution of the animals near which they are placed; the pictorial mounting of specimens of birds and of birds' nests; and the specially important duty of a local museum of getting together as complete a local collection as possible. At the same time we regard it as a most wholesome symptom in the case that Mr. Blake is by no means satisfied with his achievements, and we wish him more rapid progress in all his present and future undertakings on behalf of the Museum.

AMONG recent contributions to natural-history literature, attention is due to an interesting work by the eminent Norwegian naturalist, Leonhard Stejneger, published in the United States under the title of "Results of Ornithological Explorations in the Commander Islands and Kamtschatka." In this work, which is illustrated by coloured plates, the author describes upwards of 140 species, all of which were collected or observed by himself during his various visits to Behring's Sea and the neighbouring coast-lands and islands. The main results of these expeditions had previously appeared in the *Bulletins* of the U.S. National Museum, in the Ornithological Department of which institution Mr. Stejneger holds the post of Assistant Curator, while he has also from time to time supplied *Naturen* with pleasantly-written popular reports of his voyages and observations, and to these we have more than once had occasion to make favourable reference. As, however, both these sources are inaccessible to the general reader, we welcome with great satisfaction the present comprehensive English exposition of Mr. Stejneger's most recent contributions to the branch of science which he so successfully cultivates. Within his own province he has, moreover, been doing good service to popular science as compiler of the ornithological portion of the American "Standard Natural History," published at Boston by Messrs. Cassino and Co. In Mr. Stejneger's original contributions to this work, which claims to be based on the most recent results of science, he has been able by his own observations to make various additions to, and corrections of, the statements of Brehm, who has hitherto been trusted as our principal authority regarding North European and American ornithology.

DURING a severe thunderstorm which passed over Central Norway last week a remarkable example of the power of lightning was witnessed. In a field at Löiten a fir-tree 80 feet in height was struck by lightning some 12 feet from the ground, with the effect that the tree was cut in halves and the upper portion—about 60 feet in length—thrown a distance of several yards. The most curious part, is, however, that the surface of the detached part is as smooth as if the tree had been sawn through, whilst that of the stump remaining in the ground is jagged, charred, and splintered to the root. The ground around the tree is furrowed in all directions, one being several feet in

width and depth, and extending for some 10 yards. A spruce-tree close by shows a furrow an inch in width running from a height of 6 feet down to the root.

ON Thursday night, at 11.30, M. L'Hoste crossed from Cherbourg in a balloon, alighting in the neighbourhood of London at 6.30 on Friday morning. M. L'Hoste had a small sail to assist in directing the balloon, and an apparatus for letting down into the sea to draw water into the balloon to act as ballast. He was accompanied by M. Mangot, the astronomer. The highest altitude attained was 3600 feet, and the lowest temperature observed 7° C.

THE additions to the Zoological Society's Gardens during the past week include a Malbrouck Monkey (*Cercopithecus cynosurus*) from West Africa, presented by Mrs. Barrington; a Blue-faced Amazon (*Chrysotis æstiva*) from Brazil, presented by Mrs. J. Fletcher; an Aldrovandis Skink (*Plestiodon auratus*) from North Africa, presented by Mr. R. J. M. Teil; two Grey Parrots (*Psittacus erithacus*) from West Africa, deposited; a Sea Eagle (*Haliaetus* ——), a Masked Weaver Bird (*Hyphantornis personata*) from Africa, a Short-eared Owl (*Asio brachyotus*), European, a Hyacinthine Macaw (*Ara hyacinthina*) from North Brazil, two Blanding's Terrapins (*Clemmys blandingi*) from North America, two Indian River Snakes (*Tropidonotus quincunciatus*) from India, purchased; two Triangular Spotted Pigeons (*Columba guinea*), a Geoffroy's Dove (*Peristera geoffroii*), four Brazilian Teals (*Querquedula brasiliensis*), five Slender Ducks (*Anas gibberifrons*), two Chilian Pintails (*Dafila spinicauda*), two Wild Ducks (*Anas boschas*), a Himalayan Monaul (*Lophophorus impeyanus*), bred in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *AUGUST* 8–14

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on August 8

Sun rises, 4h. 37m.; souths, 12h. 5m. 25·3s.; sets, 19h. 34m.; decl. on meridian, 16° 6′ N.: Sidereal Time at Sunset, 16h. 43m.

Moon (two days after First Quarter) rises, 14h. 45m.; souths, 19h. 25m.; sets, 0h. 1m.*; decl. on meridian, 16° 41′ S.

Planet	Rises	Souths	Sets	Decl. on meridian
	h. m.	h. m.	h. m.	° ′
Mercury ...	6 10 ...	12 51 ...	19 32 ...	7 24 N.
Venus	1 57 ...	10 3 ...	18 9 ...	22 4 N.
Mars	10 51 ...	16 11 ...	21 31 ...	8 41 S.
Jupiter... ...	9 5 ...	15 7 ...	21 9 ...	0 25 S.
Saturn	1 58 ...	10 4 ...	18 10 ...	22 4 N.

* Indicates that the setting is that of the following morning.

Variable Stars

Star	R.A.	Decl.			
	h. m.	° ′		h. m.	
U Cephei	0 52·2 ...	81 16 N. ...	Aug. 12,	21 49	*m*
R Arietis	2 9·6 ...	24 31 N. ...	,, 12,		*M*
T Ursæ Majoris ...	12 31·2 ...	60 7 N. ...	,, 10,		*m*
U Virginis	12 45·3 ...	6 10 N. ...	,, 10,		*m*
R Camelopardalis .	14 26·3 ...	84 21 N. ...	,, 11,		*M*
δ Libræ	14 54·9 ...	8 4 S. ...	,, 14,	20 30	*m*
S Libræ	15 14·9 ...	19 59 S. ...	,, 12,		*M*
U Ophiuchi	17 10·8 ...	1 20 N. ...	,, 12,	2 10	*m*
			,, 12,	22 18	*m*
U Sagittarii	18 25·2 ...	19 12 S. ...	,, 12,	0 0	*m*
β Lyræ	18 45·9 ...	33 14 N. ...	,, 8,	0 0	m_2
R Lyræ	18 51·9 ...	43 48 N. ...	,, 13,		*m*
η Aquilæ	19 46·7 ...	0 43 N. ...	,, 13,	2 0	*m*
δ Cephei	22 24·9 ...	57 50 N. ...	,, 11,	0 0	*M*

M signifies maximum; *m* minimum; m_2 secondary minimum.

Meteor Showers

The principal shower of the week is that of the *Perseids*, R.A. 43°, Decl. 56° N. The maximum occurs on August 10, but many meteors from the same radiant are usually seen on the nights both immediately preceding and following that date. Meteors are frequently seen also from neighbouring radiants, *e.g.* near the Pleiades, R.A. 55°, Decl. 26° N.; near Capella, R.A. 68°, Decl. 46° N.; near ν Tauri, R.A. 55°, Decl. 7° N.; and from Lynx, R.A. 96°, Decl. 71° N. A radiant from α Pegasi, R.A. 345°, Decl. 15° N., is also active at this season, which is usually the most prolific of the year.

Occultations of Stars by the Moon (visible at Greenwich)

Aug.	Star	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image
			h. m.	h. m.	° °
8 ...	24 Scorpii ...	5 ...	19 15 ...	20 34 ..	92 255
11 ...	*d* Sagittarii ...	5 ...	18 44 ...	20 0 ...	74 242

Aug.	h.	
8 ...	14 ..	Venus in conjunction with and 0° 1′ south of Saturn.

THE SCIENTIFIC DEVELOPMENT OF THE COAL-TAR COLOUR INDUSTRY[1]

THE subject on which I propose to address you this evening has been brought under the notice of the Society of Arts on former occasions, and was only last year thoroughly treated of by Dr. W. H. Perkin, the pioneer of this branch of manufacture, in his Presidential Address to the Society of Chemical Industry. It has, moreover, quite recently furnished materials for a lecture at the Royal Institution by Sir Henry Roscoe, so that I feel in some measure obliged to apologise for again bringing forward a topic upon which it may appear to chemists that nothing particularly new remains to be said. Having, however, been intimately associated with this branch of chemical industry for many years, I have had exceptional opportunities of watching its development, and of forming ideas upon the causes of its progress, which may justify my engaging your attention upon the present occasion.

The manufacture of coal-tar products is a subject which offers distinct advantage for popular treatment, both on account of the practical utility of the products, and the striking and beautiful colour phenomena which they present. But I do not propose now to avail myself of these advantages, because I think there is a more serious aspect of the subject than the mere enumeration of the names, chemical formulæ, and mode of preparation of the recently discovered products. I must take it for granted that those present this evening are familiar with the fact that out of coal-tar there are obtained series of hydrocarbons from which are prepared nearly all the dye-stuffs at present in use; that the introduction of these artificial colouring-matters has revolutionised the tinctorial industries, and that the tar of gas-works, which was formerly a waste product and a nuisance, is now a valuable source of revenue to the gas companies. It is, doubtless, known to you also that, besides colouring-matters, there are obtained from the same hydrocarbons artificial perfumes, and drugs which rival quinine in efficiency; and that quite recently a substance has been discovered by Dr. Fahlberg, which goes by the name of "saccharine," and which is stated to possess 220 times the sweetening power of cane sugar.[2] But I must content myself by merely pointing to such discoveries as triumphs which the chemist, by his "so potent art," has achieved in recent times, because I want to bring home to English manufacturers one particular point in connection with this industry, and the Chemical Section of the Society for the Encouragement of Arts, Manufactures, and Commerce seems to me to be the right place for so doing. It cannot be denied that the coal-tar industry has for some years past been migrating from this country, the land of its birth, to the continent of Europe. Of the causes of this decline assigned by Dr. Perkin and others who have expressed opinions upon the subject, I do not now propose to touch upon those which may be considered as purely politico-economical

[1] A Paper read at the Society of Arts by Prof. R. Meldola, F.R.S., F.C.S., F.I.C., on May 13, 1886.

[2] The substance referred to is an anhydro-derivative of orthosulphaminebenzoic acid having the formula

$$C_6H_4 < \begin{matrix} CO \\ SO_2 \end{matrix} > NH.$$

For details, see a paper by I. Levinstein, *Journ.* Soc. Chem. Ind., Feb. 1886, p. 75. Also the original communications in the *American Chemical Journal*, vol. i. pp. 170 and 426, and vol. ii. p. 181. For an investigation of the physiological action see the *Archivio per le scienze mediche*, vol. ix. No. 22, p. 407 (Turin, 1886).

ones, such as Free Trade, the Patent Laws, or the available energy of a British as compared with a German workman under the stimulus of a certain amount of weekly wage. These and kindred questions—such as the cost of railway carriage, or the relative ideas of "making a manufacture pay" which may exist in the British and Teutonic minds—have, doubtless, a most important bearing upon the main subject, but their discussion would occupy far too much time, and would, moreover, be out of place in this Section. I might even go so far as to express a private belief that if this portion of the subject were handed over for legitimate treatment by economists, the conclusions arrived at (if any) would hardly be commensurate with the amount of discussion which would be evoked. In fact, it appears to me that although, in a general way, each of the causes mentioned must be a factor in determining the success of any branch of manufacture, it is quite impossible to assign its true value to each of these factors; and in the case of the present industry I am persuaded that it is now a question of chemical and not of economic science that is pressing for consideration.

It will, I think, be conceded that the manufacture of coal-tar products is *par excellence* the most scientific of the chemical industries. This high position may fairly be claimed for the industry when we consider the number and complexity of the products, the delicacy of many of the reactions employed, the special arrangements of plant required, and the intimate knowledge of the chemistry of the aromatic compounds which the colour chemist must at the present time possess. Moreover, the industry is of comparatively recent growth—it has been born and has reached its present development within the last thirty years, so that the successive phases of its evolution can be clearly traced. For these reasons the subject is well calculated to throw light upon the general question of technical chemical education, a question of which the importance to the country at large now bids fair to become duly recognised.

In treating of the industrial development of a branch of chemical manufacture, it is important that we should begin with a distinct idea of the products themselves. I must claim the indulgence of chemists if at this stage I find it necessary to go over somewhat old ground, and to state facts with which so many are familiar. It would, of course, be quite impossible to give, on the present occasion, anything like a complete chronological list of the various colouring-matters, and it would be equally impossible for me to enter into the discussion of the chemical structure of the beautiful compounds which are now to be met with in the market. If, later on, I find it necessary to enter into questions of chemical constitution, it will be chiefly with the object of illustrating general principles by appealing to particular cases. In the brief historical sketch which I now propose to lay before you, I shall mention only those discoveries which may be considered to mark distinct commercial epochs in the development of the industry. The successive steps in this development will furnish us with one of the most striking illustrations of the utilisation of scientific discovery for industrial purposes, and the reaction of industry upon pure science.

Commencing in the year 1856, the foundation of the coal-tar colour industry was laid by Perkin, by the discovery of mauve, a violet dye, obtained accidentally in the course of an investigation having for its object the preparation of quinine by an artificial synthesis. In 1860, magenta, which had formerly been made in small quantities by expensive processes, was rendered a product of the first order of commercial importance by the discovery of the arsenic acid process by Medlock and E. C. Nicholson simultaneously. During this same year phenylated blues were first produced by Girard and De Laire, by the action of aniline upon magenta base at a high temperature. These blues had but a limited application owing to their insolubility, and their value was enormously enhanced by Nicholson's discovery, in 1862, that these colours could be converted into soluble sulphonic acids. The first azo-colour, amidoazobenzene, a basic yellow dye, was introduced in 1863 by the firm of Simpson, Maule, and Nicholson, under the name of "aniline yellow." In this same year the methylic and ethylic derivatives of magenta were manufactured by the same firm under the name of "Hofmann violets," in honour of their discoverer. "Azodiphenyl blue," the first of the colouring-matters now known as indulines, and Manchester yellow, appeared in 1864; and in 1866 "Bismarck brown" (triamidoazobenzene) was first manufactured at Manchester. The same year (1866) was marked by the introduction of Coupier's nitro-benzene process for the manufacture of magenta. In 1868 Graebe and Liebermann gave to the world their great discovery of the chemical constitution of alizarin, and in the following year the manufacture of this colouring-matter from anthracene was commenced. The first members of the great family of the "phthaleïnes," viz. galleïn and fluoresceïn, were discovered by Baeyer in 1871; and the first technical application of this discovery was made in 1874 by Caro, who introduced the beautiful pink tetrabromfluoresceïn into commerce, under the name of "eosin." Diamidoazobenzene was discovered by Caro and Witt independently in 1875, and was introduced into commerce by the latter as "chrysoïdine." A great impetus was given to the technical production of azo-colouring matters by this discovery, the naphthol oranges and other "tropœolines," fast-red, the ponceau scarlets, &c., appearing in 1878. Methylene blue and acid magenta were introduced by Caro in 1877, and in the same year the old and fugitive "aniline yellow" was converted into a valuable acid yellow by Grässler, who patented a process for converting the base into a sulphonic acid. Malachite green was introduced in 1878, and in 1879 the first member of the now important group of secondary azo-compounds appeared under the name of Biebrich scarlet. It is these secondary azo-scarlets, and especially the "croceine scarlets" (discovered in 1881) which are exterminating the cochineal industry. The year 1880 was marked by the brilliant discovery of the constitution of indigo, and the synthesis of this colouring-matter by Baeyer, a discovery which is none the less a triumph of synthetical chemistry because the manufacture is not at present successful from a commercial point of view. Indophenols were introduced by Koechlin and Witt in 1881, and in 1883 appeared Caro's first patent for the production of colouring-matters of the rosaniline group by the method of "condensation" with phosgene gas, in the presence of suitable condensing-agents.

This chronological record comprises nearly all the chief colouring-matters from coal-tar which are, or have been, of industrial value. It is important to note that the list, even as it stands in the form of a bald statement of facts in chemical history, reveals the existence of that fundamental law of the "survival of the fittest." Old products have been displaced by newer ones, as fresh discoveries were made, or processes improved, and to the chemist it is of interest to observe how this development of an industry has gone on *pari passu* with the development of the science itself. The moral conveyed to the manufacturer is sufficiently obvious. If we are to recover our former supremacy in this industry, we must begin by dispelling conservative ideas —we must realise the fact that no existing process is final, and that no product at present sent into the market is destined to survive for an unlimited period. The scientific manufacturer must be brought to see that present success is no guarantee for future stability, and unless he realises this position in its fullest significance, he may find the sale of his standard products gradually falling off, or be compelled to wake up to the unpleasant fact that his competitors are underselling him, owing to improved methods of manufacture.

It may appear to many that I am here simply preaching the doctrine of progress, and that the remarks which I have offered are mere truisms. Unfortunately, the facts of the case render this appeal necessary. It must never be forgotten that the coal-tar colour industry is essentially of English origin. It was Faraday who first discovered benzene in 1825; it was Mansfield who, in 1847, first isolated this substance in large quantities from coal-tar, and showed how nitro-benzene could be manufactured therefrom. The beginning of the colour industry was Perkin's discovery of mauve; and the introduction of the new colour into dyeing establishments was due to the example set by Messrs. Pullar, of Perth, in 1856. The manufacture of magenta on a large scale was the result of the discovery of the arsenic acid process by Medlock and Nicholson; and the phenylic blues were made commercially valuable by Nicholson. The first azo-colours, "aniline yellow" and "Manchester brown," as well as "Manchester yellow" (dinitro-α-naphthol) were manufactured in this country. We may thus fairly lay claim to have given to the commercial world the types of all the more important colouring-matters of the present time. If, as is certainly the case, the development of these typical products has been allowed to take place in other countries, it behoves us, as a practical nation, to inquire closely into the cause of this success abroad—a success which will appear all the more remarkable when we bear in mind that we are the largest European producers of the raw material, gas-tar, out of which the colours are manufactured,

as well as being among the largest consumers of the dyes themselves. It is estimated that the amount of tar distilled annually in this country is about 500,000 tons, and it is certain that we distil at least one-half of the whole amount of tar produced in Europe. The present state of affairs is that our competitors can afford to import the raw materials from us, to manufacture and return the colours so as to compete with us successfully in our own markets, and to undersell us in the foreign markets. The bare mention of these facts will be sufficient to indicate the existence of something requiring radical reform in our manufacturing system.

Before submitting to you the statistics of this industry which I have been able to collect, I think it desirable to make an attempt to show the inner mechanism by which chemical science has been and is being so successfully adjusted to commercial wants by our Continental neighbours. I regret exceedingly that my predecessors on this and other platforms have not left me the chance of giving a general sketch of the chemical development of the different groups of colouring-matters. In fact, I find myself suffering here from several distinct disadvantages, but I hope, with your forbearance, to make the best of the situation. It will serve my purpose equally well, or perhaps even better, to confine my illustration to one particular group of colouring-matters. The more striking achievements, such as the syntheses of alizarin and indigo, are now so familiar to chemical audiences that their repetition would be unnecessary. Equally instructive, from the present point of view would be the history of the colouring-matters of the rosaniline group, and I can only express a passing regret that time will not permit me to recapitulate the steps in the beautiful series of investigations which led to the establishment of the structural formula of rosaniline and its derivatives by E. and O. Fischer, and then to the synthesis of these colours by Caro from ketone bases. The principle which I wish to bring out may seem a strange one to a "practical" people, but I am convinced that the whole secret of success abroad is the spirit of complete indifference to immediately successful results in which the researches are carried on. I say "immediately successful" because it would of course be absurd on the part of an investigator not to take advantage of any discovery which happened to be of commercial value. But, as a general principle, the question of practical utility does not in the first place enter into the work. The great development of this and many other industries is mainly due to the complete and thorough recognition, on the part of our competitors, of the vital importance of chemical science. In this country, where the word "practical" threatens to become a reproach, we put science into the background, and attach all importance to the mere *technique* of our manufactures. If I might venture to offer an aphorism to the English manufacturer, it would be to the effect that he should look after the science, and leave the *technique* to take care of itself.

After these considerations, you will see that it is a matter of perfect indifference whether I take by way of illustration products which have been successful from a financial point of view or not. In order to give greater emphasis to the principle, I propose, however, to consider the history of some colouring-matters which have found a market value, and I select this group with the more readiness because, on the one hand, it was not treated of last year by Dr. Perkin, and, on the other hand, it furnishes a splendid illustration of the way in which these coal-tar products are being scientifically developed in the foreign laboratories.

In 1863, Mr. E. C. Nicholson discovered a basic orange colouring-matter among the by-products formed during the manufacture of magenta by the arsenic acid process. The method of isolating this substance in a state of purity was very skilfully worked out by Messrs. Simpson, Maule, and Nicholson, and the colour was introduced into the market under the name of "phosphine." This dye was the first basic orange discovered, and the advantages which it possessed for certain kinds of dyeing enabled the manufacturers to sell it at a price which helped to cheapen the cost price of magenta to an appreciable extent. The chemical composition of the substance was established in 1863 by Hofmann, who assigned the formula $C_{20}H_{17}N_3 . H_2O$, and described the base under the name of chrysaniline. Although other and cheaper basic orange colouring-matters have since been discovered, chrysaniline still finds a distinct use; and I am informed by Messrs. Brooke, Simpson, and Spiller that the amount of this colour now sold is not appreciably less than at the time of its introduction by their predecessors. The chemical constitution of chrysaniline remained unknown till about two years ago, when the problem was solved by O. Fischer (*Berichte*, 1884, p. 203). In order to be able to follow the steps in the investigation, it will be necessary, in the first place, to go back to the discovery of another colouring-matter, called flavaniline, of which the existence was made known by O. Fischer and C. Rudolph in 1882 (*Berichte*, 1882, p. 1500). Flavaniline was produced by the action of dehydrating agents, such as zinc chloride, upon acetanilide, this fact having been observed by Rudolph in 1881, and the practical manufacture of the colour having been carried on under a patent by Messrs. Meister, Lucius, and Brüning, of the Hoechst colour-works.[1] Supplied with a large quantity of the pure crystalline material by the manufacturers, Messrs. Fischer and Rudolph established the formula of flavaniline, $C_{16}H_{14}N_2$, and showed that its formation from acetanilide might be expressed by the equation :—

$$2C_6H_5 . NH . C_2H_3O - 2OH_2 = C_{16}H_{14}N_2.$$

By the action of nitrous acid upon flavaniline a diazo-compound was produced which, by the usual method of decomposition by water, gave a phenolic derivative termed flavenol, and possessing the formula $C_{16}H_{12}N . OH$, thus proving that flavaniline contained a displaceable NH_2 group. By heating flavenol with zinc dust, a base was obtained having the formula $C_{16}H_{13}N$, and termed flavoline. This base had an odour resembling that of quinoline, and all its properties suggested to the authors that flavaniline was in reality a quinoline derivative. That flavaniline was amido flavoline was proved by nitrating the latter base, and reducing the nitro-compound, when flavaniline was obtained. In a later publication by Besthorn and Fischer (*Berichte*, 1883, p. 68) it was announced that flavenol, when oxidised by potassium permanganate in an alkaline solution, gave an acid which, on distilling with lime, furnished a base having all the characters of lepidine. By the continued oxidation of flavenol with excess of alkaline permanganate, another acid was obtained, which proved to be picoline-tricarbonic acid, and the latter, on further oxidation, gave picoline-tetracarbonic acid (*Berichte*, 1884, p. 2925).

So much for the facts; now for their interpretation. The production of flavenol from flavaniline by the diazo-reaction shows that the respective formulas of these substances are :—

$$C_{16}H_{12}(NH_2)N \qquad C_{16}H_{12}(OH)N.$$

Flavenol gave, as the first product of oxidation, lepidine-carbonic acid, of which the formula is $C_{10}H_8N(CO_2H)$, and by further oxidation it gave picoline-tricarbonic acid, of which the formula is $C_6H_4N(CO_2H)_3$. Now the C-atoms oxidised by the breaking down of the 16-carbon atom flavenol into 11-carbon atom lepidine-carbonic acid, are those C-atoms which in flavenol are associated with the hydroxyl group, because this group is no longer contained in the product of oxidation. Thus the formulas of flavaniline, flavenol, and flavoline are better expressed as :—

$$C_{10}H_8N . C_6H_4(NH_2)$$
$$C_{10}H_8N . C_6H_4(OH)$$
$$C_{10}H_8N . C_6H_5.$$

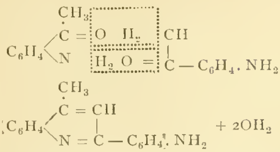
From this it appears that flavaniline is amidophenyl-lepidine, flavenol hydroxyphenyl-lepidine, and that flavoline is phenyl-lepidine.

The central nucleus of flavaniline having thus been shown to be lepidine (which is methylquinoline), the next question to be settled was the mode of formation of the colour base from acetanilide. The authors suggest that at the high temperature of the reaction, acetanilide, in the first place, becomes transformed into the isomeric orthoamidoacetophenone :—

$$\underset{\text{Acetanilide.}}{\begin{array}{c}\mathrm{H}\quad\mathrm{CH_3}\\ \mathrm{C_6H_5 . N . C : O}\end{array}} \qquad \underset{\text{Amidoacetophenone.}}{\begin{array}{c}\mathrm{CH_3}\\ \mathrm{C_6H_4}\left\langle\begin{array}{l}\mathrm{C : O}\\ \mathrm{NH_2}\end{array}\right.\end{array}}$$

By the condensation of two molecules of the amidoacetophenone with the elimination of two molecules of water, flavaniline would be produced in a manner analogous to the formation of mesitylene by the condensation of three molecules of acetone under the influence of dehydrating agents :—

[1] I am indebted to this firm for having kindly supplied me with specimens of these products for exhibition.



The accuracy of this suggestion was verified by showing that orthoamidoacetophenone is present in small quantity when the reaction is arrested as soon as the formation of colouring-matter commences; and conversely, when pure orthoamidoacetophenone was heated with zinc chloride, flavaniline was produced in small quantity.[1]

We may be permitted to pause at this stage of the investigation, before proceeding to consider the connection of this work with the constitution of chrysaniline. These results cannot but be regarded by chemists as a very beautiful piece of investigation; but the person of a "practical" turn of mind may possibly want to know what bearing they have upon the question of market value—the question which the manufacturer but too frequently considers as the only one of importance. Now it is the essence of chemical science—as indeed of all other sciences—that every discovered fact is related to other groups of facts, and although the relationship may not at once be apparent, it is only a matter of further development that is necessary in order to reveal relationships which are at present obscure on account of our imperfect knowledge. Thus, the policy of looking at a chemical product from the narrow point of view of immediate utility is not only unscientific, but it is detrimental to the interests of the manufacturer himself. Every new compound or process discovered—every structural formula established by legitimate investigation, may have an enormous influence, directly or indirectly, upon the market value of products at present sent into commerce. Our manufacturers must realise this if they wish to recover their position in the coal-tar industry, or in fact in any other chemical industry. There is no branch of manufacture so perfect as not to be open to further improvement, and until the broad spirit of scientific development is made to replace the suicidal policy of immediate utility, our position as a manufacturing nation is not likely to be improved.

In order to justify this digression by the particular instance now under consideration, we must return to the work of Messrs. Fischer and Besthorn. The discovery that flavaniline was a quinoline derivative was of importance as a principle, quite apart from any immediate value attaching to the dye-stuff itself. Up to the time of this discovery, the quinoline derivatives, with the exception of alizarine blue, had been practically of no importance in the tinctorial industries, but as a consequence of the present investigation, the question at once suggested itself whether the analogous bases of high boiling-point, which are present in coal-tar, such, for example, as acridine, might not be utilised as sources of colouring-matters. I may remind you that the fact of quinoline being an aromatic compound was first established by the researches of our Chairman this evening, Prof. Dewar, who obtained aniline from this base. In a subsequent paper on chrysaniline (O. Fischer and G. Körner, *Berichte*, 1884, p. 203), it was pointed out that in the course of his investigations upon rosaniline Fischer had observed that the former base, like rosaniline, was capable of furnishing a diazo-compound. An observation made by Claus is also mentioned, viz. the conversion of chrysaniline into a phenol (chrysophenol) by heating to a high temperature with hydrochloric acid in accordance with the equation:—

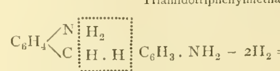
$$\underset{\text{Chrysaniline hydrochloride.}}{C_{19}H_{15}N_3 . HCl} + H_2O = \underset{\text{Chrysophenol.}}{C_{19}H_{15}N_2O} + NH_4Cl.$$

The investigation of flavaniline appears to have given an impetus to the ideas respecting chrysaniline, because of the general similarity in the properties of these two substances. In confirmation of this impression, it was found that by the oxidation of chrysophenol an acid was obtained which, on distillation with lime, gave a pyridine base. I need hardly remind you that picoline, which was obtained from the acid resulting from the extreme oxidation of flavenol, is methylpyridine. It was thus established that chrysaniline was a derivative of a quinoline base.

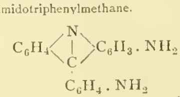
The next step in the investigation is a very important one. By decomposing the diazo-compound of chrysaniline with alcohol according to the Griess reaction, phenylacridine was obtained. Acridine is a base belonging to the quinoline series, having the formula $C_{13}H_9N$. It was discovered by Graebe and Caro in 1872 in crude anthracene. Phenylacridine accordingly possesses the formula $C_{13}H_8N . C_6H_5$; and chrysaniline appears as diamidophenylacridine, $C_{13}H_7(NH_2)N . C_6H_4(NH_2)$, because two amido groups are replaced by H by the diazo reaction. Thus the formula $C_{20}H_{17}N_3$ (first assigned by Hofmann to chrysaniline) is really the formula of the higher homologue, chrysotoluidine.

In order to explain the formation of chrysaniline during the oxidation of the materials (aniline and toluidine) in the "red melt" still, several suggestions were put forward, of which the most probable appeared to be that the base was derived from triamidotriphenylmethane, the latter compound resulting from the condensation of two molecules of aniline with one of orthotoluidine:—

$$C_6H_4\begin{cases}NH_2\\CH_3\end{cases} + 2C_6H_5 . NH_2 - 2H_2 = \underset{\text{Triamidotriphenylmethane.}}{HC(C_6H_4NH_2)_3}$$



Triamidotriphenylmethane.



Diamidophenylacridine = Chrysaniline.

The relationship of chrysaniline to the colouring-matters of the rosaniline group is thus indicated; but, tempting as is this theme, time will not admit of further digression into this field. The main point, so far as we are at present concerned, is that by means of the present investigation, we have now arrived at a knowledge of the parent substance, acridine, of which a colouring-matter more than twenty years old proves to be a derivative. By such results new fields of investigation are opened up, and direct methods for the production of chrysaniline suggest themselves. Even the practical requirements would be satisfied if it could be shown that the colour could be manufactured cheaply by a direct synthesis, instead of depending, as heretofore, upon the small and capricious secondary product of the magenta manufacture. As a matter of fact several syntheses of chrysaniline have been effected, one of which forms the subject of a patent (German Patent, 29142, April 1884) by Messrs. Ewer and Pick, of Berlin. Into the mode of preparation by this patented process I cannot now enter any further than by merely stating that nitrodiphenylamine and nitrobenzoylchloride form the starting-points, and that the specification bears the title—"Preparation of chrysaniline and other colouring-matters of the phenylacridine group." If an elaborate scientific investigation culminates in a patent, its utility will, I know, be conceded by many for whom the work would otherwise have possessed no particular interest.

The illustration which I have given is a typical example of the kind of scientific development which is being carried on by our chemical colleagues abroad, and which is being taken advantage of in the Continental factories. I do not wish to give you the impression that the particular colouring-matters dealt with are of supreme importance industrially—they are of considerable importance, but the modern history of any other colouring-matters would have been equally instructive. The beautiful researches of Bernthsen upon the constitution of methylene blue, would have done equally well had time permitted of my making use of them. It seemed to me more appropriate to this Section of the Society of Arts to give a somewhat detailed account of one particular series of investigations, rather than to take a skim over the mere surface of the enormous field which the coal-tar

[1] Just after writing this paper, a new contribution to the chemistry of flavaniline was published by O. Fischer (*Berichte*, 1886, p. 1036), from which it appears that the condensation is really between one molecule of *ortho-* and one molecule of *para*-amidoacetophenone, the latter being produced by the isomeric transformation of the ortho-compound at the high temperature employed. The constitution of flavaniline is thus definitely established as para-amidophenyl-γ-lepidine.

colour industry now offers. The case considered at any rate presents the advantage of not being too hackneyed, and this will be sufficient excuse for having made use of it.

It was stated at the commencement of this paper that there is reason to believe that our supremacy in the coal-tar colour industry has, for some years, been declining, and I have further expressed my belief that the chief cause of this falling off is the subordinate position given to chemical science in this country as compared with the status of this science abroad. Whether this explanation be accepted or not, the fact of the decadence of the manufacture remains, and I am in a position to bring this unpleasant truth home to our countrymen by a strong body of evidence. It must be borne in mind that the decline of any industry cannot be measured by the absolute weight of the products turned out annually, because the demand for the products in question may be on the increase, and we may be actually producing a greater weight of colours now than we were during our most successful period. The whole question is a relative one—it is simply how much material are we now turning out as compared with the amount produced by our competitors—what proportion of coal-tar products do we supply for our own and foreign consumption? In order to answer this question with some approach to numerical exactness, it occurred to me that the most trustworthy information could be obtained from the consumers themselves; and through the kindness of Mr. Robert Pullar, of Perth, and Mr. Ernest Hickson, of Bradford, I have been enabled to put myself into communication with several of the representative dyeing and printing establishments of this country. The facts obtained, as showing the actual state of the industry at the present time, appear to me of sufficient interest to be given here in some detail. I may take the present opportunity of stating that my application for statistical information has been most courteously responded to by the various firms, to whom I have great pleasure in returning my thanks.

Edward Ripley and Son, of Bradford, perhaps the largest dyers of piece-goods in the kingdom, inform me that during the year 1885 they used 86½ per cent. of foreign coal-tar colours, and 13½ per cent. of English make.

Walter Walker and Son, of Dewsbury, dyers of wool for rugs, mats, carpet yarn, and blanket stripes, estimate that during 1885 they used 80 per cent. of German dyes. They state that the exact proportion is difficult to estimate, so that the figure given is only approximative. Referring to their larger consumption of foreign colour they state:—"It is very discouraging to have to do this and send the trade out of our country, but to our own interest and advantage we have to do it."

John Newton, silk dyer, Macclesfield. Mr. Walter Newton, F.C.S., informs me that during 1885 they used 80 per cent. of foreign colour. He adds:—"The rapid advancement in the improved manufacture of some of these dyes by the Germans is the only cause of our desertion from the English colour-manufacturer."

G. W. Oldham, silk dyer, of Netherton, near Huddersfield, informs me that during 1885 he used 2000 lbs. of German dyes, 1100 lbs. of English dyes, and 800 lbs. of doubtful origin.

James Templeton and Co., of Glasgow, state that they dye as much as 30,000 lbs. of yarn (chiefly worsted) weekly, but they use only a small proportion of coal-tar dyes, all of which are of German manufacture.

Messrs. Leckie and MacGregor, of Paisley, inform me that in the west of Scotland, including Glasgow and Paisley, they are certain that at least 90 per cent. of the dyes used come from the Continent. Their own consumption of English colour only reached 6·8 per cent.

Alexander Harvey and Son, of Glasgow, yarn dyers, state that during 1885 they used 60 per cent. of German and 40 per cent. of English dyes. These figures do not include alizarin, of which they state that they used about equal quantities of German and English make. The English supply is chiefly made up of one article, "aniline salt." They add:—"We find the German makes in general of better value than the British, as our rule is, *cæteris paribus*, to give the home-make the preference."

Messrs. Manson and Henry, Glasgow, yarn dyers, state that they use only German dyes, adding that they find it to their advantage "for both cheapness and quality."

Among the largest consumers of coal-tar colours in this country are the jute dyers. As representing this department of the tinctorial industry, Messrs. James Stevenson, of Dundee, inform me that during 1885 they used only 7·7 per cent. of English colour. They have been good enough to supply the following analysis of their consumption:—

Scarlet	37 per cent.,	of which nothing	is English.
Crimson	16	,, 6·4	,,
Blues	11·5	,, nothing	,,
Oranges	11	,, ·05	,,
Greens	7	,, nothing	,,
Magenta (residues)	6·5	,, ,,	,,
Maroon	5·5	,, ,,	,,
Pink	2·75	,, ,,	,,
Brown	1·25	,, 1·25	,,
Violet	1	,, nothing	,,
Various	0·5	,, ,,	,,
	100·0	7·7	

Messrs. Cox Bros., of the Camperdown Jute Works, Lochee, state that practically the whole of the "aniline" colours used by them are of Continental manufacture.

With reference to the calico printers, the following facts have been collected:—

Messrs. Z. Heys and Sons, of Barrhead, state that during 1885 they used over 10,000 lbs. weight of colours (exclusive of alizarin), of which 700 lbs. only were of English make.

Messrs. James Black and Co., of Bonhill, Dumbartonshire, state that in their belief more than one-half of the colour used by calico printers is of foreign manufacture.

In the course of the present inquiry, it seemed desirable to obtain information concerning the consumption of alizarin, with reference to which the following statements have been received:—

Messrs. Walter Crum and Co., of Thornliebank, Glasgow, are of opinion that "the great bulk of what is used in this country is manufactured in Germany." They do not profess to be able to give actual figures having any approach to accuracy.

Mr. John Christie, of the Alexandria Turkey-Red Works, Dumbartonshire (John Orr, Ewing, and Co., states that they use only artificial alizarin in their establishment, their consumption being considerably over 2,000,000 lbs. weight of 10 per cent. paste annually. Their consumption was, in—

1880	... 98 per cent. German	... 2 per cent. English	
1881	... 99 ,, ,,	... 1 ,,	,,
1882	... 100 ,, ,,	... 0 ,,	,,
1883	... 77 ,, ,,	... 23 ,,	,,
1884	... 56 ,, ,,	... 44 ,,	,,
1885	... 47 ,, ,,	... 53 ,,	,,

Messrs. William Stirling and Sons, of Glasgow, state that their relative consumption of English and German alizarin for Turkey-red dyeing varies so much from year to year that they have no means of directly supplying useful data. This firm has, however, been good enough to make inquiries for me from a competent authority, who has furnished the following report:—

"In 1883 and 1884 I estimate that the sales in the United Kingdom amounted to a monthly average of about 530 tons, 10 per cent. (say 6360 tons, 10 per cent. per annum). Of this quantity I estimate about 30 33 per cent. was manufactured in this country. Taking 1884 alone, the figures are estimated at 566 tons, 10 per cent. per month (say 6800 tons, 10 per cent. per annum). Proportion manufactured in Great Britain, say about 30-35 per cent. In 1886 the consumption may be estimated at 550-600 tons, 10 per cent. per month (say 6900 tons, 10 per cent. per annum). Proportion manufactured in this country probably now very considerably more than 35 per cent."

This estimate of the total consumption (550-600 tons, 10 per cent. per month) is confirmed by my friend Mr. Thomas Royle, F.C.S., of the British Alizarin Company's works at Silvertown, but he is of opinion that 50 per cent. of this is of English manufacture.

By way of further confirmation, it appeared to me to be desirable to get the opinion of manufacturers themselves, and although this has been a matter of considerable difficulty, I am able to give some kind of an estimate. Mr. Ivan Levinstein, of Manchester, estimates that Germany produces:—

Colours derived from benzene and toluene, six times more than England.

Colours derived from naphthalene, seven times more than England.

Colours derived from anthracene, five times more than England.

The average production of Germany is thus about six times that of this country. Mr. W. A. Mitchell, of the firm of W. C. Barnes and Co., Phœnix Works, Hackney Wick, informs

me that of some 159 tons of "aniline" dyes which passed through their hands as agents last year, 95 per cent. were of Continental make. With reference to the two chief raw materials, benzene and aniline, this same firm estimates that about 75 per cent. of the whole quantity of these products made in England goes to the Continent.[1]

The facts and figures which I have now laid before you must be left to tell their own story—time will not permit me to attempt any analysis of them. The evidence collected will at any rate give a much more forcible idea of the true state of the coal-tar colour industry in this country than has hitherto been attempted, and if this evidence goes against us as a manufacturing nation, it is all the more desirable that our true position should be realised. I find that it is almost impossible to give a correct numerical expression in pounds sterling for the annual value of this industry to the country, as the estimates vary within very wide limits. According to Dr. Perkin, whose opinion on this matter will perhaps carry the greatest weight, the value of the annual output is between 3,000,000*l.* and 4,000,000*l.* That the industry is one of considerable importance on the Continent may be gathered from the official returns relating to the German exports. For the following figures I am indebted to Dr. H. Caro, of the "Badische Anilin und Soda Fabrik," Ludwigshafen on Rhine:—

Exported from Germany, from January 1 *to December* 31, 1885

Alizarin paste (? per cent.)	4283	tons
Aniline and intermediate products ...	1713	,,
Aniline, &c., colours	4645	,,

Dr. Caro adds that it is generally believed that about four-fifths of the entire German production are exported.

The magnitude of this branch of chemical industry abroad will be gathered from the fact that a German factory of about the third magnitude consumes at the present time between 500 and 600 tons of aniline annually. According to information recently furnished to me from the two largest of the German factories, the Badische Company employ 2500 working men and officials, and the Hoechst Colour Works (formerly Meister, Lucius, and Brüning) 1600 working men and fifty-four chemists. It must, of course, be borne in mind that in these factories the products are not "aniline" colours only, but alizarin, acids, alkalies, and all chemicals required in this branch of manufacture.

The industry which has been selected for this evening's topic is thus not only an important one in itself, but for us, as chemists, its development is fraught with meaning both scientifically and educationally. In taking up this subject it has not been my desire to exalt the coal-tar colour industry to a position of undue importance, nor do I wish it to be inferred that the remarks which I have made concerning its decadence, or at any rate stagnation, in this country are applicable to this manufacture only. The failure on our part to grasp the true spirit of chemical science in its relation to our manufactures makes itself felt in every industry in which chemistry is concerned. The strength of our competitors is in their laboratories, and not, as here, upon the exchanges. It is only by showing up our weakness in each industry that the state of affairs can be remedied, and our prestige as a manufacturing country restored. If each specialist would do for his industry what I have here attempted to do broadly for the coal-tar colour industry, we should get together a body of evidence which the Royal Commissioners on the depression of trade would do well to take into consideration. We have heard a great deal of late years about the subject of technical education, but the talk has been rather one-sided. We have had utterances from those who, recognising the enormous importance of this subject to the country, have munificently endowed those institutions for the promotion of technical education which are springing up around us; we have had all kinds of schemes from those who are taking upon themselves the duties of technical educators, but it appears to me that we have not heard with sufficient distinctness the voices of those who may be presumed to suffer most from the want of technical education, viz. the manufacturers themselves. I have heard rumours of the existence of a certain class of manufacturer—let us hope a rare species—who declares that science is no use to him, and that he can get along better without it. I must confess that I never met this individual in the flesh, but I know that he exists in some of our manufacturing centres. As a species he is, however, doomed to extinction in the struggle with his competitors, and we may consider him out of court in the discussion of schemes of technical education. It is now generally admitted that the days of empiricism have passed away, and most manufacturers admit that present success and future development depend upon a proper recognition of technical, *i.e.* of applied science. But unless the manufacturers themselves speak loudly on this question, the voices of those who wish to promote scientific education may be drowned by the clamour of mere theorists.

In no other department of our manufactures is the want of technical science more felt than in the chemical industries. We not only see this in the greater development of these industries abroad, but in some of our most successful factories here—and this applies more especially to the coal-tar colour industry—foreign chemists are employed, and as I have lately been informed by a well-known manufacturer, it is even impossible to get the necessary plant properly made in this country. There is no doubt that the recondite character of the truths of chemical science, as compared with the more obvious truths of mechanics and physics, has much to do with the want of popularity of this branch of knowledge, and is responsible for the circumstance that our science is regarded with comparative indifference until some branch of manufacture is *in extremis.* In our national characteristic of being "practical," we are apt to become short-sighted in our manufacturing policy, and to recognise only actualities, to the exclusion of the potentiality conferred upon a nation by a broader scientific culture.

In conclusion, I have to express my thanks to Messrs. Brooke, Simpson, and Spiller; Messrs. Burt, Bolton, and Haywood; and to the British Alizarin Company for the fine series of specimens now exhibited. For the beautiful specimens illustrating the Continental manufacture, I am especially indebted to the Badische Anilin and Soda Fabrik, of Ludwigshafen on Rhine, and to the Hoechst Colour Works. The series of patterns dyed with known weights of fifty distinct coal-tar colours were prepared by Mr. Ivan Levinstein for the lecture recently delivered at the Royal Institution by Sir Henry Roscoe, to whom I am indebted for being able to show them on the present occasion.

[1] According to a later estimate, kindly supplied by Mr. Ivan Levinstein, the quantity of benzene and toluene used in this country amounts to about 500,000 gallons, and that used in Germany to about 2,000,000 gallons annually. About half the English production is, however, exported as aniline, toluidine, and aniline salt, while Germany converts into colouring-matters at least 1,600,000 gallons of these hydrocarbons.

DRYING UP OF SIBERIAN LAKES

THE rapid drying up of lakes in the Aral-Caspian depression, in so far as it appears from surveys made during the last hundred years, is the subject of a very interesting and important paper contributed by M. Yadrintseff to the last issue of the *Izvestia* of the St. Petersburg Geographical Society (vol. xxii. fasc. 1). Two maps, which will be most welcome to physical geographers, accompany the paper. One of them represents the group of lakes Sumy, Abyshkan, Moloki, and Tchany, in the Governments of Tobolsk and Tomsk, according to a survey made in 1784. The other represents the same lakes according to three different surveys made during our century, in 1813 to 1820, in 1850 to 1860, and finally in 1880, and it shows thus the rapid progress of drying up of these lakes. There are also earlier maps of Lake Tchany, which represent it as having very many islands (Pallas estimated their number at seventy), but they are not reliable. As to the map of 1784, no cartographer, accustomed to distinguish "nature-true" maps from fancy ones, would hesitate in recognising it as quite reliable as to its general features. It is also fully confirmed by the ulterior detailed surveys dating from the beginning of our century. It appears from this series of four maps, dating from different periods, that the drying up has gone on at a speed which will surely appear astonishing to geographers. The group of lakes consisted of three large lakes—Sumy, Abyshkan, and Tchany, with a smaller lake, Moloki, between the two latter. Lake Tchany (the largest of the three) has much diminished in size, especially in its eastern and southern parts; but the greatest changes have gone on in the other lakes. Whole villages have grown on the site formerly occupied by Lake Moloki, which had a length of twenty miles at the end of last century, and now is hardly three miles wide. Of Lake Abyshkan, which had a length of forty miles from north to south, and a width of seventeen miles in the earlier years of this century, and whose surface was estimated at 530 square miles,

only three small ponds have remained, the largest of them being hardly one mile and a half wide. The drying up has been going on with remarkable rapidity. Even twenty-five years ago there were several lakes ten and eight miles long and wide, where there are now but little ponds. Lake Tchebakly, which was represented in 1784 as an oval forty miles long and thirty miles wide, has an elongated irregular shape on the map of the beginning of our century; it measures, however, still forty miles in length, and its width varies from seven to twenty miles; while several small lakes to the east of it show its former extension. Thirty years later we find in the same place but a few small lakes, the largest of which hardly has a length and width of three miles; and now, three small ponds, the largest of them having a width of less than two miles, are all that remain of a lake which covered about 350 square miles a hundred years ago. The same process is going on throughout the lakes of West Siberia, and throughout the Aral-Caspian depression. No geologist doubted upon, but we cannot but heartily thank M. Yadrintseff for having published documents which permit to estimate the rapidity of the process. P. K.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

We understand that Mr. Granville Cole has been appointed to the Professorship of Geology at the Bedford College, London, and Miss C. A. Raisin to the Demonstratorship in Botany at the same institution.

SCIENTIFIC SERIALS

The *Journal of Botany*.—The number for July commences with the first part of an important article (illustrated), by Messrs. Roy and Bisset, on Japanese Desmids, chiefly obtained from a lake in the Island of Yesso. The majority of the forms obtained are cosmopolitan, but some of them of great rarity in Europe. There are also some new species. Papers follow on British *Rubi*, on the *Rubi* of Somersetshire, and on the flora of St. Kilda.

American Journal of Science, July.—Memorial of Edward Tuckerman, by Asa Gray. This botanist, who was born in Boston, December 7, 1817, and died March 15, 1886, was distinguished especially in the field of lichenology, to which he devoted most of his life. He was the author of a "Synopsis of the Lichens of New England, the other Northern States, and British America," of "Lichenes Americæ Septentrionalis exsiccati" (3 vols.), and many other papers on this branch of botany, in which he has left behind him no superior.—Notes on American earthquakes (No. 15), by Prof. C. G. Rockwood, jun. This fifteenth paper of the series gives a summary of such information as the author has been able to gather on the earthquakes of North and South America during the year 1885. It tabulates seventy-one shocks, classed according to their intensity as very light, light, moderate, strong, severe, or destructive. Of these as many as thirty-four occurred on the Pacific coast of the United States, where the Bay of San Francisco appears to be a chief centre of seismic disturbance.—Observations on the Tertiary and Grand Gulf of Mississippi, by Dr. Otto Meyer. The author finds no place where Grand Gulf strata overlie the Marine Tertiary, although there are two districts where strata undistinguishable from unquestioned Grand Gulf are overlain by Marine Tertiary. The Grand Gulf is not, generally speaking, a marine formation, for it contains fresh-water shells. In Eastern Mississippi occurs a thick and extended marine greensand formation parallel to the strata immediately below the Claiborne profile. Its fauna is Claibornian, but approaches the Jacksonian.—Notes on the volcanic rocks of the Republic of Salvador, Central America, by Arnold Hague and Joseph P. Iddings. This study is based on specimens gathered by Mr. W. A. Goodyear in the course of his explorations in Salvador. They are of a highly diversified character, ranging from very basic to highly acidic forms, from rocks rich in olivine to others abounding in quartz, and may be classified under the heads of basalt, pyroxene-andesite, hornblende-pyroxene-andesite, hornblende-mica-andesite, dacite, and possibly rhyolite, basalt and dacite being best represented. Nearly all have their counterpart in Nevada, although there occur many varieties in Nevada not found in the limited series from Salvador.—The genus Strephochetus: distribution and species, by Henry M. Seely. Since reporting last year the presence of the fossil sponge, *Strephochetus ocellatus*, at one or two places in Vermont and New York, the author has traced it to many other districts in those States. To the type of the genus, *S. ocellatus*, he now also adds three new species—*S. brainerdi*, *S. atratus*, and *S. richmondensis*.—Preliminary report on the geology of the Cobscook Bay district, Maine, by N. S. Shaler. This paper, published by permission of the Director of the U.S. Geological Survey, gives a portion of the general results of two months' exploring work on the shore-line of Cobscook Bay during the summer of 1884. The fossiliferous strata have a special interest as throwing light on the position of the shore-line in past times. A conglomerate apparently of the Clinton or Niagara age on the west side of South Bay seems to show that the shore in this district was not far away during a portion of the time when the Cobscook series was forming. In the age of the Perry section there is also evidence that the coast was near its present position and that the rocks exposed to erosion were chiefly of the Laurentian epoch.—On the well-sperometer, by Alfred M. Meyer. The instrument here described, with numerous illustrations, has for the last ten years been used by the author in his laboratory for the purpose of measuring the radius of curvature of a lens of any linear aperture.—On some general terms applied to metamorphism and to the porphyritic structure of rocks, by James D. Dana. The three recognised forms of metamorphism are described and characterised as (1) crystallinic; (2) paramorphic; (3) metachemic. A full terminology of porphyritic varieties is given, based in plan on such terms as *orthophyre*, *augitophyre*, &c.

Bulletin de l'Académie Royale de Belgique, May.—On the transparency of platina, by Ed. van Aubel. After ascertaining by experiment that a sheet of cobalt, iron, or nickel obtained by electrolysis on a transparent sheet of silver, is not really transparent, as is now generally assumed, the author here endeavours to settle the question as regards mirrors of platina chemically produced, that is, by a deposit of platina on a sheet of glass, and the transparency of which is admitted by Kundt. Working with a large mirror supplied by Paul Lohmann of Berlin, from whom Kundt also obtained those used by him, M. van Aubel found, by means of spectroscopic observations, that the metal of these mirrors is not really transparent, the light merely filtering through the interstices left between the particles of platina deposited on the surface.—A contribution to the study of the salts of platina, by M. Eugène Prost. The author deals especially with the action of nitric acid and of perchloric acid on platinic hydrate, and with the action of nitric acid on the precipitated bisulphuret of platina, his object being to form the so-called normal platinic nitrates, perchlorates, and sulphates. Failing to obtain these substances, he endeavoured to get double salts of normal composition by combining them with alkaline salts having corresponding acids. The results show that all the compounds thus obtained still correspond with basic platinic salts, so that it would so far appear that a normal platinic nitrate cannot be obtained.—On the unstable equilibrium of the surface-layer of a fluid, by G. van der Mensbrugghe. The absolute instability of surface-layers exposed to the free action of the atmosphere is demonstrated on theoretical grounds. From this theory the author proposes in another paper to deduce the existence of superficial tension on the free surface of a fluid, or on the surface common to two fluids, or to a fluid and solid, thence deriving a rational explanation of the phenomenon of evaporation.—On the heat of the alloys of lead and tin, by W. Spring. Continuing the researches of Ermann, Rudberg, Regnault, Wiedmann, and others, the author seeks to determine for restricted intervals of temperature the total heat of these alloys relatively to that of their constituents. Further light is thus thrown both on the constitution of these bodies, and on the question why their point of fusion is lower than that of their constituents.

Rendiconti del Reale Istituto Lombardo, June.—On some unconscious intervals in a co-ordinate series of psychic acts, by Tito Vignoli. The object of this essay is to ascertain experimentally whether in the co-ordinate exercise, or logical sequence, of thought, it sometimes happens that some of the connecting links of the argument are supplied unconsciously. Several instances are quoted, together with the author's personal experience, showing that this really is the case. It is incidentally argued that, in its complexity, the brain is a large organ of compensation, so that, if any of its parts in which special functions are localised become disturbed or injured, these may, within

certain limits, be replaced by others, immediately if the lesion be slight, gradually if serious.—A contribution to the theory of quadratic forms, by G. Morera.

July.—A case of extraordinary hirsuteness, by Prof. Giovanni Zoja. The author refers briefly to a Spanish girl observed by him at Pavia in 1881, who was above the average height, yet whose hair, when unbound, swept the ground by several centimetres. Some of the tresses measured 180 to 187·3 centimetres.—Meteorological observations made at the Brera Observatory, Milan, during the month of June.

SOCIETIES AND ACADEMIES

Sydney

Royal Society of New South Wales, May 5.—Annual Meeting.—Prof. Liversidge, F.R.S., President, in the chair.—The President stated that 27 new Members had been elected during the year, and the total number on the roll April 30 was 492. The Clarke Medal for the year 1886 had been awarded to Prof. L. G. de Koninck, M.D., of Liége, in recognition of his distinguished scientific attainments, and more particularly of his valuable contributions to our knowledge of the Palæozoic fossils of New South Wales. During the year the Society held eight meetings, at which the following papers were read:—Presidential Address, by H. C. Russell, B.A., F.R.A.S.—Notes on flying-machines, by L. Hargrave.—On a system of accurate measurement by means of long steel ribands, by G. H. Knibbs.—Local variations and vibrations of the earth's surface, by H. C. Russell, B.A., F.R.A.S.—Some causes of the decay of the Australian forests, by Rev. P. MacPherson, M.A.—The history of floods in the Hawkesbury River, by J. P. Josephson, A.M.I.C.E.—The Ringal of the North-Western Himalaya, by Dr. Brandis, F.R.S. (communicated by Baron von F. Müller, K.C.M.G., F.R.S.).—Notes on experiments in mounting the *Amphipleura pellucida* in media having a higher refractive index than Canada balsam, by Dr. W. Morris, F.R.M.S.—Notes on the characters of the Adelong Reefs, by S. H. Cox, F.C.S., F.G.S.—Stone implements of the aborigines of Australia and some other countries, by Rev. P. MacPherson, M.A.—On a form of flying-machine, by L. Hargrave.—On a new form of anemometer, by H. C. Russell, B.A., F.R.A.S.—The Medical Section held eight meetings, at which eighteen papers were read, and the Microscopical Section eight, at which three papers were read. The number of donations received was 1420 volumes and pamphlets, and 310*l*. expended in the purchase of books, &c., for the library. The Society has issued the following list of subjects, with the offer of the Society's bronze medal and a prize of 25*l*. for each of the best researches if of sufficient merit:—Series vi. to be sent in not later than May 1, 1887; (No. 20) on the silver ore deposits of New South Wales; (No. 21) origin and mode of occurrence of gold-bearing veins and of the associated minerals; (No. 22) influence of the Australian climate in producing modifications of diseases; (No. 23) on the Infusoria peculiar to Australia. Series vii., to be sent in not later than May 1, 1888: (No 24) anatomy and life-history of the Echidna and Platypus; (No 25) anatomy and life-history of Mollusca peculiar to Australia; (No. 26) the chemical composition of the products from the so-called Kerosene Shale of New South Wales.—The following Officers and Council were elected for the ensuing year:—President: C. Rolleston, C.M.G.; Vice-Presidents: H. C. Russell, B.A., F.R.A.S.; Dr. Leibius, M.A.; Hon. Treasurer: R. Hunt, F.G.S.; Hon. Secs.: Prof. Liversidge, F.R.S.; F. B. Kyngdon, F.R.M.S.; Council: Hon. Dr. C. K. Mackellar, A.M., M.L.C.; C. Moore, F.L.S.; P. R. Pedley, Dr. J. Ashburton Thompson, C. S. Wilkinson, F.G.S.; Dr. H. G. A. Wright.

Paris

Academy of Sciences, July 26.—M. Jurien de la Gravière, President, in the chair.—On the quantitative analysis of ammonia, by M. Th. Schlœsing. The author's process of analysis, based on distillation in presence of magnesia, having been questioned by M. Berthelot and others, he has made some fresh experiments, here described, which fully confirm the accuracy of the results already obtained.—Observations on the oldest sedimentary groups in the north-west of France, by M. Hébert. The region here dealt with is the northern section of Brittany and Normandy, where the most ancient sedimentary rocks are the clay-slates of Saint-Lô and the widely diffused purple conglomerates. The former, which are quite distinct from the mica-schists, gneiss, and other primitive crystalline schists, form the fundamental feature throughout the department of La Manche, stretching far eastwards into Calvados, and westwards into Brittany. They assume almost everywhere a vertical or nearly vertical disposition, and are remarkably homogeneous, being almost totally destitute of any organic remains. The whole system seems to be posterior to the granitic pudding of Granville.—On the meteorological station of l'Aigoual, by M. F. Perrier. Since the beginning of July this station has been in full activity, and has been furnished by M. Houdaille, of Montpellier, with maxima and minima thermometers, a psychrometer, an evaporometer, and a registering hygrometer. In the neighbourhood other instruments have been fitted up, including Tonnelot and Richard barometers, a large pluviometer, and a Campbell heliograph. Regular observations have already begun to be taken on this peak, which stands at an altitude of 1567 metres above sea-level, on the water-parting between the Atlantic and Mediterranean basins. The present temporary erections will soon be replaced by a solid structure, for which a grant of 4800*l*. has been made by the Minister of Agriculture.—Remarks accompanying the presentation of vol. xii. of the "Mémorial du Dépôt de la Guerre," by Col. F. Perrier. The first part of this volume describes the instruments and apparatus employed in the various geodetic operations connected with the new measurement of the meridian of Paris, with an exposition of the methods of observation. In the second are embodied all the observations taken from 1871 to 1884 between Perpignan and Paris by MM. Perrier, Bassot, and Defforges, at seventy-two stations belonging to the meridian of France.—Note on Gen. Meusnier's projected aërostatic machine, by M. Létonné. The album here referred to is a photographic reproduction of an atlas now in the military aërostatic establishment of Chalais (Meudon), and containing sixteen plates of designs relative to a projected aërostatic machine prepared by Gen. Meusnier between the years 1784 and 1789. Eight tables are added, giving the coefficients of resistance of various substances suited for the construction of this machine.—On the pressure that exists in the contracted section of a gaseous current, by M. Hugoniot. This paper is supplementary to that inserted in the *Comptes rendus* of June 28, showing that the results of M. Hirn's experiments on the flow of gases are in harmony with the laws of hydrodynamics and with the formula of Weisbach or Zeuner, which is a direct consequence of those laws. Some objections raised by M. Hirn himself are here disposed of, and the general conclusion confirmed by fresh argument.—On the velocity of light in the sulphuret of carbon, by M. Gouy. The experiments here described have been carried out with a revolving mirror analogous to that of Foucault, and capable of 800 revolutions per second by means of compressed air. The results correspond with those recently obtained by Mr. Michelson (*American Journal of Science*, and NATURE, March 11 and April 22, 1886).—Note on the construction of an absolute electrometer adapted for the measurement of very high potentials, by MM. E. Bichat and R. Blondlot. By an improvement introduced into the construction of their already described electrometer, the authors have produced an instrument possessing absolute sterility and capable of measuring potentials corresponding to explosive distances of 2·5 centimetres. A model of the apparatus has been constructed by M. D. Gaiffe, of Nancy.—On the slow decomposition of the chlorides in their extended dissolutions, by M. G. Foussereau. Further experiments with the chlorides of aluminium and magnesium, with the double chloride of rhodium and sodium, the bichloride of platina and the sesquichloride of gold show that the recently described phenomenon of decomposition probably extends to a numerous class of chlorides.—On the definition of the coefficient of self-induction in an electro-magnetic system, by M. G. Cabanellas.—On the numerical laws of chemical equilibria, by M. H. Le Chatelier. The formula for the numerical law of the chemical equilibrium of a gaseous system,

$$\log p^n p'^{n'} \ldots p''^{-n''} \ldots - \frac{273}{0{\cdot}542}\int \frac{Q}{T^2} dT = \text{const.},$$

announced by the author in the *Comptes rendus* for November 16 and December 28, 1885, is here established by rigorous demonstration.—Fresh experiments on the decomposition of hydrofluoric acid by an electric current, by M. H. Moissan. These experiments show conclusively that the gas separated by electro

lysis from anhydrous hydrofluoric acid or from the hydrofluorate of fluoride is fluor, as already anticipated.—On the separation of antimony from tin, by M. Ad. Carnot. This difficult process has been successfully accomplished by a method analogous to that already employed by the author for the separation of zinc and cadmium. It is based on the simultaneous employment of oxalic acid and of the hyposulphide of soda.—On the manganites of soda, by M. G. Rousseau.—On the determination of the absolute acidity of the fluids present in the organism, and on some phenomena connected with the saturation of orthophosphoric acid, by M. Ch. Blarez. From the experiments here described it is inferred that the exact determination of the absolute basic property of phosphoric acid is impossible, there being nothing absolute in this property itself; also that it is impossible to determine the absolute acidity of the animal fluids, of whose constituent principles phosphoric acid and the phosphates form part.—On some thermic data relating to the chromates, by M. Paul Sabatier.—Thermic researches on the seleniures, by M. Charles Fabre. The author here deals with the heat of formation of the seleniure of dissolved ammonium, and with some problems connected with the seleniures of lithium.—Researches on some crystallised basic sulphates, by M. Athanasesco. By employing the process used by Friedel for the artificial reproduction of brochantite, the author has succeeded in obtaining fine crystallised subsulphates of cadmium, zinc, alumina, iron, and uranium. By a slightly modified process he afterwards obtained some subsulphates of nickel, cobalt, mercury, and bismuth.—Researches on some crystallised arseniates, by M. Coloriano All these arseniates, except the bibasic, are insoluble in water, and resist the acids. They were obtained by the various processes of Debray, Friedel and Sarasin, Verneuil and Bourgeois.—On a nitrated camphor and its saline and alcoholic combinations, by M. P. Cazeneuve.—Discussion on the reactions of pilocarpine, by MM. E. Hardy and G. Calmels.—Physiological function of the pulmonary tissue in the exhalation of carbonic acid, by M. L. Garnier.—On a universal chromatometer, by M. L. Andrieu (de l'Etang). The apparatus here described and illustrated is intended to define and measure the colours of liquids by giving them numerical expression.—On the Anguillules of smut, by M. G. Pennetier. From his recent experiments the author concludes that these parasites preserve the vital spark for a period of fourteen years, but no longer.—On the milky secretion of pigeons during incubation, by MM. Charbonnel-Salle and Phisalix.—Researches on the structure of the brain of the Myriapods, by M. G. Saint-Remy.—Researches on the Miocene vegetation of Brittany, by M. Louis Crié.—On the picturesque group of rocks collectively known by the name of Montpellier-le-Vieux (Aveyron), by M. E. A. Martel.

BERLIN

Physiological Society, July 2.—Dr. Joseph reported on the results of experiments instituted with a view to ascertaining the influence of the nerves on the skin. Following up the experiments of Waller, he had excised a somewhat large piece of the second cervical nerve peripherically from the ganglion, and a few days thereafter had observed behind the ear of the side operated upon a perfectly circumscribed place on which the hairs had fallen out, but which, beyond the baldness, showed no symptom of change. The cutting through of the posterior root of the cervical nerve had not the same effect, but the extirpation of the second cervical ganglion had that result. The microscopical examination of the hairless spots showed absence of hair papillæ and of the hair root, while the other constituents of the skin remained unchanged. No abnormal vascularisation of the spots in question nor of the ears generally was observed. Seeing that the protected situation of the depillated spot and the presence of sensibility went to disprove the idea of a mechanical removal of the hairs, while the result of the anatomical examination attested that the hairs were exclusively affected without the vascular system having undergone any essential alteration; the speaker was therefore of opinion that the results of his investigations might be taken as demonstration of the existence of trophical nerves. After the separation of the peripherical nerve no change in the ganglion was ever observed, whereas the dissection of the posterior root gave rise to atrophy.—Dr. H. Virchow next demonstrated four drawings representing the incurvations of the vertebrate columns of different human types and manifesting the surprisingly great differences obtaining among the normal vertebrate columns of a Russian, an Italian, a male German existing in model, and a pregnant German wife. From these figures it was to be concluded that the breadth of the normal fluctuations of these incurvations was great. On the study of the vertebral column it was to be observed that it was indeed comparable to an elastic rod which became expanded by lateral pressure, and compressed by pressure from the top, but that it was composed of several parts independent of one another (the lumbar, pectoral, and cervical parts) which were adapted to the special functions of the respective sections of the rump. By a simple model he made these differences among the different sections apparent. On a second model he showed that very marked displacements in the centre of gravity at the uppermost part of the body were equalised, not by compensatory incurvations, but by bendings in the undermost joints. The speaker then demonstrated by curves rendered in paste the incurvations shown by the vertebral column of dead bodies when the ligaments of the vertebrate bodies in front or behind, in the dorsal or lumbar vertebræ, were cut through. The curves became more marked after the elastic ligaments of the upper vertebræ were cut through, but they hardly changed at all when the lowest tendinous ligaments were cut through. The intesticular ligaments consisted of a soft elastic kernel and of distended ligamentous fibres compressing the kernel. The action of the expending kernels, which made the vertebral column firmer, was illustrated by a third model. The speaker had taken exact measure of the situation of the kernel in each disk on vertebrate columns sawn through, and when he combined together these points on a drawing, he obtained a more marked incurvation than that possessed by the vertebral column. Thus altogether apart from the muscular activity, the different forces acted on the incurvation of the vertebrate column, which for the rest appeared to be different in the different races.

BOOKS AND PAMPHLETS RECEIVED

"Indigenous Fodder Grasses of North-West India" (Roorkee).—"Die Ergebnisse der Untersuchungsfahrten, S. M. Knbt." "Drache" (Mittler und Sohn, Berlin).—"Russland," by Von Waldeck (Freytag).—"Der Ozean," by Dr. O. Krümmell (Freytag).—"Die Schweiz," by Dr. Z. Z. Egli (Freytag).—"Vital Statistics of the City of Glasgow," by Dr. J. B. Russell (Macdougall).—"Beiträge zur Biologie der Pflanzen," Vierter Band, Zweiter Heft (Kern, Breslau).—"Transactions of Vasser Brothers' Institute and its Scientific Section," vol. iii. part 1.—"Partiality in Unity" (Wyman).—"Bulletin of the U.S. Geological Survey," Nos. 24, 25, 26 (Washington).—"Speculations from Political Economy," by C. B. Clarke (Macmillan).—"Annalen der k. k. Universitäts Sternwarte in Wien," ii. iii. Band, 1883, by E. Weiss (Wien).—"Report on the Migration of Birds," 7th Report, 1885 (Macfarlane and Erskine).

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THURSDAY, AUGUST 12, 1886

CENTRAL AMERICAN ENTOMOLOGY

Biologia Centrali-Americana. Insecta: Coleoptera. Vol. III. Part 2, "Malacodermata," by the Rev. Henry Stephen Gorham, F.Z.S., &c. (1880–86.) Vol. V. "Longicornia," by Henry Walter Bates, F.R.S. "Bruchides," by David Sharp, M.B. (London: R. H. Porter, 1879–86.)

TWO more instalments of the entomological portion of this great work show how earnestly it is being pushed on; and if it can be completed on the same lavish scale of illustration, and with the same thoroughness of execution, it will afford materials for a true conception of the richness of insect life in the tropical regions far beyond anything that has been hitherto attempted. Not only will it be superior to any other work of a similar character, but it will probably surpass in magnitude all the other works dealing with tropical insect-faunas combined.

The plan and method of treatment being exactly the same as that of Mr. Bates's volume on the Cicindelidæ and Carabidæ, already reviewed in NATURE (vol. xxxiii. p. 77), it will only be needful here to make a few remarks on points of general interest. Taking first the Malacodermata—a group represented in Britain by our Telephori, "soldiers and sailors," our glow-worm, and other allied forms—Mr. Gorham informs us that nearly one fourth of all the known species of the world are here described from Central America, a preponderance in this district which is due no doubt to the fact that the group has never been a favourite one among coleopterists, and has thus been comparatively little attended to by collectors in the tropics. The large number of 813 species here enumerated as against 1272 of the favourite Longicornes, shows that it is not impossible that this tribe may one day rank among the richest groups of beetles. From a comparison of certain of the best known families in different parts of the world Mr. Gorham is of opinion that the total number of species in the tribe is not less than 12,000. He also states that the tropical American forms are as a whole very distinct from those of Africa and the Eastern tropics, and that they rank as "persistent forms of an earlier stage of development." This is specially interesting, because it agrees so well with the fact that nowhere else in the world do low forms of mammalia and birds constitute so large a proportion of a wonderfully rich fauna as in tropical America. Another suggestive remark is, that whenever "a genus is common to Central or South America and other distant parts of the world, it is also the case that it is represented by a species also identical or nearly so in both districts." Many examples are given of this interesting fact, and the no doubt correct solution is suggested, that in these cases there must have been a comparatively recent transmissal, either from one country to another, or from some common centre to both. The Miocene beetles of Switzerland exhibit so close a resemblance to living forms that we may well suppose these identical species to have been common to Europe and North America in Miocene times, and to have passed southward to the Old and New World tropics respectively when the temperate zones became unsuitable to them.

Of Longicornia about 9000 species are known, so that those from Central America alone are nearly one-seventh of the whole; but in this tribe more perhaps than any other is our knowledge imperfect, owing to the bulk of the species being restricted to the virgin forests, where they are very local and marvellously specialised; while though exceedingly abundant under favourable conditions —that is, when extensive clearances in the forest have been *recently* made—yet at other times they are so scarce that it is impossible to obtain even a moderate collection of them.

Mr. Bates remarks on the wonderful "endemicity" of the tropical American Longicorn fauna, 304 genera out of 330 being exclusively American; while both he and Mr. Gorham insist on the whole of the Central American fauna, including that of the highlands of Mexico, having tropical rather than north-temperate affinities. As regards the Malacodermata, however, the northern parts of Mexico are said to be "totally unexplored," while Mr. Bates states that there are 30 northern generic forms which reach Mexico but rarely go further south.

The Bruchides form a small tribe of usually minute beetles which have been so imperfectly collected in the tropics that no comparisons of any value can be made. No less than 150 species are here enumerated, forming nearly one-fourth of all that are yet known, and nearly 120 of these are new species, 25 of which are figured.

On looking over the beautifully executed coloured plates, on which nearly 500 new species of Longicornes are figured, we are struck by the great preponderance of protective tints in these insects, whole plates being filled with species of delicately mottled brown or grey colours evidently harmonising with the varying hues and rugosities of the tree trunks on which they rest; while those of more elegant forms and brilliant tints are usually of smaller size, except when they gain protection by their resemblance to other inedible insects. It fortunately happens that the other group treated in these volumes—the Malacodermata—are very largely, if not wholly, such a protected group, it having been found by experiment that birds will not eat our gay-coloured Telephori, and Mr. Belt found the same to be the case with the fire-flies of Nicaragua and their allies. In all parts of the world these insects are mimicked by others which have no such protection, and it is interesting to compare the plates in these two volumes and to see how many of the Longicornes have taken on the form and colouring of the Malacoderms. Whenever I noticed a pair which undoubtedly resembled each other, I turned to the descriptions, and in every case found that they inhabited the very same locality. Thus the Longicorn *Otheotethus melanurus* imitates the Malacoderm *Lucidota discolor*, both found at Chontales, the species mimicked having however, as is usual, a wider range. *Tethlimmena aliena* and *Lygistopterus amabilis*, another mimicking pair, are both recorded from Chontales only. *Callia albicornis*, from Panama, resembles two species of Malacoderms, *Silis chalybeipennis* and *Colyphus signaticollis*, both from Panama, and both taken on the Volcano de Chiriqui. If these last two are both inedible, it is a case among the Coleoptera similar to the numerous interesting cases of protected genera of Heliconoid butterflies resembling

each other, the theory of which has been so lucidly explained by Dr. Fritz Müller (see NATURE, vol. xxvi. p. 87, and vol. xxvii. p. 481). A genus of Longicornes has been named *Lycidola*, and its eight known species are all said to resemble species of *Lycus*. Besides these there are at least a score of Longicornes which are evidently mimickers, though the exact species imitated does not happen to be represented on the plates. There are also many of the smaller species which evidently mimic ants or wingless Mutillidæ. Three such species are named by Mr. Belt, and two of these are figured, but they do not appear to resemble ants half so much as at least a score of other species; showing how difficult it is to determine whether a species is protectively coloured by means of figures however carefully drawn and coloured. The extensive collections on which these volumes are founded would, I feel sure, afford a mass of interesting cases of mimicry if search were made for them, since, besides those already mentioned, there seems to be a considerable number of Longicornes which resemble some of the Cleridæ figured among the Malacodermata, and these also are probably cases of mimicry, although I am not aware that the Cleridæ have been proved to be an uneatable group.

Looking at the copious series of figures here given there does not seem to be any superiority of colouring over the corresponding Eastern groups. The much larger proportion of Cerambycidæ to Lamiidæ in tropical America gives it an advantage over the Eastern tropics, because the former family comprises most of the elegant forms and gay colours of the tribe; but notwithstanding their inferiority in this respect the Longicornes of Penang, of Java, and of New Guinea appear to be quite equal in their development of colour to those of Central America.

The present work has been got up at so great an expense both of time, labour, and money to its originators, Messrs. Godman and Salvin, that it must be considered one of the noblest individual contributions to the study of natural history that has ever been made. Its great bulk and cost must, however, render it inaccessible to many students who would wish to possess it, while its value to them would have been considerably increased if descriptions of all the recorded species had been given as well as of those which are new, rendering it a complete book of reference to the Insecta of Central America.

I would therefore suggest to Messrs. Godman and Salvin that they would confer a still greater boon on entomological students if they could make arrangements for the preparation of a series of compact octavo volumes giving the letterpress only of the present work, together with either the original descriptions or sufficient diagnoses of all the species enumerated which are not here described. These volumes could be issued after the completion of the great work, all brought up to one uniform date; and if published at a moderate price they would be sure to command a very large sale. Complete faunal hand-books of the kind suggested are among the most generally useful works that can be published, because they obviate the enormous waste of time and labour involved in consulting scores of expensive volumes in order to determine the name and history of perhaps half the insects which a student may possess.

It is quite unsafe to venture on any detailed criticism of the work of one so thoroughly acquainted with Longicorn Coleoptera as Mr. Bates, but my attention was attracted to Table II. by the figures of two alleged female Prionidæ, which are represented of a rich green colour, while the respective males are bronzy olive. If this is the fact, it is a curious case of reversed sexual coloration, though by no means unprecedented. In one of these species, *Mallaspi belti*, however, two varieties are figured, one green, the other olive brown, both said to be females; but the green specimen (as figured) differs greatly from the brown specimen, in having the femora of the second pair of legs much longer and more slender, in the somewhat different dentation of the thorax, and especially in the very different form of the scutellum, important differences which seem inconsistent with identity of species. Should any error have crept into the plates, the author will no doubt be glad to have his attention called to it.

ALFRED R. WALLACE

GEOMETRICAL OPTICS

An Elementary Treatise on Geometrical Optics. By W. Steadman Aldis, M.A. (London: Deighton, Bell, and Co., 1886.)

THIS is a second edition of a work which appeared first in 1872, and which was designed to meet the requirements of students reading for the first three days of the Mathematical Tripos. The new edition does not differ greatly from the old, except in form. The type is larger and clearer, and in this respect the book is considerably improved.

The laws of reflection and refraction, and the reflection and refraction of direct pencils at plane and spherical surfaces, are treated in a clear and comprehensive manner. In Article 36 reference is made to a useful method of illustrating from co-ordinate geometry the relations between a point and its image. If ϕ, ϕ' be the principal focal lengths of an optical system, u and u' the distances of an object and its image from the principal points, we have $\frac{\phi}{u} + \frac{\phi'}{u'} = 1$. Thus taking rectangular axes, and measuring along them distances ϕ and ϕ', we see that u and u' are the intercepts on the axes made by a straight line passing through the point ϕ, ϕ'. This has been worked out in an interesting paper in the *Philosophical Magazine* for December 1884, by Prof. J. Loudon, of Toronto.

The next chapter deals with the oblique reflection and refraction of small pencils. The general explanation is extremely lucid, but it surely is a mistake not to have introduced the notation of the differential calculus. Of course this is excluded from the first three days of the Tripos, but so too are oblique reflection and refraction, and the work is rendered unnecessarily cumbersome by the omission. A similar remark may apply to some of the sections of the next chapter on refraction through prisms and plates.

Chapter VI. treats of lenses, which are dealt with in the ordinary manner. This part of the book would have been improved by the introduction of some of the geometrical results in which the main consequences of Gauss's work have been expressed by various writers. It is really a misfortune that the theory of principal and nodal points is so little known to English authors. It is

clearly explained in Mr. Pendlebury's book on lenses, but that does not include other parts of the subject, and is somewhat needlessly long.

The book concludes with an account of some simple optical instruments, dispersion and achromatism, and the geometrical theory of the rainbow.

OUR BOOK SHELF

New Commercial Plants and Drugs. By T. Christy, F.L.S., &c. No. 9. (London: Christy and Co., 1886.)

THIS pamphlet of 73 pages treats for the most part of medicinal products, though some consideration is also given to fodder and food-plants, essential oils, india-rubber, and various others. The first article is devoted to the Doundake (*Sarcocephalus esculentus*), a West African Rubiaceous plant, which has attracted some attention of late in cases of nervous paralysis. The root has been analysed by Messrs. Heckel and Schlagdenhauffen, and their analysis is given together with a reproduction of the two plates which accompanied their paper. Two new perfume oils come under consideration, namely, from *Eucalyptus staigeriana* and *Backhousia citriodora*. The first is a Queensland tree, and is known as the lemon-scented iron bark. The odour of the leaves is said to be exactly like that of the lemon-scented verbena, and the oil yielded by them is identical in fragrance with that from *Andropogon citratus*, or lemon-grass oil, which is imported into this country both from Ceylon and Singapore, where the plants are very extensively cultivated. Mr. Christy says that "the odour of the oil of this tree is quite different from that of *Eucalyptus citriodora*, which resembles, and might be substituted for, citronella oil, so extensively used for perfuming soap." The *Backhousia* oil is described as being like that of *Eucalyptus staigeriana*, and upon being tested for scenting soaps it was found to answer well, and would probably find a ready market in this country if it could be imported at a price to compete with ordinary verbena oil. It might realise 1*s*. 4*d*. to 2*s*. per pound.

The Kava root (*Piper methysticum*) of the Fiji Islands, which is so well known for the disgusting ceremonies which, in former times perhaps more than the present, accompanied its preparation, has of late years been introduced amongst us for its medicinal properties. The active principle of the Kava root appears to reside in a resinous substance extracted with alcohol. From a series of experiments it seems that this principle is a substance of very great importance as a local anæsthetic, but that in larger doses it produces a scaly affection of the skin. From the Kava root a spirit or liqueur has been distilled, and this under the name of Yagona is on sale at the refreshment bars of the present Colonial and Indian Exhibition.

Another new drug which probably has a future before it is the Kombe of Central Africa (*Strophanthus hispida* or *S. Kombe*) which has been proved to be of considerable value in affections of the heart. The first communication relating to the physiological action of this drug was made by Prof. Fraser to the Royal Society of Edinburgh in 1870, which was followed in 1885 by a more elaborate paper at the Cardiff Meeting of the British Medical Association. There seems, however, even after this lapse of time to be a difficulty in obtaining the seeds in quantity, or even the right species, several forms having been introduced from the Gold Coast, Sierra Leone, and other parts of Africa; the chief difference lies in the seed, some forms of which are covered with long, fine silky hairs.

Mr. Christy's pamphlet, like its predecessors, is a useful record of newly introduced and useful plants.

Heidelberg gefeirt von Dichtern und Denkern seit fünf Jahrhundesten. Herausgegeben von Albert Mays. (Heidelberg, 1886.)

IT was a happy thought of the compiler of this volume to collect and publish on the five hundredth anniversary of the foundation of the University of Heidelberg a selection of what has been written about the city and the University by eminent men of various nations at different periods of time. A collection of all that has been written about the ancient city and its lovely situation would, Herr Mays says, fill a respectable library, for besides histories in verse of the Palatinate and its capital there are innumerable tales, novels, and the like based on incidents in its history, and lyrical and historical poems on Heidelberg by the hundred. In making a selection from this vast mass of matter, the compiler has only retained poems or descriptions which are of special poetical or literary value, or those which are of special interest on account of the author, or, finally, those which exhibit some special originality or peculiarity. But even when thus winnowed a handy volume is left. Needless to say, the vast majority of the writers are German; there are a few English, and one American (Longfellow) The list commences with an extract from the Bull of Pope Urban VI. of October 23, 1385, authorising Prince Rupert to found the University. This is followed by extracts from over sixty authors arranged chronologically. Herr Mays notices as a curiosity that not one of these is French. The English authors naturally dwell on the castle, "next to the Alhambra of Granada the most magnificent ruin of the Middle Ages," rather more than on the University; but indeed the German writers do the same. The book will show the good people of Heidelberg, if they lack such knowledge at this festive season, that they are citizens of no mean city. It should also prove an interesting memento to many in Europe and America who have passed a few years at the most impressionable period of their lives at the old University, which, with its sister at Bonn, has of late years drawn the British student away from Göttingen.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Organic Evolution

FOR some time I have much desired to direct the attention of your readers all over the world to the two very remarkable articles on Organic Evolution, by Mr. Herbert Spencer, which appeared in the April and May numbers of the *Nineteenth Century*. I hope they will be separately published. They mark in my opinion a new departure in the Philosophy which has been built up by a certain school of writers on the Darwinian Theory. Let me explain what I mean.

From the first discussions which arose on this subject I have ventured to maintain that the successors of Darwin have run quite wild from the teaching of their master—that his Hypothesis, even if completely true so far as it went, offered no adequate explanation whatever of the multiform and complicated facts of Organic Evolution—that the phrase "natural selection" represented no true physical cause, still less the complete set of causes requisite to account for the orderly procession of organic forms in Nature; that in so far as it assumed variations to arise by accident it was not only essentially faulty and incomplete, but fundamentally erroneous; in short, that its only value lay in the convenience with which it groups under one form of words, highly charged with metaphor, an immense variety of causes, some purely mental, some purely vital, and others purely physical or mechanical.

The violence with which false interpretations were put upon this Theory and a function was assigned to it which it could never fulfil, will some day be recognised as one of the least creditable episodes in the history of science. With a curious perversity it was the weakest elements in the Theory which were seized upon as the most valuable, particularly the part assigned to blind chance in the occurrence of variations. This was valued not for its scientific truth,—for it could pretend to none,—but because of its assumed bearing upon another field of thought and the weapon it afforded for expelling Mind from among the causes of Evolution.

There have been many symptoms that this Philosophy is breaking down. Mr. Herbert Spencer, although he has worked out the consequences of Evolution with enthusiasm, has never been blind to some of its defects. His mind is too closely analytical not to be brought into contact at many points, with its manifest inapplicability and its wordy hollowness.

But in these two articles we have for the first time an avowed and definite declaration against some of the leading ideas on which the Mechanical Philosophy depends; and yet the caution, and almost the timidity, with which a man so eminent approaches the announcement of conclusions of the most self-evident truth—is a most curious proof of the Reign of Terror which had come to be established.

I cannot in this letter indicate the breadth and sweep of the admissions now made by Mr. Herbert Spencer in the two articles referred to,—fatal to the adequacy of the Mechanical Philosophy as any explanation of organic evolution. They cluster round, and follow from the central admission that "the words 'natural selection' do not express a cause in the physical sense." Another great admission is that the "co-operation" which is required in the growth and development of useful parts, cannot be accidental.

Of course, now that so eminent a man as Mr. Herbert Spencer has opened his eyes and his mouth to these—and many other—admissions, we shall have all the *Dii Minores* following suit.

I have read with great pleasure an article in your last number (p. 314) on "Physiological Selection," with an "additional suggestion on the origin of species." I rejoice that the author has at last discovered that "natural selection has been made to pose as a theory of the origin of species, whereas in point of fact it is nothing of the kind." This has been my contention for many years. ARGYLL

Aurora

WITH reference to the aurora of July 27, accounts of which appear in NATURE, vol. xxxiv. pp. 311 and 312, the following particulars of the accompanying magnetic disturbance recorded here may be of interest. The disturbance commenced about 3 p.m. on July 27 with small fluctuations in declination and horizontal force, followed by larger movements which commenced sharply at 10.20 p.m. in all three elements, and continued to about 7 a.m. on July 28. The greatest movement was between 10.20 and 11.30 p.m., amounting to 45′ in declination, ·011 of the horizontal force, and ·005 of the vertical force. Corresponding earth-currents were recorded as usual.

W. H. M. CHRISTIE

Royal Observatory, Greenwich, August 10

Mock Suns

KINDLY add the following, to make up for omission of my figure in your issue of July 29 (p. 289):—

The "arched eyebrows," as I called them, can best be described thus—

Resting on the top of the halo circle, where the third mock light stood, was a bow of peculiar curve. It was like two well-arched eyebrows flowing together by a curve of gentle dip at the point where it touched the halo. Each arch was about equal to one-eighth part of the halo circle in every respect except that its centre lay about the middle of the chord joining the upper mock light with the mock light on that side of the sun. The contrariflexure, and the anomalous positions of the two centres of the two arches, strike me as very noteworthy. I cannot presume to guess at an explanation.

May I add that a correspondent of the *Standard* states that he too saw the white ray on the *left* side; and that it stretched, to use his expression, "round the sky almost to the east, and at the end of it was another mock sun much less brilliant," where it "seemed about to begin a fresh series of mock suns and circles." This too seems to me too striking a feature to be lost to record in NATURE. W. J. HERSCHEL

Littlemore, August 2

Meteors

ON August 4, 10h. 40m., a beautiful slow meteor was seen here threading its way from about 2° S. of *o* Ursæ Majoris to very slightly below β Aurigæ. Its light fluctuated greatly, but at its best it must have been brighter than Jupiter, though the effect was much marred by mist. The most noteworthy feature was its extreme slowness of movement; a careful determination gave 8 seconds as the time it remained in sight. There was no train of any sort; the meteor rolled along with a star-like aspect, and its velocity near the end point became so much impeded that it seemed almost stationary. I observed fifty-seven other meteors during the same night, but none of these could be associated in appearance and direction with the one specially described. Its radiant-point was probably in Ursa Major, close to β, at about $162° + 59°$.

On August 6, 10h. 3m., a meteor equal to Jupiter was seen pursuing a long path just south of and nearly parallel to α and ϵ Pegasi. It left a bright streak, and was a conspicuous object, notwithstanding the moonlight. The radiant-point was at about $32° + 17°$, nearly 6° S.S.E. of α Arietis, or possibly in the extreme east boundaries of Aries.

It would be important to hear of duplicate observations of these large meteors. In the eastern parts of England they must have appeared very bright, and being visible at a convenient hour in the evening many persons will have noticed them.

Bristol, August 9 W. F. DENNING

LAST night at about eleven o'clock a fine meteor was visible here through an opening in clouds. Its path was in Aquarius, commenced a little to the east of η, and seemed to be in the direction of a line joining η and δ. In three or four seconds the meteor passed over about 20°, and it left momentarily a trail over the last 10°. This was slightly curved, the convex side being to the east, and the colour varying from yellow for a quarter of the curve to red during the remainder. At first the meteor resembled Saturn in size and colour, then became larger, whiter, and afterwards pale blue, and when it finally disappeared behind the clouds it considerably exceeded Venus at her brightest, both in size and brilliance. L. J. H.

Ramsey, Isle of Man, August 5

PHYSIOLOGICAL SELECTION: AN ADDITIONAL SUGGESTION ON THE ORIGIN OF SPECIES [1]

II.

NEXT, let it be observed that we cannot expect to meet with much direct evidence of physiological selection from our domesticated varieties. For, first, breeders and horticulturists keep their strains separate artificially, and preserve many kinds of variation other than those of the reproductive system with which alone we are concerned; and, secondly, it is never the aim of these men to preserve this particular kind of variation. Therefore, all that we can here learn from our domesticated productions is the paramount importance of preventing intercrossing with parent forms, if a new varietal form is ever to gain a footing. No one of these domesticated varieties could have been what it now is unless such intercrossing had been systematically prevented by man; and this gives us good reason to infer that no natural species could have been what it now is unless every variety in which every species originated had been prevented from intercrossing with its parent form by nature. For the cases are extremely rare in which one species differs from another (living or extinct) in respect of any feature so highly utilitarian in character as to justify belief that the newer species owed its differentiation to natural selection having been able to overcome the swamping effects of free intercrossing.

[1] Abstract of a Paper read before the Linnean Society on May 6, by George J. Romanes, M.A., LL.D., F.R.S. &c. Continued from p. 316.

Again, as to plants and animals in a state of nature, the particular variation with which we are concerned would scarcely be noticed until it had given rise to a new species. In this respect, therefore, the theory of physiological selection is in the same predicament as that of natural selection: in neither case are we able directly to observe the formation of one species out of another by the agency supposed; and, therefore, in both cases our belief in the agency supposed must to a large extent depend on the probability established by general considerations. Nevertheless, although our sources of direct evidence are thus seen to be necessarily limited, I shall now hope to show that they are sufficient to prove the only fact which they are required to prove—namely, that the particular kind of variation, on the occurrence of which my theory depends, does occur both in nature and under domestication.

One very obvious example of the particular variation which is required by the theory of physiological selection has already been given in the season of flowering or of pairing being either advanced or retarded. This I take to be a most important case for us, inasmuch as it is one that must frequently arise in nature. Depending as it chiefly does on external causes, numberless species both of plants and animals must, I believe, have been segregated by its influence. For in every case where a change of food, temperature, humidity, altitude, or of any of the other many and complex conditions which go to constitute environment—whether the change be due to migration of the species or to alterations going on in an area occupied by a stationary species—in every case where such a change either retards or promotes the season of propagation, there we have the kind of variation which is required for physiological selection. And it is needless to give detailed instances of such variation where this is due to so well known and so frequently observed a cause.

But it is on what may be called the spontaneous variability of the reproductive system itself that I mainly rely for evidence of physiological selection. The causes of variability are here much more numerous, subtle, and complex than are such extrinsic causes as those just mentioned; and they are always at work in the reproductive systems of all organisms. The consequence is, as Mr. Darwin has shown by abundant evidence, that variations in the direction of sterility depend more on what he calls the nature of the organism than on the influence of external conditions. Of this fact we have direct evidence, firstly in individuals, secondly in varieties, and thirdly in species.

(1) *Individuals.*—Mr. Darwin observes, "it is by no means rare to find certain males and females which will not breed together, though both are known to be perfectly fertile with other males and females. We have no reason to suppose that this is caused by these animals having been subjected to any change in their habits of life; therefore such cases are hardly related to our present subject. The cause apparently lies in an innate sexual incompatibility of the pair when matched." He then proceeds to give examples from horses, cattle, pigs, dogs, and pigeons, concluding with the remark that "these facts are worth recording, as they show, like so many previous facts, on what slight constitutional differences the fertility of an animal often depends." Elsewhere he gives references to similar facts in the case of plants; and instances of this individual incompatibility, both in plants and animals, might easily be multiplied.

Now, if even as between two individuals there may thus arise absolute sterility without there being in either of them the least impairment of fertility towards other individuals, much more may such incompatibility extend towards a number of individuals. For certainly the most remarkable feature about this individual incompatibility is the fact of its being only individual: it would not be nearly so remarkable, or antecedently improbable, if the incompatibility were to run through a whole race or strain. In the fact of individual incompatibility, therefore, we have the kind of variation which my theory requires, and this as arising spontaneously in the highest degree of efficiency.

(2) *Races.*—But of even more importance for us is the direct evidence of such a state of matters in the case of varieties, breeds, or strains. In the ninth chapter of the "Origin of Species," and in the nineteenth chapter of the "Variation of Plants and Animals under Domestication," Mr. Darwin adduces miscellaneous instances of varieties of the same species which exhibit a higher degree of fertility within themselves than they do with one another. In this respect, therefore, they resemble natural species; but as they are only domesticated varieties known to belong to the same species, they are here available as evidence of what may be termed racial incompatibility, or of the particular kind of variation which my theory requires. To quote only two instances: "The yellow and white varieties (of Verbascum) when crossed produce less seed than the similarly coloured varieties"; and the blue and red varieties of the pimpernel are absolutely sterile together, while each is perfectly fertile within itself. Such instances are the more suggestive on account of their arising under domestication, because, as a rule, domestication increases fertility, and is thus inimical to sterility—sometimes even breaking down the physiological barriers between natural species. Therefore, if in some cases even under domestication the reproductive system may vary in this manner, so as to erect physiological barriers between artificial varieties, much more are such barriers likely to be erected between varieties when these arise in a state of nature.

But as regards varieties in a state of nature, I have not been able to meet with any evidence of racial incompatibility. Nor is this to be wondered at: for, unless the degree of such incompatibility were well pronounced, it would not be noticed; while, if it were well pronounced, the two varieties would for this very reason be classified as species. Therefore, the fact of racial incompatibility within the limits of wild species could only be proved by experiments undertaken expressly for the purpose, in the way which I shall afterwards explain.

(3) *Species.*—According to the general theory of evolution, which in this paper is taken for granted, the distinction between varieties and species is only a distinction of degree; and the distinction is mainly, as well as most generally, that of mutual sterility. Therefore my theory of physiological selection is here furnished with an incalculable number of instances of the particular kind of variation which is required; for in so many instances as variation has led to any degree of sterility between parent and varietal forms—or between the varying descendants of the same form—in so many instances it is merely a statement of fact to say that physiological selection must have taken place. There remains, however, the question whether the particular change in the reproductive system which led to all these cases of mutual sterility was anterior or posterior to changes in other parts of the organism. For, if it was anterior, these other changes—even though they be adaptive changes—were presumably due to the sexual change having interposed its barrier to crossing with parent forms; while, if the sexual change were posterior to the others, the presumption would be that it was the latter which, by their reaction on the sexual system, induced the former. I shall have to consider this alternative later on. Here, therefore, it is enough to point out that under either possibility the principles of physiological selection must have been at work; only these principles are accredited with so much the more causal influence in the production of species in the proportion that we find reason to suppose the sexual change to have been, as a rule, the prior change. But under either alternative, and on the doctrine that species are extreme

varieties, we have many hundreds of thousands of instances of fertility within the varietal forms with sterility towards allied forms.

Probably enough has now been said to show that, as a matter of fact, the particular kind of variation in the reproductive system which is required by the theory of physiological selection does occur, firstly, in individuals; secondly, in races; and thirdly, in species. But the evidence of physiological selection as an agency in the evolution of species is so far only *primâ facie*. That is to say, although we have evidence to prove the occurrence of this particular kind of variation, and although we can see that whenever it does occur it must be preserved, as yet we have seen no evidence to show how far this kind of variation has been at work in the formation of species. I will, therefore, next proceed to give an outline sketch of the evidence which I have been able to find, tending to show that the facts of organic nature are such as they ought to be, if it is true that physiological selection has played any considerable part in their causation. And to do this I will begin by taking the three cardinal objections to the theory of natural selection with which I set out—namely, sterility, intercrossing, and inutility. For, as we shall see—and this in itself is a suggestive consideration—all the facts which here present formidable obstacles to the theory of natural selection, when this is regarded only as a theory of the origin of species, are not only explained by the theory of physiological selection, but furnish to that theory some of the best evidence which I have been able to find.

Argument from Sterility.—In what respects do species differ from one another? They differ firstly, chiefly, and most generally in respect of their reproductive systems: this, therefore, I will call the primary difference. Next, they differ in an endless variety of more or less minute details of structure, which are sometimes adaptive and sometimes not. These, therefore, I will call the secondary differences. Now, the secondary differences are never numerous as between any two allied species: in almost all cases they admit of being represented by units. Yet, if it were possible to enumerate all the specific differences throughout both the vegetable and animal kingdoms, there would be required a row of figures expressive of many millions. In other words, the secondary specific distinctions may occur in any parts of organisms, but never occur in many parts of the same organism. So that, if we have regard to the whole range of species, the secondary distinctions are seen to be, in the highest imaginable degree, variable or inconstant: the only distinction which is at all constant or general is the primary distinction, or the one which belongs exclusively to the reproductive system. Surely, therefore, what we primarily require in any theory of the origin of *species*, is an explanation of this relatively constant or general distinction. But this is just what all previous theories fail to supply. Natural selection accounts for some among the many secondary distinctions, but is confessedly unable to explain the primary distinction. The same remark applies to sexual selection, use and disuse, economy of growth, correlated variability, and so forth. Even the prevention of intercrossing by geographical barriers or by migration is unable to explain the very general occurrence of some degree of sterility between two allied varieties which have diverged sufficiently to take rank as different species. All these theories, therefore, are here in the same predicament: they profess to be theories of the origin of species, and yet none of them is able to explain the one fact which more than any other goes to constitute the distinction between species and species. The consequence is that most evolutionists here fall back upon a great assumption: they say it must be the change of organisation which causes the sterility—it must be the secondary distinctions which determine the primary. But the contrary proposition is surely at least as probable, namely, that it is the sterility which, by preventing intercrossing with parent forms, has determined the secondary distinctions—or, rather, that it has been the original condition to the operation of the modifying causes in all cases where free intercrossing has not been otherwise prevented. For, obviously, it is a pure assumption to say that the secondary differences have always been historically prior to the primary difference, and that they stand to it in the relation of cause to effect. Moreover, the assumption does not stand the test of examination, as I will now proceed to show.

(1) On merely *a priori* grounds it scarcely seems probable that whenever any part of any organism is slightly changed in any way by natural selection, or by any other cause, the reproductive system should forthwith respond to that change by becoming sterile with allied forms. Yet this is really what the assumption in question requires, seeing that *all* parts of organisms are subject to the secondary specific distinctions. What we find in nature is a more or less constant association between the one primary distinction and an endless profusion of secondary distinctions. Now, if this association had been between the primary distinction and some one—or even some few—secondary distinctions, constantly the same in kind; in this case I could have seen that the question would have been an open one as to which was the conditional and which the conditioned. But as the case actually stands, on merely antecedent grounds, it does not appear to me that the question is an open one. Here we have a constant peculiarity of the reproductive system, repeated over and over again—millions of times—throughout organic nature; and we perpetually find that when this peculiar condition of the reproductive system is present, it is associated with structural changes elsewhere, which, however, may affect any part of any organism, and this in any degree. Now, I ask, is it a reasonable view that the one constant peculiarity is always the result, and never the condition, of any among these millions of inconstant and organically minute changes with which it is found associated?

(2). But, quitting *a priori* grounds, it is a matter of notorious fact that in the case of nearly all our innumerable artificial productions, organisms do admit of being profoundly changed in a great variety of ways, without any reaction on the reproductive system following as a consequence.

(3) Again, as regards wild species, Mr. Darwin proves that "the correspondence between systematic affinity and the facility of crossing is by no means strict. A multitude of cases could be given of very closely allied species which will not unite, or only with extreme difficulty; and, on the other hand, of very distinct species which unite with the utmost facility." And he goes on to say that "within the limits of the same family, or even of the same genus, these opposite cases may occur"; so that "the capacity of the species to cross is often completely independent of this systematic affinity, that is, of any difference in their structure or constitution, excepting in their reproductive systems." Now, on the supposition that sterility between species is always, or generally, caused by the indirect influence on the reproductive system of changes taking place in other parts of the organism, these facts are unintelligible—being, indeed, as a mere matter of logic, contradictory of the supposition.

(4) Mr. Darwin further shows that, "independently of the question of fertility, in all other respects there is the closest general resemblance between hybrids and mongrels." Clearly, this fact implies that natural selection and artificial selection run perfectly parallel in all other respects, save in the one respect of reacting on the reproductive system, where, according to the views against which I am arguing, they must be regarded as differing, not only constantly, but also profoundly.

(5) Lastly, Mr. Darwin concedes—or rather insists—

that "the primary cause of the sterility of crossed species is confined to differences in their sexual elements." A general fact which assuredly proves that the primary specific distinction is one with which the organism as a whole is not concerned: it is merely a local variation which is concerned only with the sexual system. Why, then, should we suppose that it differs from a local variation taking place in any other part of the organism? Why should we suppose that, unlike all other such variations, it can never be independent, but must always be superinduced as a secondary result of changes taking place elsewhere? It appears to me that the only reason why evolutionists suppose this is because the particular variation in question happens to have as its result the origination of species; and that, being already committed to a belief in natural selection or other agencies as the causes of such origination, they are led to regard this particular kind of local variation as not independent, but superinduced as a secondary result of these other agencies operating on other parts of the organism. But once let evolutionists clearly perceive that natural selection is concerned with the origin of species only in so far as it is concerned with the origin of adaptive structures—or of some among the secondary distinctions—and they will perceive that the primary specific distinction takes its place beside all other variations as a variation of a local character, which may, indeed, at times be due to the indirect influence of natural selection, use, disuse, and so forth; but which may also be due to any of the numberless and hidden causes that are concerned with variation in general.[1]

I trust, then, that reasons enough have now been given to justify my view that, if we take a broad survey of all the facts bearing on the question, it becomes almost impossible to doubt that the primary specific distinction is, as a general rule, the primordial distinction. I say "as a general rule," because the next point which I wish to present is that it constitutes no part of my argument to deny that in some—and possibly in many—cases the primary distinction may have been superinduced by the secondary distinctions. Indeed, looking to the occasional appearance of partial sterility between our domesticated productions, as well as to the universally high degree of it between genera, and its universally absolute degree between families, orders, and classes, I see the best of reasons to conclude that in *some* cases the sterility between *species* may have been originally caused, *and in a much greater number of cases subsequently intensified*, by changes going on in other parts of the organism. Moreover, I doubt not that of the agencies determining such changes natural selection is probably one of the most important. But what does this amount to? It amounts to nothing more than a re-statement of the theory of physiological selection. It merely suggests hypothetically the cause, or causes, of that particular variation in the reproductive system with which alone the theory of physiological selection is concerned, and which, as a matter of fact, *howsoever caused*, is found to constitute the one cardinal distinction between species and species. Therefore I am really not concerned with what I deem the impossible task of showing how far, or how often, natural selection—or any other cause—may have induced this particular kind of variation in the reproductive system by its operations on other parts of an organism. Even if I were to go the full length that other evolutionists have gone, and regard this primary specific distinction as in all cases due to the secondary specific distinctions, still I should not be vacating my theory of physiological selection: I should merely be limiting the possibilities of variation within the reproductive system in what I now consider a wholly unjustifiable manner. For, as previously stated, it appears to me much the more rational view that the primary specific distinction is likewise, as a rule, the primordial distinction; and that the cases where it has been superinduced by the secondary distinctions are comparatively few in number.[1]

If we thus regard sterility between species as the result of what I have called a local variation, or a variation arising only in the reproductive system—whether this be induced by changes taking place in other parts of the organism, to changes in the conditions of life, or to changes inherent in the reproductive system itself—we can understand why such sterility rarely, though sometimes, occurs in our domesticated productions; why it so generally occurs in some degree between species; and why as between species it occurs in all degrees.

It rarely occurs in our domesticated productions because it has never been the object of breeders or horticulturists to preserve this kind of variation. Yet it sometimes does occur in some degree among our domesticated productions, because the changes produced on other parts of the organism by artificial selection do, in a small percentage of cases, react upon the reproductive system in the way of tending to induce sterility with the parent form, while not lessening fertility with the varietal form. Again, this particular condition of the reproductive system is so generally characteristic of species simply because in as many cases as it occurs it has constituted the reason why species exist as species. And, lastly, this particular variation in the reproductive system has taken place under nature in such a variety of degrees—from absolute sterility between species up to complete, or even to more than complete, fertility—because natural species, while being records of this particular *kind* of variation are likewise the records of all *degrees* of such variation which have proved sufficient to prevent overwhelming intercrossing with parent forms. Sometimes this degree has been less than at other times, because other conditions—climatic, geographical, habitatorial, physiological, and even psychological[2]—have co-operated to prevent intercrossing, with the result of a correspondingly less degree of sterility being required to secure a differentiation of specific type. Lastly, where species have been evolved on different geographical areas, or by use, disuse, and other causes of a similarly "direct" kind, there has been no need to prevent intercrossing in any degree; so that allied species formed under any of these conditions may still remain perfectly fertile, or even more than naturally fertile, with one another.

In view of these considerations, I should regard it as a serious objection to my theory if it could be shown that sterility between allied species is invariably absolute, or even if it could be shown that there are no cases of unimpaired fertility. What my theory would expect to find is exactly what we do find—namely, an enormous majority

[1] Mr. Darwin himself does not appear to have held the view against which I am now arguing—v.z. that the primary distinction is always, or usually, superinduced by the secondary. Not even here, therefore, is his authority opposed to my views: upon this question his voice is merely silent.

[1] The paper here develops another line of argument which it is difficult to render in abstract. Its object, however, is to show that, even in the cases where the primary distinction is superinduced by the secondary—whether these cases are, as I believe, "comparatively few" or comparatively numerous—my theory is available to explain why the primary distinction is so habitual an accompaniment of the secondary distinctions, of whatever kinds or degrees the latter may happen to be. For, according to my theory, the reason of this association in such cases is that it can only be those kinds and degrees of secondary distinction which are able so to react on the reproductive system as to induce the primary distinction that are, *for this reason*, preserved. Or, otherwise expressed, in cases where the secondary distinctions induce the primary, the former owe their existence to the fact that they happened to be of a kind capable of producing this particular effect. Under this view, even in these cases it is the principles of physiological selection that have determined the kinds of secondary distinction which are allowed to survive. For these principles have, in all such cases, *selected* the particular kinds of secondary distinction which have proved themselves capable of so reacting on the reproductive system as to bring about the primary distinction—a general view of the subject which appears to be justified by the very general association between the two.

[2] See "Origin of Species," p. 81, where it is shown that among vertebrated animals different varieties of the same species, even when living on the same area, frequently exhibit a marked repugnance to pairing with one another. In the same passage, it is remarked the different varieties sometimes occupy different stations.

of instances where sterility occurs in all degrees, with a few exceptional instances where secondary distinctions have been able to develop without being associated with the primary distinction. So that, on the whole, I cannot but candidly consider that all the facts relating to the sterility of natural species are just what they ought to be, if they have been in chief part due to the principle which I am advocating. Mr. Darwin appears to have clearly perceived that there must be some one principle serving to explain all these facts—so curiously related, and yet so curiously diverse. For he says, and he says most truly, "We have conclusive evidence that the sterility of species must be due to some principle quite independent of natural selection." I trust I have now said enough to show that, in all probability, this hitherto undetected principle is the principle of physiological selection.

(*To be continued.*)

RED SUNSETS AND VOLCANIC ERUPTIONS

THE great volcanic eruption in New Zealand raises anew the question of the connection between volcanic eruptions and sunset phenomena arising from attenuated matter in the upper regions of the atmosphere. The theory that the noteworthy sunsets about the end of 1883 were due to the Krakatão eruption has been questioned on the ground that, in many parts of the world, these red sunsets have continued until the present time, though not with the same intensity as in 1883. Beautifully variegated sunsets have always been very common in this country. The result was that the sunset phenomena of 1883 did not appear to us as anything new in kind, but only as an intensification of something with which we were already familiar. In order to reach a decisive conclusion we must have observations made in regions where the upper atmosphere is exceptionally free from vapours or other attenuated matter. The advent of such matter can then be detected when it could not be detected at other places. Among such regions I would suggest South Africa, especially the Cape of Good Hope. During my brief residence there in November and December of 1882, nothing was more striking than the extreme whiteness and purity of the western twilight. If such a twilight is there the rule during the whole year, then any diffusion of volcanic vapour in the upper atmosphere must produce a very striking effect. I would therefore suggest to observers in that region the value of precise information on this point, especially with a view of learning to what extent, and within what time, the red sunsets of 1883 disappeared, and whether any such phenomena now reappear as the result of the volcanic eruption in New Zealand. S. NEWCOMB

MR. FORBES'S EXPEDITION TO NEW GUINEA

NATURALISTS will be glad to learn that a collection of natural history objects has been sent to England by our countryman Mr. H. O. Forbes, who has been doing good work on the Astrolabe Mountains in south-eastern New Guinea. Unfortunately a lack of proper support appears to have greatly crippled the efforts of this energetic traveller, who fears that he may be compelled to abandon his proposed expedition to Mount Owen Stanley, the ascent of which was the primary object of his explorations on leaving England. The disaster which befel Mr. Forbes at Batavia, where the boat with all his equipment for the expedition was capsized and everything lost, will be fresh in the recollection of our readers (see NATURE, vol. xxxii. p. 552), and it is only by the utmost display of courage and energy, and by a large pecuniary sacrifice on his own part, that Mr. Forbes has been able to fit out an expedition to New Guinea from Brisbane. It is to be hoped that the great Societies of this country and Australia will not allow this expedition to come to an end for lack of funds. Mr. Forbes has shown what he can do in the cause of science, and a little timely help would now enable him to conduct the work of exploration on which his soul is bent and bring it to a successful issue. It is not generally known in this country that during his last expedition to the Malay Archipelago he expended more than 600*l*. of his own money in endeavouring to render his expedition more complete, and nothing but a little generous encouragement is needed to enable him to sustain the serious pecuniary loss which has befallen him in his attempt to reach Mount Owen Stanley. At the time of writing, we hear that there is a prospect of Australia coming to the rescue and aiding Mr. Forbes towards the attainment of his object, and we trust that England will also do something for a man who, as an explorer and a naturalist, has reflected credit on his country.

The district recently explored by Mr. Forbes has been visited before by Mr. Goldie and Mr. Hunstein, the latter of whom has procured some remarkable novelties among the Birds of Paradise, which have been recently described by Dr. Finsch and Dr. Meyer (*Zeitsch. Ges. Orn.* ii. pp. 369-391). Hunstein has indeed penetrated further than Mr. Forbes was able to do on the present occasion, as the latter has as yet only worked the Sogeri district from a height of 1000 to 5000 feet, and this only in the rainy season.

Among the many interesting species found by Mr. Forbes in the Sogeri district may be mentioned *Harpyopsis novæ-guineæ*, Salvad, *Charmosyna stellæ*, Meyer, *Psittacella pallida*, Meyer (scarcely to be distinguished from *P. brehmi* of Mount Arfak), *P. madaraszi*, Meyer, *Eos incondita*, Meyer, *Parotia lawesi*, Ramsay, *Lophorina minor*, Ramsay, *Paradisornis rudolphi*, Meyer, *Amblyornis subalaris*, Sharpe, *Paradisea raggiana*, Sclater, *Microdynamis parva*, Salvad (*Rhamphomantis rollesi*, Ramsay), *Melidectes emilii*, Meyer, *Rallicula rubra*, Schlegel, and many other notable species, amongst which are two which appear to be undescribed, viz.,

Melirrhophetes batesi, sp. n.

M. similis *M. ochromelani*, Meyer, sed fasciâ supraoculari cervina distinguendus. Long. tot. 9·3, culm. 1·35, alæ 4 8, caudæ 4·2, tarsi 1·15.

Pseudogerygone cinereiceps, sp. n.

P. similis *P. flavilaterali*, Gray, ex Novâ Caledoniâ, sed minor, et rectricibus haud subterminaliter albo fasciatis distinguenda. Long. tot. 3·4, culm. 0·4, alæ 1·9, caudæ 1·2, tarsi 0·65.

It was a little unfortunate for Mr. Forbes that Mr. Hunstein should have visited the Horse-shoe Range so shortly before the arrival of the former in New Guinea, but it says much for the complete way in which Mr. Forbes does his work of exploration, that he should have obtained specimens of nearly every one of the new species discovered by Mr. Hunstein. Unfortunately the Birds of Paradise were out of colour at the time of his visit, but the specimens sent are of great interest, as showing the moults and changes of plumage in these birds. Of the extraordinary species known as Prince Rudolph's Bird of Paradise (*Paradisornis rudolphi*) with blue wings and blue flank-plumes, Mr. Forbes secured only one, apparently a female. As these surprising novelties have been discovered in the Astrolabe Range, which has an elevation of less than 5000 feet, what prizes and discoveries may not be awaiting the explorer if he reaches Mount Owen Stanley with an altitude of 13,000 feet? May he succeed!

R. BOWDLER SHARPE

THE PERSISTENT LOW TEMPERATURE

IT is seldom that the weather maintains such a decided persistency for temperatures below the average conditions. Week after week passes, and the thermometrical

records are monotonously alike in chronicling low temperatures over the whole country.

The Weekly Weather Reports issued by the Meteorological Office are compiled from observations at stations fairly representative of the whole of the British Islands, and the results are grouped into twelve districts. These returns show that the low temperature is not limited to any special area of the United Kingdom, but is common to every part. From the middle of May to the beginning of August there was only one week, ending July 5, in which the temperature was above the average in the western districts of England, Ireland, or the Channel Islands, whilst in the period of seven months from January 4 to August 2 the temperature in the north-west and south-west of England and in the Channel Islands has only been above the average in three weeks—March 29, May 10, and July 5; and averaging the results for the whole of the British Islands, these are the only weeks in which the resultant temperature was above the average, and may fairly be considered the only warm periods during the seven months.

This persistency of low temperature is to be traced over the whole of the past twelve months, commencing with the beginning of August 1885. To the three warm weeks already mentioned there must be added those of November 9 and 30, December 21, and January 4, making seven in all, and these represent the only warm weeks throughout the entire period, and are the only weeks in which the mean temperature for the whole of the British Islands was above the average.

The following table, which is compiled from the Weekly Weather Reports for the fifty-two weeks ending August 2, 1886, shows the number of weeks with the temperature in excess or defect of the average, and the extent of the deficiency for the several districts. The averages used for the comparison are for the twenty years 1861–1880.

	Weeks above the average	Weeks in agreement with average	Weeks below the average				
			1°-2°	3°-4°	5°-6°	7°-8°	9°-10°
Scotland, N. ...	7	6	15	16	6	2	—
Scotland, E. ...	11	4	17	10	7	3	—
England, N.E. ...	12	2	14	13	8	3	—
England, E. ...	9	3	13	11	12	1	3
Midland Counties ...	11	2	12	13	9	2	3
England, S. ...	10	7	14	10	6	2	3
Scotland, W. ...	10	1	23	9	5	4	—
England, N.W. ...	7	—	17	12	11	2	3
England, S.W. ...	7	6	14	13	7	2	3
Ireland, N. ...	9	1	18	15	5	4	—
Ireland, S. ...	8	5	19	10	7	2	1
Channel Islands ...	6	9	20	11	5	—	1

From this it is seen that the highest number of weeks during the year with the temperature above the average was twelve in the north-east of England, whilst the lowest was six in the Channel Islands.

Throwing the weekly values together so as to form a monthly result, it is seen that November 1885 is the only month of the last twelve in which the resultant temperature for the whole of the British Islands was above the average, and then the excess only amounted to 1°; of the remaining eleven months, one was in agreement with the average, one had a defect of 1°, four had a defect of 2°, three a defect of 3°, and two a defect of 4°. The three consecutive months having the greatest deficiency of temperature were January to March, the defect averaging fully 3° for the entire period.

The rainfall for the same twelve months was above the average during the six months September, October, and November 1885, and January, May, and July 1886, the excess being larger in England than in Scotland or Ireland. It was in fair agreement with the average in March and April, and in defect in the four months August and December 1885, and February and June 1886.

CHAS. HARDING

THE PLIOCENE DEPOSITS OF NORTH-WESTERN EUROPE

In the series of stratigraphical monographs on which the Geological Survey is engaged, the preparation of the volume treating of the Pliocene deposits has been assigned to Mr. Clement Reid. In pursuance of the plan on which these works are being written, I requested him to visit some of the Continental regions where deposits of corresponding age are best developed, and a personal acquaintance with which would extend his knowledge of their English equivalents. He has accordingly spent some time recently in Belgium and Holland, and among other localities visited the well-known exposures of the Diestian beds around Diest and Antwerp. The sections there laid open, the remarkable assemblage of organic remains contained in them, and the peculiar condition in which the shells at Diest have been preserved led him on his return to this country to re-examine the curious deposit of ironstone at Lenham, on the North Downs, in which, so far back as 1857, the occurrence of Pliocene shells was announced by Prof. Prestwich. Doubt was cast upon this identification of the age of these shells: by many geologists they were looked upon as Lower Eocene, though their original discoverer has consistently maintained his opinion. Mr. Reid has now been fortunate enough to obtain a considerable number of additional species that settle beyond doubt the Pliocene age of the Lenham beds, and thus confirm the view of the veteran Oxford Professor. The establishment of this point raises questions of such wide interest in geology that I feel justified in anticipating the appearance of the memoir in which the facts will be detailed. At my request Mr. Reid has drawn up the following report, which briefly embodies the facts he has brought to notice, and touches upon some of the problems which they suggest.

ARCH. GEIKIE

Some years ago Prof. Prestwich announced the discovery of beds of Pliocene age at a height of over 600 feet on the North Downs (*Quart. Journ. Geol. Soc.*, vol. xiv. p. 322). The bad preservation of the fossils, however, led Mr. S. V. Wood, who examined them, to mark all the species as doubtful, though he was inclined to agree that they were probably Pliocene. Owing to the unsatisfactory nature of the palæontological evidence, and apparently also to an accidental mixture of Eocene fossils from other localities, this discovery has been discredited or ignored, though Prof. Prestwich himself has always maintained its accuracy.

Recently, while preparing an account of the British Pliocene beds for the Geological Survey, it has been necessary for me to examine any outlying deposits which have been considered to belong to that period. For this purpose I paid a second visit to Lenham, near Maidstone, having several years ago examined that locality with no satisfactory result, owing to the obscurity of the sections. A number of blocks of fossiliferous ironstone were obtained from pipes in the Chalk—just as the original specimens were found. These were brought to London, carefully broken up, and impressions taken from the moulds of fossils with which the ironstone was filled. By this means a series of casts was obtained very much better than the obscure impressions so doubtfully determined by Mr. S. V. Wood. The result of the examination of these fossils has thoroughly corroborated Prof. Prestwich's view, for there is not a single Eocene species among them. With two or three exceptions they are all known Pliocene forms; some are new to England, though occurring in France and Italy.

The species obtained were the following, my determinations in every case being indorsed by Messrs. Sharman and Newton:—

Pyrula reticulata, Lam.	*Arca lactea*, Linn.
Nassa prismatica, Broc.	,, *diluvii*, Lam. (new to England)
Ringicula ventricosa, J. Sow.	*Leda*, sp. (not *L. myalis*)
Pleurotoma turrifera (?), *Nyst.*	*Nucula*, sp.
,, *consobrina* (?), Bellardi (Upper Miocene)	*Diplodonta rotundata*, Mont., oval var.
,, *Jouanneti* (?), Des M. (Upper Miocene)	*Cardium*, 3 sp.
Trophon muricatus, Mont.	*Cardita senilis*, Lam.
Cerithium tricinctum, Broc.	*Astarte Basteroti*, Laj.
Turritella incrassata, J. Sow.	,, *gracilis*, Munster
Fusus, sp. (= an undetermined Red Crag species)	*Tapes*, sp.
Scalaria clathratula, Turt.	*Gastrana fragilis*, Linn.
Margarita trochoidea (?), S. V. Wood, var.	*Tellina donacina*, Linn.
Trochus millegranus, Phil.	*Mactra*, sp.
Natica, 2 sp.	*Lutraria elliptica*, Lam.
Bulla lignaria, Linn.	*Teredo* (?)
Ostrea, sp. (young)	*Terebratula grandis*, Blum.
Pecten, 2 sp.	*Lunulites* (?)
Pectunculus glycimeris, Linn.	*Balanus*, sp.
	Diadema (?)

The first thing that strikes one in this list is that the whole of the living species are southern forms, and the nearest allies of the extinct species belong to much warmer seas than ours. This, and the general character of the fauna, and proportion of extinct species, lead me to refer the beds to the Older Pliocene period, and to correlate the deposit with our Coralline Crag and with the Lower Crag or Diestian of Belgium. The fossiliferous clay of St. Erth, in Cornwall, I also think is of about the same age, and not newer.

Some curious questions are raised by these recent discoveries, and by others equally remarkable in Belgium and Holland. We now find that, instead of the Older Pliocene period being one of elevation, there must have been wide-spread submergence over great part of Western Europe. A few years since the Coralline Crag was generally considered to be our only representative of the period, and as it did not rise much above the sea-level, it was often assumed that much of the rest of England was dry land. Now it is known that Pliocene beds cap the highest parts of the North Downs, and from the undisturbed position of the shells, unworn, and generally with the valves united, it is evident that the depth of water must have been sufficient to prevent the deposits at the bottom being affected by storms. A subsidence sufficient to allow only 20 or 30 fathoms of water over the highest parts of the North Downs (and the depth in which the Lenham shells lived could hardly have been less) would submerge the whole of the east and south of England, except a few hills.

In Cornwall also there appears to have been a submergence to a considerable depth, for the St. Erth clay was evidently laid down in still water, which would not be found at a less depth than 40 or 50 fathoms in a district exposed like this to the Atlantic swells. The fossils also in that clay point to some considerable depth of water, while the general flattened contour of the country suggests that this district has nearly all been submerged within a comparatively recent period. The lower parts of Cornwall form a smooth, undulating country, out of which rise abruptly the higher hills. Round one of these hills—St. Agnes Beacon—coarse sand is found at a high level. This is probably a beach deposit of the same age as the clay at St. Erth, though all fossils have now disappeared from it. Cornwall seems at that period to have formed a scattered archipelago like the Scilly Islands.

Among the hard rocks of Cornwall denudation does not appear much to have changed the general configuration of the country since the Older Pliocene period. Moreover the Pliocene deposits were probably never continuous or thick, but merely formed patches in sheltered places and round the shores, the rest of the sea-bottom being rocky. In the south-east and east of England, however, the case was different, for the rocks of that region are soft and much more easily denuded. The position of the Lenham beds, at the very edge of an escarpment, over 600 feet above the sea, indicates that the great valleys of the Thames and Weald have to a large extent been excavated since Pliocene times.

On the other hand, the question arises whether the elevation of the Wealden axis was still in progress during the Pliocene period. That the greater part of this enormous disturbance had been completed before that period seems proved by the absence of any Pliocene beds in the Hampshire or London basins in the synclinal folds parallel with the Weald. But there is possibly evidence of less violent movements of upheaval in the different levels at which Older Pliocene beds now occur. We find the Coralline Crag slightly above the present sea-level, the Lenham beds 610 feet above, while at Utrecht deposits of about the same age are found at a depth of at least 1140 feet below the sea, and their bottom has not yet been reached.[1] If the movements in North-Western Europe have been regular and of equal amount everywhere, then, taking the Downs near Lenham as the starting-point, with a depth of 20 fathoms, we should expect to find in the Coralline Crag the deposits and fauna of 120 fathoms, and at the bottom of the well at Utrecht those of 310 fathoms.

A Pliocene fauna of over 300 fathoms would be most interesting to examine, but of such a fauna no trace has yet been detected anywhere in North-Western Europe. The Pliocene deposits, though now at such different levels, are shown by their included fossils to have been laid down in about the same depth of water. Though differing much in mineral composition at the various localities, they nevertheless agree as closely in regard to their shells as the very different nature of the sea-bottoms would lead us to expect. The whole 1143 feet of Pliocene and Pleistocene beds at Utrecht consists of essentially shallow-water deposits, pointing to a continuous depression.

Were we to assume that the present positions of the Pliocene deposits of the north-west of Europe represent the relative depths at which the beds were originally laid down, a curious difficulty would present itself in any attempt to compare the Pliocene and recent sea-bottoms. Any deep depressions in the seas around England are now filled with cold water and contain an Arctic fauna. In similar depths during the Pliocene period one would expect to find a similar fauna, unless there existed, as in the Mediterranean, a barrier to cut off the Polar currents, or unless there was at that time no cold area at the Pole. Neither of these explanations seems sufficient, and it is more probable that those geologists are right who maintain that the direction of the movements in areas of subsidence or elevation remains the same during long periods. Holland may thus have undergone continued, though probably intermittent, depression since the early part of the Pliocene period; thus allowing the accumulation of a great thickness of shallow-water Newer Tertiary beds. The axis of the Weald, including the Downs near Lenham, has been correspondingly elevated. Suffolk was little affected, and the deposits were therefore, in that district, thin and largely of organic origin.

I do not bring forward these conclusions as to elevation and submergence as indisputable facts, but merely as the results of my recent studies in the Pliocene beds at home and on the Continent. Any day new discoveries may profoundly modify our views, but the curious facts will remain, that Northern Europe has yielded only a shallow-water Pliocene fauna, and no trace of boreal outliers such

[1] See Dr. J. Lorié, "Contributions à la Géologie des Pays-Bas," *Archives du Musée Teyler*, ser. ii. vol. ii. part 3.

as occur in our existing seas—and this notwithstanding the very different levels at which Pliocene beds now occur

CLEMENT REID

EARTHQUAKE-RECORDERS FOR USE IN OBSERVATORIES

TWO years ago the writer described in NATURE (vol. xxx. pp. 149 and 174) some of the instruments which he had designed and used in Japan for the registration and analysis of earthquake movements. In response to applications from the directors of several observatories, who wished to add seismometric apparatus to their other equipment, arrangements were some time ago made with the Cambridge Scientific Instrument Company for the manufacture of instruments by aid of which the observation of earthquakes might become part of the ordinary work of any meteorological or astronomical station where such movements occasionally occur. In the design of these seismographs the object has been kept in view of

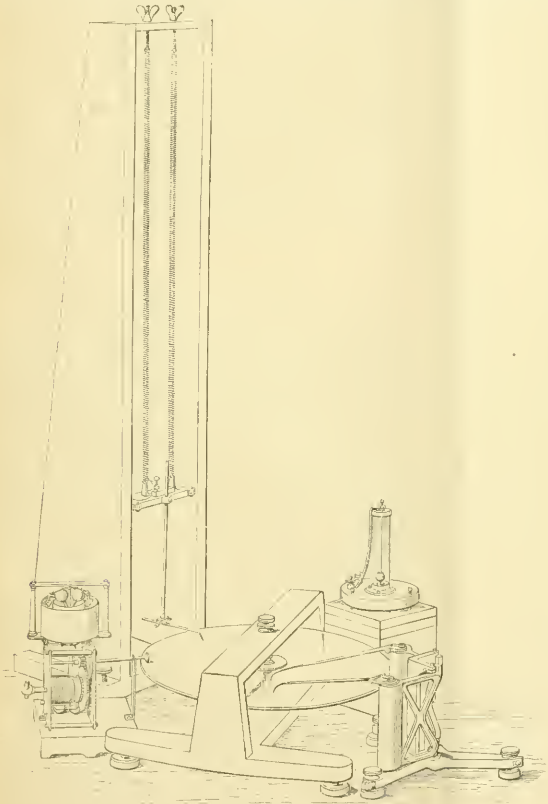


FIG. 1.—Complete three-component seismograph, for motions in all direction.

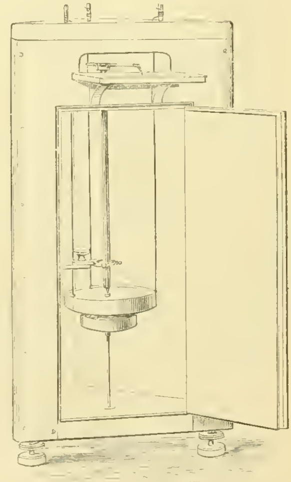


FIG. 2.—Duplex pendulum seismograph, for horizontal motion.

making them easily capable of use by observers who have not made seismometry a special study. They are entirely self-recording, and require little attention during the long intervals which must, in most situations, be expected to elapse between one period of activity and the next.

One group of instruments is arranged to give a complete record of every particular of the movement by resolving it into three rectangular components—one vertical and two horizontal—and registering these by three distinct pointers on a sheet of smoked glass which is made to revolve uniformly by clockwork. A single earthquake always consists of many successive displacements of the ground; hence the record traced by each pointer on the moving plate is a line comprising many undulations, generally very irregular in character. The amplitude, period, and form of each of these are easily measured, and by compounding the three we obtain full information regarding the direction, extent, velocity, and rate of acceleration of the movement at any epoch in the disturbance.

This group of instruments is shown in Fig. 1. In the centre is the plate of smoked glass, which gets its motion

through a friction-roller from a clock furnished with a centrifugal governor, acting by fluid-friction, and balanced so that its speed is not sensibly affected by the shaking of the ground. The clock is started into motion by means of a Palmieri seismoscope, which appears in the figure behind the plate, on the right. This is a small common pendulum whose bob carries at the bottom a piece of stiff platinum wire that projects into a recess in a cup of mercury below—the recess being formed by an iron pin standing lower than the surface of the surrounding mercury. On the slightest shaking of the ground, contact with the edge of the mercury takes place, and this closes a circuit which releases an electro-magnetic detent and starts the clock. This occurs during the preliminary tremors which are usually found in advance of the main movements of an earthquake. The same circuit starts another clock (of the escapement type) which fulfils two functions. It marks time on the revolving plate during a part of the first revolution, and then continues to go as an ordinary clock, so that, by inspecting its dial afterwards, the interval which has elapsed from the occurrence of the earthquake is known, and the date of the shock in hours and minutes is thus determined with as much precision as the phenomenon admits of. This part of the apparatus is omitted from the figure. The two horizontal components of motion are recorded by a pair of horizontal pendulums, set at right angles to each other, but with their indices inclined so that they write side by side on one radius of the plate. The pendulums are supported on a single stand, but with independent adjustments for position and stability. Each has two pivots, consisting of hard steel points, which turn in sapphire centres. At the pivots and at the tracing-points every effort has been made to avoid friction. The indices are of aluminium, and a part of their weight is taken by springs (not shown in the figure), so that their pressure on the plate may be no greater than is necessary to produce a trace on the sooty film. The vertical component of motion is recorded by the instrument which appears behind the clock. A massive bar, free to move vertically about a horizontal axis, is held up by a pair of long spiral springs. Its equilibrium is made nearly neutral by applying the pull of the springs at a suitable distance below the horizontal plane through the axis of support, in the manner described in the article to which reference has already been made. A bell-crank lever with a jointed index gives a multiplied trace of the apparent vertical oscillations of the bar, which correspond to vertical displacements of the ground. In this instrument, as in the others, sapphire centres are used to minimise friction.

Records inscribed on the plate are preserved by varnishing the plate, and using it as a "negative" to print photographs. The motion, as recorded, is magnified to an extent which experience of Japanese earthquakes has shown to be desirable in dealing with disturbances ranging from those which are just recognisable as earthquakes up to those which are to some extent destructive. For great earthquakes, separate apparatus of the same type is designed, in which the multiplying indices are dispensed with, and the scale and style of the other parts are considerably modified.

Another and distinct instrument, also manufactured by the Cambridge Company, is the duplex pendulum seismograph, shown in Fig. 2. A massive bob is hung by three parallel wires from the top of a three-cornered box, and is reduced to nearly neutral equilibrium by being coupled by a ball-and-tube joint to the bob of an inverted pendulum below it. The two form a system which can be made as nearly astatic as is desirable, and so furnish a suitable steady-point for the horizontal part of earthquake movement in any azimuth. The motion is magnified and recorded by a vertical lever geared to the upper bob by a ball-and-tube joint, supported on gimbals from a bracket fixed to the box, and furnished with a jointed index which writes on a fixed plate of smoked glass. Records of the kind which the duplex pendulum gives are of course incomplete in two important particulars: they show nothing of the vertical motion (which, however, is usually a comparatively small part of the whole), and they show nothing of the relation of *time* to displacement throughout the disturbance. But they exhibit very clearly the change of direction which the movements undergo, and the actual direction taken by any pronounced element of the shock. The writer has recently learnt from his former assistant, Mr. Sekiya, now Professor of Seismology in the University of Tokio, that as many as fifteen of the duplex pendulum seismographs are in use by official and private observers in Japan.

The instrument shown in the figures are now on view at the Edinburgh International Exhibition (Court 21, No. 917). Similar sets are being made for the Lick Observatory, California, the Ben Nevis Observatory, and other places. It is scarcely necessary to add that they show the high finish and perfection of workmanship characteristic of the Cambridge Company's manufactures. To Mr. Horace Darwin the writer is especially indebted for a number of suggestions the adoption of which has contributed much to scientific accuracy in details and simplicity in structural arrangements.

J. A. EWING

THE INSTITUTION OF NAVAL ARCHITECTS AT LIVERPOOL

THE Institution of Naval Architects departed this year from their almost invariable custom of holding meetings in London, and had a most successful series of meetings at Liverpool. The papers read were few in number, but they were of special, and, in some cases, unusual interest. The meetings were attended by a large number of the Members of the Institution, as well as Liverpool scientific men, shipowners, underwriters, engineers, and others interested in the subjects of discussion. A local paper was read by Mr. G. F. Lyster, C.E., the Engineer to the Mersey Dock and Harbour Board, upon the Docks of Liverpool; Prof. F. Elgar read a paper upon "Losses of Life at Sea"; Mr. B. Martell upon "The Carriage of Petroleum in Bulk on Over-sea Voyages"; Mr. W. John upon "Atlantic Steamers"; and Mr. W. Parker on the "Progress and Development of Marine Engineering."

Prof. Elgar's paper upon losses at sea has attracted much attention. It contains a general analysis of the losses that happened during the triennial period that has recently caused so much controversy as to whether loss of life at sea is increasing or not, viz. the three years 1881-83. Details are given, in a set of tables appended to the paper, of the steamers and iron sailing ships belonging to the United Kingdom, of and above 300 tons gross register, that were reported to the Board of Trade as foundered or missing during the five calendar years 1881-85. The facts contained in these tables show clearly the great advantage it would be to the shipping community if such information were published periodically in a clear and convenient form. Probably no documents that emanate from any Government Department are more bewildering, or more difficult to extract any tangible information from, than the voluminous and complicated returns of wrecks and casualties, and of lives lost at sea, that are published annually by the Board of Trade. We hope that the attention of the Royal Commission now sitting upon Loss of Life at Sea, has been forcibly directed to the many imperfections and the comparative uselessness of the present published returns that profess to deal with these matters; and that one of the Committee's recommendations will be that something should be done to make them clear and instructive.

There is another cognate matter which we hope will also be dealt with satisfactorily by the Royal Commission, viz.

the manner in which wreck inquiries are now conducted. As matters stand, the evidence obtained at those inquiries, and the rulings that are given by the Courts, have no scientific or practical value. There is no class of quasi-scientific literature that we know of which contains more bad science than is to be found in the rulings of the Wreck Inquiry Courts. Prof. Elgar puts the case very mildly when he says that the returns of the Wreck Inquiry Courts are not all that might be wished as regards the publication of facts connected with losses, and that they are often imperfect and erroneous where difficult technical points are involved. He adds that "this probably arises from the perfunctory character of many of the inquiries; as it has been explained by the Wreck Commissioners that the number of inquiries, and the distant places at which they are sometimes held, often make exhaustive inquiries impossible."

Nothing can be held to excuse imperfect inquiries into the causes of those losses at sea which have formed the subject of so much public discussion and excitement, and respecting which a Royal Commission is now sitting; and nothing would be more likely to promote an increase of knowledge, or to lead to the adoption of precautions for preventing losses, than thorough and trustworthy inquiries into the causes of those losses that so frequently occur.

The conclusions arrived at by Prof. Elgar in his paper, as the result of an examination of the analysis contained in it, are the following:—

(1) The shifting of cargoes is one of the chief causes of the foundering of steamers and iron sailing ships at sea, independently of mere depth of loading.

(2) Dangerous shifting of grain sometimes takes place through hasty and imperfect stowage, inefficient shifting-boards, or weakly-constructed end bulkheads, or through the omission to fit end bulkheads, where such are required on account of the density of the cargo; and dangerous shifting of coal sometimes takes place, because it is carried in compartments that are not fitted with shifting-boards.

(3) Many steamers carrying grain and coal cargoes—notably the class of narrow three-decked steamers built several years ago—are vessels that have insufficient stiffness when fully laden, to resist heeling to a dangerous angle, in the event of cargo shifting or of water getting below.

(4) The effect upon such vessels of the shifting of cargo, and of water below, is generally to hold them over at a considerable angle of inclination, but not to completely capsize them.

(5) Pumping power at the bilges is often an essential condition of preventing loss in such circumstances, and of getting a vessel righted.

(6) The stability of these vessels when laden with the various cargoes they are likely to carry, should be completely determined by calculation before they are sent to sea; and clear instructions, based upon the information so obtained, should be framed for the guidance of those who are responsible for their loading. Such instructions should include particulars of the empty spaces to be left in the 'tween decks, or of the weight of ballast to be carried, or both, for each class of cargo.

(7) All the authentic particulars procurable of ships that have foundered and are missing, and of the circumstances and the manner in which the foundered ships were lost, should be collected and published periodically for the information of the shipping community.

(8) The losses of steamers through the shifting of cargoes seem to be chiefly among the narrow steamers of the three-decked type that were built several years ago. The steamers of that type that have recently been built have more beam and much greater stability than those formerly built, and it may be confidently hoped that the attention which has been given to this matter of late, and the improvements that have consequently been introduced into this type of vessels, will lead to a diminution of losses among them.

Mr. Martell's paper upon "The Carriage of Petroleum in Bulk on Over-sea Voyages" deals very fully with the history of the carriage of petroleum by sea, and the special precautions that are necessary to enable it to be carried safely and economically in ships. Besides those points, however, that are special to the treatment of petroleum as a cargo, there are others which naturally grow out of a consideration of the subject. There is, for instance, the important question of the use of liquid fuel for marine propulsion. The mechanical difficulties involved by this have now been overcome, so that *astatki*, the residuum of crude oil, might be profitably used as fuel for steamers employed upon comparatively short voyages. It is largely and successfully used for marine propulsion on the Caspian Sea, where oil is very cheap and coal is very expensive. The chief obstacle at present to extending the use of this fuel is its cost. The price of *astatki* at Baku varies from 4*d.* to 1*s.* 3*d.* per ton; the carriage by rail to Batoum raises the cost at that port to about 1*l.*; and after adding freight charges for bringing it to this country, its total cost on delivery would, according to Mr. Martell, be not less than 2*l.* 2*s.* From these figures it must be evident that while the best steam coal can be shipped at Cardiff for about 9*s.* per ton, liquid fuel cannot be economically used in competition with it. As the cost of transport of liquid fuel from Baku to Batoum becomes reduced—and this can only be a question of time—there is no doubt that liquid fuel will come into general use for local steamers, and most likely for many steamers trading in the Mediterranean.

Mr. W. John refers, in his paper upon "Atlantic Steamers," to several matters that are of importance to the travelling public. He advocates the adoption of twin-screws in first-class Atlantic steamers as a provision against total breakdown in the event of a shaft, or of any other vital part of the propelling machinery, giving way. He also advocates a middle-line bulkhead, and greater internal subdivision generally, so that ships may be more safe in the event of a compartment being bilged through collision. Mr. John states that improved designs for the Atlantic passenger steamers of the future now form the subject of work and investigation in the drawing-offices of several shipyards. The developments that are taking place are, doubtless, generally in the direction of providing greater safety against accidents to the hull of the ship, or to the propelling machinery, by means of greater internal subdivision and twin-screw engines. Higher speeds than any yet realised are being contemplated by building purely passenger vessels that will carry no cargo; and many improvements of details, which are in the direction of making the accommodation for passengers more like that furnished by a first-class hotel, are also being devised.

Mr. Parker's paper upon "The Progress and Development of Marine Engineering" forms a supplement to one read by Mr. F. Marshall before the Institution of Mechanical Engineers at Newcastle-on-Tyne, in 1881. Mr. Parker traces the progress that has been made in economy of steam propulsion since the introduction of the triple expansion-engine by Mr. A. C. Kirk in 1874. Mr. Kirk fitted triple expansion-engines to the *Propontis* for Mr. W. H. Dixon, in that year, with boilers designed for a working pressure of 150 lbs. per square inch; but the boilers did not prove satisfactory, and were ultimately removed. The next triple expansion-engines were those of the yacht *Isa*, designed by Mr. A. Taylor, of Newcastle-on-Tyne, in 1877; and those of Messrs. G. Thompson and Sons' steamer, *Aberdeen*, which were constructed by Mr. A. C. Kirk, in 1881, for a steam pressure of 125 lbs. per square inch. The *Aberdeen* was the real pioneer vessel of the triple expansion type of engine; as it was proved in her that triple expansion-engines could be made not only to fulfil all the ordinary conditions of working at

sea, but to effect a great economy in coal-consumption. Since then the new system has come rapidly into use, and shipbuilders and marine engineers are now looking in the direction of triple and quadruple expansion-engines for the economical advantages of high-pressure steam in future ships.

It is estimated that the present triple-expansion marine engine, with 150 lbs. of steam-pressure, has an advantage of from 20 to 25 per cent. in economy of coal consumption, over the ordinary compound engine with 90 lbs. pressure, which it is rapidly supplanting. These results have been achieved through a clear appreciation of the waste of energy which is caused by the alternate heating and cooling of a steam cylinder that takes place in consequence of the difference of temperature at which the steam enters and leaves it. The steam jackets introduced by James Watt as a cure for this were imperfect and wasteful in their action. An effectual remedy has been found as the result of many years of study and experiment by men of science and engineers, by expanding the steam successively in several cylinders, so as to make the variation of the temperature of the steam in each cylinder as small as possible.

NOTES

We greatly regret to announce the death of Mr. George Busk, F.R.S., the well-known surgeon and naturalist, in the seventy-eighth year of his age. We must reserve a detailed notice of Mr. Busk's life and work.

Geologists will be sorry to hear of the death of Mr. Gerrard Kinahan, son of the well-known geologist of the Geological Survey of Ireland. Last October he accepted an appointment in the service of the National African Trading Company. The last letter received from him gave an interesting account of his explorations up the southern tributaries of the Niger. He died on May 23 from a wound with a poisoned arrow in a fight with the native tribes at a place called Anyappa. His training as a chemist and geologist at the College of Science in Dublin and also at the School of Mines in London had thoroughly qualified him for original research, and his quietly enduring temperament and kindliness of nature augured a most successful scientific future, whether at home or abroad. But he has been cut down on the very threshold of his career—another young victim to the dangers of African exploration.

The death is announced of Dr. R. J. Mann, F.R.C.S., aged sixty-nine. Dr. Mann was for three years President of the Meteorological Society, and was a Member of the Astronomical, Geographical, Photographic, and other Societies. He gave up his medical practice to take a Government appointment in Natal, where he served as head of the Education Department and Medical Officer for many years. On his return, about 1864, he became Emigration Agent for the Colony, and when, some ten years later, he resigned that post, he devoted himself to his favourite scientific pursuits. He was a popular and prolific writer. The protection of buildings from lightning was a subject on which he wrote a good deal, and for which he did much valuable work.

M. Chevreul, the illustrious French chemist, will complete his hundredth year on Monday next. A grand *fête* at the Museum in honour of the occasion is being organised. Delegations from foreign countries as well as from the provinces are expected. In one of the *salles* at the Museum there is to be an exhibition presenting a *résumé* of the scientific labours and discoveries of M. Chevreul. The banquet will be at noon, in order that the famous centenarian may himself be present.

Our Vienna correspondent writes:—"Dr. von Frisch, having recently experimented on preventive inoculations for hydrophobia, has made a preliminary communication to the Vienna Academy of Sciences, in which he states that it was impossible for him to prevent the breaking out of rabies by means of Pasteur's method if the infecting virus (of at least fourteen days' incubation) were administered to previously healthy animals by trepanning. This latter method of artificial infection of animals Dr. von Frisch suggests to be the only safe one. He made his experiments on rabbits and on dogs. At first sixteen healthy rabbits were trephined, and the virus (of sixteen days' incubation) was injected under the dura mater. Fifteen of these rabbits were then subjected at intervals to the usual preventive inoculations, the remaining one not being inoculated. All these animals except one, which, as Frisch believes, was not sufficiently infected, died between the fourteenth and thirty-third day after infection, with symptoms of rabies, and if particles of their spinal cord were injected into healthy rabbits, the latter also became rabid. Similar experiments were then made on dogs, and with the same results. But a series of rabbits infected by subcutaneous injection of the virus, and then treated by Pasteur's method, continue healthy up to the present time."

The Grosvenor Museum for Chester and North Wales was opened at Chester on Monday by the Duke of Westminster. The new museum is intended as the home of four influential local societies—namely, the Chester Society of Natural Science, founded by Charles Kingsley; the Chester Archæological Society, which is closely associated with the name of the late Dean Howson; the Chester Schools of Art; and the Chester Science Classes. The first floor is devoted entirely to science, except one room to be used as a laboratory, a committee-room, and a model-room for the school of art. The upper floor is entirely devoted to the school of art. In the several rooms there are exhibited specimens illustrative of the natural history of the district, water-colour drawings, collections of antiquities, oil paintings, wood carvings, a collection of pictures by members of the Art Club, appliances for science teaching, memorials of Canon Kingsley and Dean Howson, a collection of objects illustrative of Oriental art, a magnificent collection of tapestry, choice embroidery, lace, porcelain, textile fabrics, &c.

The autumn congress of the Sanitary Institute of Great Britain will be held in the city of York on September 21 and following days, under the Presidentship of Sir T. Spencer Wells, Bart.

The summer meeting of the Institution of Mechanical Engineers will be held on Tuesday morning, August 17, and Wednesday morning, August 18, at 25, Great George Street, Westminster. The following papers have been offered for reading and discussion after the address by the President:—Experiments on the steam-jacketing and compounding of locomotives in Russia, by M. Alexander Borodin, of Kieff; on the working of compound locomotives in India, by Mr. Charles Sandiford, of Lahore; description of a portable hydraulic drilling-machine, by M. Marc Berrier-Fontaine, of Toulon; description of the Blackpool electric tramway, by Mr. M. Holroyd Smith, of Halifax; on triple-expansion marine engines, by Mr. Robert Wyllie, of Hartlepool.

The French Association for the Advancement of Science begins its annual meeting to-day at Nancy.

The lectures recently delivered at Oxford by Prof. Sylvester on his "New Theory of Reciprocants," will appear in the coming numbers of the *American Journal of Mathematics*. The lectures are presented in quite simple style, and will be exceedingly interesting to all students of the modern algebra, or, more accurately, of the theory of invariants. The first eight or nine lectures will appear in the forthcoming number of the *Journal*, vol. viii. No. 3.

A Society for the study of anthropology has been founded in Bombay. Before the departure of the East Indian mail the

second ordinary meeting of the Society had been held, and Mr. E. Tyrrel-Leith, the President, was able to give a very satisfactory account of its prospects.

THE report of the Directors and Secretary of the City and Guilds of London Institute on the technological examinations of the present year has just been issued. It states that there is a large increase in the number of candidates who presented themselves, and a satisfactory increase in the number of those who have passed. In 1885, 3968 candidates were examined, of whom 2168 passed. In 1886, 4764 candidates were examined, of whom 2627 have passed. Examinations were held this year in 48 subjects. Applications for examination were received in all the 49 subjects, but, as only one candidate presented himself in sugar manufacture, in accordance with the regulations of the Institute no examination was held in this subject. Last year examinations were held in 42 subjects. The subjects in which there has been the greatest increase in the number of candidates are mechanical engineering, plumbers' work, electric lighting, and gas manufacture. One new subject was this year added to the list—namely, brickwork and masonry, in which 99 candidates came up, and of these 57 satisfied the examiners. In addition to the practical examinations in weaving and pattern designing, in metal-plate work, in carpentry and joinery, and in mine-surveying, which had been previously held, a practical examination was held this year for the first time in typography. Seventy-seven candidates presented themselves for this examination, of whom 32 have succeeded in obtaining a certificate. From the returns furnished in November last, it appears that 7660 persons were receiving instruction in the registered classes of the Institute, as compared with 6396 in the previous year. This increase is due partly to increased attendance at some of the classes, and partly to the formation of new classes, the number of classes in connection with the Institute having increased from 263 in the Session 1884-85 to 329 during the past year. These classes were held in 116 different towns of the United Kingdom. 203 prizes have this year been awarded, being an increase of 26 on those granted last year. These awards include 180 money prizes, 65 silver and 138 bronze medals, of which 62 are in the honours grade and 141 in the ordinary grade. Last year there were 177 money prizes, 51 silver and 126 bronze medals.

THE Norwegian Meteorological Institute of Christiania has adopted an ingenious plan for disseminating its weather reports among the farmers, fishermen, &c. Thus, on the brake of every train departing from the capital to any part of the country after 3 p.m. a signal is exhibited indicating the weather to be expected for the ensuing twenty-four hours. These signals are very simple, consisting of red and white triangles, squares, and balls, each of which, or several combined, have their meaning; a white ball, for instance, "fine weather," &c. These signals will also be displayed from the masts of several coasting steamers. The arrangement is in force from July 1 to October 1.

ACCORDING to the official returns of the Minister of Education in Prussia the number of students in philology and the kindred subjects of philosophy and history in that kingdom—now for a considerable time known to be sensibly on the decrease in the field of the ancient classics—has altogether steadily declined from Michaelmas 1881 to Easter 1885. The number of students of philosophy, philology, and history at the Universities of Prussia amounted in the winter session of 1881-82 to 2522, in the summer session of 1882 to 2535, in the winter session of 1882-83 to 2504, in the summer session of 1883 to 2398, in the winter session of 1883-84 to 2311, in the summer session of 1884 to 2258, and in the winter session of 1884-85 to 2181. Within a period of $3\frac{1}{2}$ years, therefore, the decline in the number of students of philosophy, philology, and history has been about 14 per cent.

A LARGE specimen of the lizard tribe imported from Japan into the Colonial and Indian tropical aquarium lately died in the act of extruding her eggs, numbering eight. One of her feet that had become broken during her existence was found to have grown again. A further consignment of turtles has arrived at the aquarium together with a number of eggs, which have been placed in the Chelonian hatchery for incubation. Some fine specimens of foreign oysters have also arrived.

THAT kingfishers possess a remarkable instinct to avoid those fish proving harmful to them is exemplified by the following incident. A correspondent informs us that being infested with these birds he set traps for them in two of his ponds, one containing minnows and the other perch. A capture was effected daily where the minnows were located, but on no occasion was a kingfisher caught near the perch pond. The latter fish are injurious to birds.

M. GASTON TISSANDIER in a recent number of *La Nature* describes the efforts made by himself and his brother to pursue the subject of aërial photography. For this purpose they sought the assistance of M. Paul Nadar, whose father made the first attempts twenty-eight years ago, to take photographs from balloons. An ascent was made on July 2 from Auteuil, the descent taking place after a voyage of about six hours at Ségrie in La Sarthe, the length of the journey being about 180 kilometres. The maximum altitude exceeded 1700 metres. During the voyage M. Nadar succeeded in executing thirty instantaneous photographs, a dozen of which are, M. Tissandier says, unquestionably the most perfect yet obtained from a balloon. These were taken at various altitudes, ranging from 800 to 1200 metres. They were perfect in all details, but lose by reproduction by heliogravure. Those taken at 1200 metres it has been found impossible to reproduce at all, as they lose all their fineness in the process. The apparatus was placed in different positions on the edge of the car, sometimes being almost vertical, sometimes inclined so as to form with the horizon an angle varying from 25° to 45°. The time in each case was 1/250th part of a second. M. Nadar has enlarged some of the photographs with the new Eastmann paper with remarkable success. It is obvious from the illustrations given in *La Nature* that the photographs have suffered in the reproduction, the details being slightly blurred and indistinct, but the streets, principal houses, gardens, &c., are perfectly clear in the two pictures which were taken at altitudes of 800 and 1100 metres respectively.

A RECENT number of the *British North Borneo Herald* contains a report on the climate of North Borneo, by Dr. Walker, the principal medical officer, which is interesting as being the first scientific account of the meteorology of this recent addition to the British Empire. The climate, Dr. Walker says, is noticeable for nothing more than for its equability and the absence of extremes. The temperature, rainfall, winds, natural phenomena generally, and the diseases are, for a tropical country, of the most mild and temperate types. The country is visited by the regular monsoons at the ordinary times; the rainfall near the coast, according to records kept for seven years past, ranged from 156·9 to 101·26, and averaged 124·34. The temperature recorded at the coast has ranged between the extremes of 67°·5 and 94°·5; better arrangements are now being made for observing temperatures. The absence of tornadoes and earthquakes is to be noticed. The only indication at the present day of the existence of volcanic action is a hot spring, which is reported to exist on an island near the coast. The bulk of the report is occupied with information of a more medical kind, such as the chief diseases, and their respective effects on the various races, native and immigrant, in North Borneo; sanitaria, and the like.

AT a recent meeting of the Anthropological Institute at the Colonial Exhibition, Mr. Swettenham, of the Straits Civil

Service, read a paper on the races of the Straits Settlements. Of the Malays he said it was unlikely that they could be indigenous to the peninsula, but where they came from, whether Java, Sumatra, or elsewhere in the Archipelago, is a question which has never yet met with a satisfactory answer. Their own tradition is that they had a supernatural origin, and that they crossed from Sumatra. Up to about 1250 they were pagans, or followed some form of Hindoo worship, but about that time their conversion to Mohammedanism began. Up to this time, Malay, of all the Sumatran languages, had no written character of its own, but then Perso-Arabic characters were introduced, and many Arabic words found their way into the Malay language. Relics of Hindoo superstitions are still found amongst the Malays and Negritos of the peninsula, and the customs even now observed, especially in Perak, on certain occasions savour strongly of devil-worship. As to the Negritos, Mr. Swettenham remarked that the most complete collection of the clothing, weapons, and ornaments of these people ever yet brought together will be found in the Straits Settlements Court of the Exhibition. His own observation led him to divide the Negritos into two widely different sections—the Sakai and the Semang—the former a people of moderate stature and large bones, rather fairer in complexion than Malays, with long unkempt wavy hair standing straight out from their heads; the latter small and dark, with black frizzy hair close to their heads, like that of the Negro races. Some writers have found in the comparison of languages a connecting link between the Negritos of various tribes and Malays, and believe the former to show traces of Melanesian blood. The paper contains many interesting observations on the manufactures of the Malays, and on the customs of these and the Negritos.

We have received two excellent memoirs of American men of science, recently read before the National Academy. The first is by Mr. J. D. Dana, and deals with the life and work of Arnold Guyot; the second, the life of Jeffries Wyman, by Mr. A. S. Packard. In each case the work appears to be done admirably, and a list of the writings of the subject of the memoir is appended. At the beginning of his account of Guyot, Mr. Dana notices as a remarkable fact in the history of American science, that, forty years since, the same Republic of Switzerland lost, and America gained, three men of science who became leading men of the country in their several departments —Agassiz in zoology, Guyot in physical geography, and Lesquereux in palæontological botany. A fourth—De Pourtalès, who accompanied Agassiz—also deserves prominent mention, for he was the pioneer of deep-sea dredging in America. The Society of Natural Sciences of Neuchâtel lost all four to the United States between 1846 and 1848. The memoir of Wyman (the list of whose papers, &c., reaches the number of 1875) concludes with a very touching portrait from the pen of Dr. Oliver Wendell Holmes, and an ode by Mr. Russell Lowell.

Nos. 25 and 26 of *Excursions et Reconnaissances*, which have just reached this country from Saigon, do not contain so much matter of general interest as usual. In both M. Landes continues his collection of the folk-lore of Annam, under the title "Contes et Légendes Annamites." In No. 26 M. Silvestre, an official connected with the administration of native affairs, has a short paper on the Chao tribe of the Hung-hoa province of Tonquin, which is interesting as showing incidentally the vast number of various tribes, or tribes under various names, which exist in French Indo-China. How far they are related to each other and to some earlier race of which they are the fragments is one of the ethnological problems of the future. Lieut. Campion describes a voyage made in a despatch-vessel to various islands lying off the coast of Cochin-China, which have hitherto been unvisited by Europeans; and the indefatigable M. Aymonier continues the publication of an encyclopædic work on Annam.

A recent number of the *American Meteorological Journal* contains an article on the notorious dust-storms of Pekin. These occur in the dry season, especially in winter and early spring. They come on at irregular intervals, perhaps six or eight times in the season, and last about three days. The wind is westerly, most often north-west, and blows fresh or high. The condition of the streets of Pekin, evil as that is, would not account for the heavy clouds of dust that come down with the storm. The mouth and eyes have to be protected from the fine dust, which penetrates the closest room, and makes food to taste gritty. This abundant dust is spread over a large area. It extends eastward from Pekin to the sea, and south-eastward it regularly descends as far south as the Yellow River, and sometimes Shanghai, 10° of latitude away. The writer of the paper says this vast quantity of dust must come from the great deserts of Mongolia. A series of observations during one of these storms showed a fall in the thermometer when it came on, and a rapid change in the barometer, which rose from 786 mm. to 797 mm. making several rises and falls of less magnitude in the meantime. The clouds, which the day before had been unbroken, rapidly cleared away; the sun was so obscured that it could be inspected by the naked eye; it was also set in a ring. The wind showed diurnal variations, the air was dry, and one had a feeling of malaise and nervousness. After the wind went down, the barometer remained high for a day or two, and on its descent there was another, but much less marked, dust-storm. The storm thus appears to have been a gale accompanying an area of high pressure, which came from the Desert of Gobi and travelled eastward. The dryness of the wind and its abundant dust were in part due to this desert, which lies west and north-west of Pekin, and is not far away. In his great work on China, Richthofen discusses the geological effects of these storms, which are observed throughout the south and west of the Desert of Gobi, and further west are much worse than at Pekin.

A late number of the *Journal* of the Asiatic Society of Bengal contains a short memorandum by Prof. Pedler, of the Presidency College, Calcutta, on certain experiments which he has made on the corrosion of the lead linings of Indian tea-chests —a subject of considerable importance in more senses than one. His conclusions are that tea properly manufactured in the ordinary way has no power to corrode lead; but if unseasoned and damp wood is used for the boxes, corrosion of the lead is almost certain, some varieties of wood acting more violently than others. Even with seasoned wood, if it becomes saturated with water, and be then placed in favourable circumstances of heat and moisture, corrosion takes place. The active agent, he thinks, does not exist ready formed in unseasoned wood, but is produced by a secondary action from the constituents of the wood. The corrosion is not due usually to contact action between the lead and the wood, but a volatile substance is gradually produced from the unseasoned wood. The corroding agent is usually acetic acid in the presence of moist air and carbonic acid, but other acids of the same series are sometimes produced and also act on the lead, and in the case of butyric and valeric acids the incrustation is of a greenish yellow, while that from acetic acid is whitish or yellowish. The lead being corroded by these acids, which are produced by the decomposition of substances known to be present in the woods, the tea takes up the disagreeable odour of the latter after they have undergone the change in which acetic, butyric, and the other acids are formed, and will thus become deteriorated.

The additions to the Zoological Society's Gardens during the past week include a Common Fox (*Canis vulpes* ♂), British, presented by Mr. J. W. Morgan; a Spotted Ichneumon (*Herpestes nepalensis*) from Nepal, presented by Mr. Herbert W. Brown; a Common Polecat (*Mustela putorius*), British, presented by Mr. William Buckley; a Red-crested Cardinal (*Paro-*

aria cucullata) from South America, presented by Mr. W. E. Ayerst; four Florida Tortoises (*Testudo polyphemus*) from Florida, presented by Mr. Hugh Bellas; a Common Viper (*Vipera berus*) from Hampshire, presented by Mr. Gerald Waller, F.Z.S.; a Brown-throated Conure (*Conurus æruginosus*) from South America, a Roseate Cockatoo (*Cacatua roseicapilla*) from Australia, a Macaque Monkey *Macacus cynomolgus*) from India, deposited; a Bandicoot Rat (*Mus bandicoota*), a Bronze-spotted Dove (*Chalcopelia chalcopilos*), bred in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *AUGUST* 15–21

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on August 15

Sun rises, 4h. 47m.; souths, 12h. 4m. 16·6s.; sets, 19h. 21m.; decl. on meridian, 14° 1′ N.: Sidereal Time at Sunset, 16h. 57m.

Moon (one day after Full) rises, 19h. 12m.*; souths, 0h. 13m.; sets, 5h. 19m.; decl. on meridian, 12° 13′ S.

Planet	Rises	Souths	Sets	Decl. on meridian
	h. m.	h. m.	h. m.	° ′
Mercury	5 13	12 4	18 55	9 7 N.
Venus	2 10	10 11	18 12	21 9 N.
Mars	10 48	15 59	21 10	10 23 S.
Jupiter	8 45	14 44	20 43	0 55 S.
Saturn	1 34	9 40	17 46	21 59 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich)

Aug.	Star	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image
			h. m.	h. m.	° °
17	4 Ceti	6	21 58	near approach	345 —
17	5 Ceti	6	22 3	22 32	10 323
17	B.A.C. 5	6	22 11	23 21	62 278
19	ν Piscium	4½	22 42	23 47	68 261

Aug.	h.	
16	8	Mercury in inferior conjunction with the Sun.

Variable Stars

Star	R.A.	Decl.			
	h. m.	° ′		h. m.	
U Cephei	0 52·2	81 16 N.	Aug. 17,	21 28	*m*
Algol	3 0·8	40 31 N.	,, 20,	0 18	*m*
R Comæ	11 58·4	19 25 N.	,, 17,		*M*
W Virginis	13 20·2	2 47 S.	,, 17,	0 0	*M*
δ Libræ	14 54·9	8 4 S.	,, 21,	20 4	*m*
U Coronæ	15 13·6	32 4 N.	,, 15,	2 21	*m*
W Herculis	16 31·2	37 34 N.	,, 18,		*M*
U Ophiuchi	17 10·8	1 20 N.	,, 17,	2 56	*m*
			,, 17,	23 4	*m*
W Sagittarii	17 57·8	29 35 S.	,, 21,	0 0	*M*
U Sagittarii	18 25·2	19 12 S.	,, 15,	0 0	*M*
β Lyræ	18 45·9	33 14 N.	,, 20,	21 0	m_2
S Vulpeculæ	19 43·7	27 0 N.	,, 21,		*M*
R Sagittæ	20 8·9	16 23 N.	,, 19,		*m*
δ Cephei	22 24·9	57 50 N.	,, 20,	2 0	*m*

M signifies maximum; *m* minimum; m_2 secondary minimum.

TENTH ANNIVERSARY OF THE JOHNS HOPKINS UNIVERSITY

THE tenth anniversary of the Johns Hopkins University at Baltimore was celebrated on April 26 last. Of the addresses delivered on that occasion we reprint two, the second of which reviews the work of this distinguished institution since its foundation. The work of the University in every department of human knowledge is well-known and appreciated in this country, and it is unnecessary to add a word to the address of Dr. Thomas, beyond expressing a cordial hope that the future may, in the words of the very appropriate ode read on the occasion, be

"Smooth course and splendour of the sunset-smiles."

The following is the address delivered by Prof. Henry A. Rowland, Ph.D., who took for his subject "The Physical Laboratory in Modern Education."

"From the moment we are born into this world down to the day when we leave it, we are called upon every moment to exercise our judgment with respect to matters pertaining to our welfare. While nature has supplied us with instincts which take the place of reason in our infancy, and which form the basis of action in very many persons through life, yet, more and more as the world progresses and as we depart from the age of childhood we are forced to discriminate between right and wrong, between truth and falsehood. No longer can we shelter ourselves behind those in authority over us, but we must come to the front and each one decide for himself what to believe and how to act in the daily routine and the emergencies of life. This is not given to us as a duty which we can neglect if we please, but it is that which every man or woman, consciously or unconsciously, must go through with.

"Most persons cut this Gordian knot, which they cannot untangle, by accepting the opinions which have been taught them and which appear correct to their particular circle of friends and associates: others take the opposite extreme, and, with intellectual arrogance, seek to build up their opinions and beliefs from the very foundation, individually and alone, without help from others. Intermediate between these two extremes comes the man with full respect for the opinions of those around him, and yet with such discrimination that he sees a chance of error in all, and most of all in himself. He has a longing for the truth, and is willing to test himself, to test others, and to test nature until he finds it. He has the courage of his opinions when thus carefully formed, and is then, but not till then, willing to stand before the world and proclaim what he considers the truth. Like Galileo and Copernicus he inaugurates a new era in science, or, like Luther, in the religious belief of mankind. He neither shrinks within himself at the thought of having an opinion of his own, nor yet believes it to be the only one worth considering in the world; he is neither crushed with intellectual humility, nor yet exalted with intellectual pride; he sees that the problems of nature and society can be solved, and yet he knows that this can only come about by the combined intellect of the world acting through ages of time, and that he, though his intellect were that of Newton, can, at best, do very little toward it. Knowing this he seeks all the aids in his power to ascertain the truth, and if he, through either ambition or love of truth, wishes to impress his opinions on the world, he first takes care to have them correct. Above all, he is willing to abstain from having opinions on subjects of which he knows nothing.

"It is the province of modern education to form such a mind, while at the same time giving to it enough knowledge to have a broad outlook over the world of science, art, and letters. Time will not permit me to discuss the subject of education in general, and, indeed, I would be transgressing the principles above laid down if I should attempt it. I shall only call attention at this present time to the place of the laboratory in modern education. I have often had a great desire to know the state of mind of the more eminent of mankind before modern science changed the world to its present condition and exercised its influence on all departments of knowledge and speculation. But I have failed to picture to myself clearly such a mind, while, at the same time, the study of human nature, as it exists at present, shows me much that I suppose to be in common with it. As far as I can see, the unscientific mind differs from the scientific in this, that it is willing to accept and make statements of which it has no clear conception to begin with, and of whose truth it is not assured. It is an irresponsible state of mind without clearness of conception, where the connection between the thought and its object is of the vaguest description. It is the state of mind where opinions are given and accepted without ever being subjected to rigid tests, and it may have some connection with that state of mind where everything has a personal aspect and we are guided by feelings rather than reason.

"When, by education, we attempt to correct these faults, it is necessary that we have some standard of absolute truth: that we bring the mind in direct contact with it, and let it be convinced of its errors again and again. We may state, like the philosophers who lived before Galileo, that large bodies fall faster than small ones, but when we see them strike the ground together, we know that our previous opinion was false, and we learn that even the intellect of an Aristotle may be mistaken. Thus we are taught care in the formation of our

opinions, and find that the unguided human mind goes astray almost without fail. We must correct it constantly and convince it of error over and over again until it discovers the proper method of reasoning, which will surely accord with the truth in whatever conclusions it may reach. There is, however, danger in this process that the mind may become over-cautious, and thus present a weakness when brought in contact with an unscrupulous person who cares little for truth and a great deal for effect. But if we believe in the maxim that truth will prevail, and consider it the duty of all educated men to aid its progress, the kind of mind which I describe is the proper one to foster by education. Let the student be brought face to face with nature: let him exercise his reason with respect to the simplest physical phenomenon, and then, in the laboratory, put his opinions to the test; the result is invariably humility, for he finds that nature has laws which must be discovered by labour and toil, and not by wild flights of the imagination and scintillations of so-called genius.

"Those who have studied the present state of education in the schools and colleges tell us that most subjects, including the sciences, are taught as an exercise to the memory. I myself have witnessed the melancholy sight in a fashionable school for young ladies of those who were born to be intellectual beings reciting page after page from memory, without any effort being made to discover whether they understood the subject or not. There are even many schools, so-called, where the subject of physics or natural philosophy itself is taught, without even a class experiment to illustrate the subject and connect the words with ideas. Words, mere words, are taught, and a state of mind far different from that above described is produced. If one were required to find a system of education which would the most surely and certainly disgust the student with any subject, I can conceive of none which would do this more quickly than this method, where he is forced to learn what he does not understand. It is said of the great Faraday that he never could understand any scientific experiment thoroughly until he had not only seen it performed by others, but had performed it himself. Shall we then expect children and youth to do what Faraday could not do? A thousand times better never teach the subject at all.

"Tastes differ, but we may safely say that every subject of study which is thoroughly understood is a pleasure to the student. The healthy mind as well as the healthy body craves exercise, and the school-room or the lecture room should be a source of positive enjoyment to those who enter it. Above all, the study of nature, from the magnificent universe, across which light itself, at the rate of 186,000 miles per second, cannot go in less than hundreds of years, down to the atom of which millions are required to build up the smallest microscopic object, should be the most interesting subject brought to the notice of the student.

"Some are born blind to the beauties of the world around them, some have their tastes better developed in other directions, and some have minds incapable of ever understanding the simplest natural phenomenon; but there is also a large class of students who have at least ordinary powers and ordinary tastes for scientific pursuits: to train the powers of observation and classification let them study natural history, not only from books, but from prepared specimens, or directly from nature; to give care in experiment and convince them that nature forgives no error, let them enter the chemical laboratory; to train them in exact and logical powers of reasoning, let them study mathematics; but to combine all this training in one and exhibit to their minds the most perfect and systematic method of discovering the exact laws of nature, let them study physics and astronomy, where observation, common-sense, and mathematics go hand in hand. The object of education is not only to produce a man who *knows*, but one who *does*; who makes his mark in the struggle of life and succeeds well in whatever he undertakes; who can solve the problems of nature and of humanity as they arise; and who, when he knows he is right, can boldly convince the world of the fact. Men of action are needed as well as men of thought.

"There is no doubt in my mind that this is the point in which much of our modern education fails. Why is it? I answer that the memory alone is trained, and the reason and judgment are used merely to refer matters to some authority who is considered final, and worse than all they are not trained to apply their knowledge constantly. To produce men of action they must be trained in action. If the languages be studied, they must be made to translate from one language to the other until they have perfect facility in the process. If mathematics be studied, they must work problems, more problems, and problems again, until they have the use of what they know. If they study the sciences, they must enter the laboratory, and stand face to face with nature; they must learn to test their knowledge constantly, and thus see for themselves the sad results of vague speculation; they must learn by direct experiment that there is such a thing in the world as truth, and that their own mind is most liable to error. They must try experiment after experiment and work problem after problem until they become men of action, and not of theory.

"This, then, is the use of the laboratory in general education—to train the mind in right modes of thought by constantly bringing it in contact with absolute truth, and to give it a pleasant and profitable method of exercise which will call all its powers of reason and imagination into play. Its use in the special training of scientific men needs no remark, for it is well known that it is absolutely essential. The only question is whether the education of specialists in science is worth undertaking at all, and of these I have only to consider natural philosophers or physicists. I might point to the world around me, to the steam-engine, to labour-saving machinery, to the telegraph, to all those inventions which make the present age the 'Age of Electricity,' and let that be my answer. Nobody could gainsay that the answer would be complete, for all are benefited by these applications of science, and he would be considered absurd who did not recognise their value. These follow in the train of physics, but they are not physics; the cultivation of physics brings them and always will bring them, for the selfishness of mankind can always be relied upon to turn all things to profit. But in the education pertaining to a University we look for other results. The special physicist trained there must be taught to cultivate his science for its own sake. He must go forth into the world with enthusiasm for it, and try to draw others into an appreciation of it, doing his part to convince the world that the study of nature is one of the most noble of pursuits, that there are other things worthy of the attention of mankind besides the pursuit of wealth. He must push forward and do what he can according to his ability, to further the progress of his science.

"Thus does the University, from its physical laboratory, send forth into the world the trained physicist to advance his science and to carry to other colleges and technical schools his enthusiasm and knowledge. Thus the whole country is educated in the subject, and others are taught to devote their lives to its pursuit, while some make the applications to the ordinary pursuits of life that are appreciated by all.

"But for myself I value in a scientific mind most of all that love of truth, that care in its pursuit, and that humility of mind which makes the possibility of error always present more than any other quality. This is the mind which has built up modern science to its present perfection, which has laid one stone upon the other with such care that it to-day offers to the world the most complete monument to human reason. This is the mind which is destined to govern the world in the future and to solve problems pertaining to politics and humanity as well as to inanimate nature.

"It is the only mind which appreciates the imperfections of the human reason, and is thus careful to guard against them. It is the only mind that values the truth as it should be valued, and ignores all personal feeling in its pursuit. And this is the mind the physical laboratory is built to cultivate."

Dr. Thomas's address was as follows:—

"The foundation and growth of a University is an event of the greatest interest.

"Its functions and use have been elaborately discussed by many modern thinkers and scholars. I shall call your attention to three statements of men of differing schools of thought.

"Goldwin Smith, discussing Oxford University organisation, says:—

"'Experience seems to show that the best way in which the University can promote learning and advance science is—

"'(1) By allowing its teachers, and especially the holders of its great professorial chairs, a liberal margin for private study;

"'(2) By keeping its libraries and scientific apparatus in full efficiency and opening them as liberally as possible;

"'(3) By assisting, through its press, in the publication of learned works which an ordinary publisher would not undertake;

"'(4) By making the best use of its power of conferring literary and scientific honours.'

"Matthew Arnold says, the University 'ought to provide facilities, after the general education is finished, for the young man to go on in the line where his special aptitudes lead him, be it that of languages and literature, of mathematics, of the natural sciences, of the application of these sciences, or any other line, and follow the studies of this line systematically, under first-rate teaching.'

"Again, 'The idea of a University is, as I have already said, that of an institution not only offering to young men facilities for graduating in that line of study to which their aptitudes direct them, but offering to them also facilities for following that line of study systematically under first-rate instruction. This second function is of incalculable importance, of far greater importance even than the first. It is impossible to over-value the importance to a young man of being brought in contact with a first-rate teacher of his matter of study, and of getting from him a clear notion of what the systematic study of it means."

"John Henry Newman says:—'It is a great point, then, to enlarge the range of studies which a University professes, even for the sake of the students; and though they cannot pursue every study which is open to them, they will be gainers by living among those and under those who represent the whole circle. This I conceive to be the advantage of a seat of universal learning, considered as a place of education. An assemblage of learned men, zealous for their own sciences and rivals of each other, are brought by familiar intercourse and for the sake of intellectual peace to adjust together the claims and relations of their respective subjects of investigation. They learn to respect, to consult, to aid each other. Thus is created a pure and clear atmosphere of thought, which the student also breathes, though in his own case he only pursues a few sciences out of the multitude. He apprehends the great outlines of knowledge, the principles on which it rests, the scale of its parts, its lights and its shades, its great and its little, as he otherwise cannot apprehend them.

"'Hence it is that his education is called "Liberal." A habit of mind is formed which lasts through life, of which the attributes are freedom, equitableness, calmness, moderation, and wisdom. This, then, I would assign as the special fruit of the education furnished at a University. . . . This is the main purpose of a University in the treatment of its students.'

"And a great thinker of another generation, George Fox, advised the setting up of schools for instructing 'in whatsoever things were civil and useful in the creation.'

"We may then conclude that a University, wisely planned and faithfully administered, should be able to gather together a company of teachers, distinguished in character and learning; to present courses of study, important and thorough; and to attract scholars mature in age and competent by reason of previous training to pursue special lines of study, in order to fit themselves in a worthy manner for their chosen vocation. It should be wide in its scope and able to supplement the College, and aid students to perfect themselves in many departments of learning. It should provide liberally all the apparatus for this study. It should be rich in laboratories, in books, in instruments. It should endow research and stimulate investigation and discovery. It should be prepared to give results of work done within its halls speedy and wide publicity amongst scholars engaged in kindred pursuits. It should give its contribution to society by training men who are fitted to help in the solution of the problems of the age—scientific, social, political, moral, and religious, both by stimulating the production of books and by contributions to the journals and literature of the day. It should encourage all noble aspirations, conserve all good inheritances of the past, and create an atmosphere of enthusiasm for hard work. It should be able to bestow honours worthy of the name in reward for faithful devotion and for the successful fulfilment of its courses of study. Its work should be known and recognised where learning is known and recognised, and its name should carry weight in other Universities and centres of research in the world of letters.

"Such thoughts as these, I am sure, Judge Dobbin, were in your mind and in the minds of the other trustees to whom was intrusted by our late townsman, Johns Hopkins, the foundation and guidance of this University which was to bear his name. On the completion of the first decade of its existence, in the presence of the trustees and the President and Faculty of the University, before the graduates, the present Fellows, and students of the University, and in the presence of this company of our friends and fellow-citizens, it has seemed fitting to allude to these sentiments as we proceed to consider the progress of this University.

"I am glad to take this opportunity of replying in public to questions such as were asked me by a young Baltimorean, who the other day said: 'Why do Baltimoreans have to go to New Haven or somewhere abroad to learn about the Johns Hopkins? Why do you not tell us what is being done?' In order to do this before a Baltimore audience, I have supposed my friend to have asked the following questions, to which I shall briefly reply:—

"(1) Have great teachers been attracted to the University?

"(2) Have important courses of study been instituted?

"(3) Have students come, and from whence?

"(4) Have patient and successful researches been carried on?

"(5) Has the University gathered together suitable apparatus, &c., for study?

"(6) Have the results of these researches been given by the University to the world?

"(7) Has the work done here been recognised elsewhere?

"(8) Has the training given proved valuable to those who have received it?

"(9) What has the University done for this community?

"The Board of Trustees was incorporated in August 1869, at the instance of Johns Hopkins and during his lifetime. About a year after the death of the founder, in December 1873, the Board was put in possession of the endowment provided by his beneficence, and organised for work. The President of the Board, the late Galloway Cheston, took an active part in the enterprise, and by his advice greatly aided in laying the foundation of the University, and his name will always be honourably associated with its history. The other members of the Board, all of whom had been named by Johns Hopkins, were Reverdy Johnson, Jun., the first Chairman of the Executive Committee, Francis T. King, Lewis N. Hopkins, Thomas M. Smith, William Hopkins, John W. Garrett, Francis White, Charles J. M. Gwinn, George W. Dobbin, George William Brown, and James Carey Thomas.

"*What Great Teachers have been attracted?*—It was soon apparent that the wise and untrammelled directions of Johns Hopkins to his trustees to found a University would attract the attention of those interested in the cause of education, especially in the United States.

"The opportunity of developing an institution suited to the needs of the country was sufficient to draw to Baltimore from across the Continent the then President of the University of California, Daniel C. Gilman, who was named to the trustees as the man best fitted by previous training and devotion to the study of educational methods, to advise and direct the establishment of the new foundation, by his former colleagues of Yale College—by President Eliot, of Harvard University, at once the most renowned and the most venerable institution of learning in the country—by President White, of Cornell University, then in the early days of its growing importance and usefulness—by President Angell, of the University of Michigan, the crowning institution of learning of the well-organised system of public instruction in that great and strong Western State—and by numerous other leading educators. At the request of the trustees Mr. Gilmun came to Baltimore, and after consultation with them accepted the Presidency of the Johns Hopkins University. Under his thoughtful care and constant and laborious effort the plan originally contemplated has been gradually and harmoniously developed.

"Besides President Gilman, the University also drew from across the ocean, from Woolwich, England, Prof. Sylvester, one of the two greatest English mathematicians, and indeed one of the greatest of the world; and from Virginia, in our own land, Prof. Gildersleeve, second to none in his attainments in and devotion to Greek and other classical study—besides younger men whose subsequent career has justified the bright promise of their early years. I shall not mention further by name the present distinguished staff of Professors and teachers, whose work I have alluded to, and who form the permanent renown and attraction of the University.

"I will give a list in chronological order of those gentlemen, not now connected with the University, who, for a longer or shorter period, have lectured here during the past ten years:—

"In Language and Literature, Profs. F. J. Child, James Russell Lowell, W. D. Whitney, C. R. Lanman, Thomas C.

Murray, H. C. G. Brandt, Sidney Lanier (too early lost), Profs. W. W. Goodwin, J. A. Harrison, J. Rendel Harris, Hiram Corson, A. S. Cook, Messrs. George W. Cable, Edmund Gosse, Justin Winsor, A. Melville Bell, Drs. Isaac H. Hall, and W. Hayes Ward; in History and Political Science, Profs. T. M. Cooley, F. A. Walker, W. F. Allen, the lamented J. L. Diman, H. von Holst, Austin Scott, James Bryce, E. A. Freeman (who gave six lectures and imparted a decided impulse to historical study here), R. M. Venable, Messrs. J. J. Knox and Eugene Schuyler; in Archæology and Art, Messrs. W. W. Story, F. Seymour Haden, J. Thacher Clarke, W. J. Stillman. Dr. Charles Waldstein, and Mr. Frederick Wedmore; in Philosophy and Logic, Profs. William James, G. S. Morris, Mr. C. S. Pierce, and Dr. Josiah Royce; in Physical and Mathematical Science, Profs. J. E. Hilgard, J. Willard Gibbs, John Trowbridge, A. Graham Bell, S. P. Langley, Arthur Cayley, C. S. Hastings, and Sir William Thomson; in Chemistry and Biology, Profs. J. W. Mallet, W. G. Farlow, J. McCrady, W. T. Sedgwick, H. Sewall, and W. Trelease.

"At our commencements, anniversaries, and other gatherings, we have heard from Presidents Eliot and White, from Dean Stanley, Dean Howson, Prof. Huxley, Archdeacon Farrar, Chief Justice Waite, Hon. W. M. Evarts, Dr. W. A. P. Martin, Dr. W. B. Carpenter, Hon. S. T. Wallis, J. B. Braithwaite, and others.

"Many of these have been listened to by those not members of the University who were specially interested in their subjects, and it may be fairly said that many eminent and great teachers have been both for long and short periods attracted to the University.

"*What University Courses are here offered, and what Graduate Students have been attracted?*—The courses of University studies that have been pursued have been so often and so fully referred to in the reports and circulars of the University, that I can only enumerate those in higher mathematics, in physics, in chemistry, in mineralogy and petrography, in biology; in Greek, in Latin, in Sanskrit, in Hebrew, in Aramæan, in Arabic, in Assyrian, and in Sumero-Akkadian; in English, in German, in the Romance group of languages, including French, old and modern, Wallachian, Italian, Spanish, Catalan, old Provençal, modern Provençal, and Portuguese; in history, ancient and modern, in political economy, physical and historical geography; in psychology, pedagogics and philosophy, in mental hygiene and ethics. In these studies advanced instruction has been given by all available means, such as lecture, laboratory practice, seminary work, books, models, and plates, in order to fit those who are preparing for teaching or special research.

"That these courses have succeeded in attracting students of mature age is evident from the fact that out of the total number of students (923) enrolled during the decade, 590 have pursued graduate courses, and these 590 came from more than 100 different Universities and Colleges as widely separated as Russia and Japan.

"*What Apparatus and Appliances have been gathered together?*—To aid in the instruction given, the trustees have from the first had in view securing the most convenient and free access to the most modern means of promoting research. They were greatly aided by the existence in Baltimore of a library of unusual value to students—the gift of the late George Peabody, and brought together with much care and diligence by the trustees, the provost, and the librarian of the Peabody Institute—and which has been liberally opened to members of the University. As a supplement to the Peabody collections the University has placed within its own walls 29,000 volumes—a portion of which are standard reference-books needed by all the teachers and students; the remainder are special and often costly books which have been called for by the specialists here engaged in work.

"The plans of the University being at first, from the nature of the case, tentative, the work was begun in two dwelling-houses purchased in 1875, on Howard Street, near Monument Street, and in a hall erected at the time, and named after the founder, which contained an assembly room and accommodations for the library and for the biological laboratory, and in a chemical laboratory built at the same time, and this was for some time the modest seat of the University. The location was found more convenient than had been foreseen, both for students who lived in the city and for those that came from elsewhere, who readily found accommodation in lodgings suited to their taste and means. Easy access was had to the Peabody and other collections of books, as those of the Historical Society, and later of the Pratt Library, and there have gradually grown around the present site complete and well-equipped laboratories. The chemical laboratory has been greatly enlarged and perfected. The biological laboratory adjoining has been erected after plans suggested by years of work and by comparison with foreign institutions of a similar kind, and there is now building near by the physical laboratory, of which Prof. Rowland has been speaking to you to-day. Laboratory work in pathology has been begun in one of the buildings of the Johns Hopkins Hospital, and it is intended to erect the Medical School on a lot now owned by the University, adjacent to the hospital.

"Into these various buildings have been gathered, at the suggestion and under the careful personal supervision of various experts, about 70,000 dols. worth of apparatus of the most approved modern make, thus placing within the reach of investigators the means of pursuing advanced research, as well as enabling students to become familiar by personal use with the newest methods of study and experiment.

"*What Research is carried on, and what has been published?*—The researches which have been made have been many and varied. I cannot refer to the more technical, such as those in mathematics and inorganic chemistry, &c., but briefly to the more easily stated.

"Our knowledge of the nature of the sun, as perceived through the solar spectrum, has received accessions from the beautiful image thrown from the gratings first made here by the agency of a wonderful dividing-engine, the invention of the Professor of Physics. From this image a map of the spectrum has been published, very much more minute than any before made.

"Researches in electricity and magnetism have been made under the auspices of the United States Government, with the co-operation of other nations; the mechanical equivalent of heat has been redetermined; investigations have been conducted in physiology, especially of the heart's action; lower animal life has been studied, especially that of the oyster in connection with the State of Maryland; both here and in Boston the cause of water pollution in great reservoirs has been discovered; the curious geological formation of our own neighbourhood has been brought to notice and has attracted wide attention. The philologians and grammarians have been engaged in the investigation of Greek and Latin syntax; in editing ancient writings, such as Pindar, the newly discovered Greek MSS. of the Teaching of the Twelve Apostles, and part of an old Syriac MS. of the New Testament. Baltimore is now one of the centres for the interpretation of Sanskrit texts and of Assyrian inscriptions. A great contribution has been made to the study of American Institutions, and new methods of historical research and of publication have been initiated.

"It is with satisfaction that I state that these researches have been widely recognised at home and abroad, not as *promises* for the future, but as successful experiments recorded, and conclusions reached which have passed into the history of science. By means of them the fame of the University has been carried into every seat of learning in the world, from Oxford and Cambridge in England, to Tokio, Japan; from the northern and more modern Universities of Sweden and Russia to the ancient seats of learning in Italy and Southern Europe. The exchanges on the shelves of our library, received with almost every foreign mail in return for the six scientific journals published by the University,[1] attest both its importance and its estimation outside its own walls. Besides this, personal and unsolicited testimonials from eminent men are on file in the office which have been received from many quarters.

"These researches, delicate, prolonged, and important, and others not now mentioned, have been made by Professors, Fellows, and advanced students. Indeed the whole plan of Fellowships has in reality been a most practical and efficient endowment of research, and has richly repaid the University and the community in the importance and value of the results obtained.

"Twenty young men who have not quite completed their

[1] (1) The "American Journal of Mathematics," commenced in 1878, now in its eighth volume; (2) the "American Chemical Journal," commenced in 1879, now in its eighth volume; (3) the "American Journal of Philology," commenced in 1880, now in its seventh volume; (4) "Studies from the Biological Laboratory," commenced in 1879, now in its third volume; (5) "Studies in Historical and Political Science," begun in 1882, of which the fourth series is in progress; (6) the "Johns Hopkins University Circulars," begun in 1879, of which forty-nine numbers have been issued.

work as students following masters, but who have gone far enough to indicate that they are possessed of unusual ability, are annually chosen by the Academic Council, and are encouraged by a generous stipend to devote all their time to study which is not of a distinctively professional character. They are chosen because of the hope they give of future achievements, or are selected on the evidence they submit of their previous intellectual attainments. The system here adopted has elsewhere been followed.

"*What has been the Value of this Training?*—Has the training here been of value to the men that have submitted to the severe ordeal of discipline and who have often surrendered honourable and lucrative positions to avail themselves of the advantages offered for research and study? Or, in other words, are the diplomas to be given to-day as testimonials of the University to the attainments of those to whom they are so worthily awarded, of real value to their possessors?

"Of the 69 persons who in these ten years have been admitted to the degree of Doctor of Philosophy, denoting proficiency in various lines of special graduate study, either in letters or in science, 56 have obtained honourable positions as professors and teachers in 32 Universities and Colleges; and of the 90 to whom the degree of Bachelor of Arts has been given, 20 have engaged in teaching in 16 Colleges and high schools.

"I will conclude this part of my subject by quoting the reply made by a graduate student from North Carolina, when asked what he had found here of most use, he replied: 'The freedom of access to able teachers and the stimulant of studying in company with men of maturer minds than one meets elsewhere.'

"*But what has the University done for this community?*—Besides the incidental advantages which must accrue to any community from the presence of a great seat of learning, the trustees have had in mind from the first the special needs of this city and state. At the conclusion of the late war fewer boys were at college than at former periods. Many young men here and further south had foregone college training, and circumstances forbade the sending of others who were growing up. It was manifest that the need of our own people was first a college in order to train for life, or for further university instruction. So side by side with the University has developed the college department of the Johns Hopkins University. This was begun when the discussion of a fixed, a free, or wholly or partly elective college course had not been so warmly debated as at the present time, but it was evident that the wide range which the development of various branches of knowledge has taken since the old arrangement of college studies was effected, and the limited time which can ordinarily be devoted by students to preparation for their life-work, made a readjustment of the college course desirable. This was accomplished here by arranging, after a fixed matriculation, the studies in groups rather than years, and demanding in each group a certain required amount of training in other than the main study of the course. Thus classical students are required to study some science, scientific students some classics, and all to receive a fixed amount of general English training in literature, ethics, philosophy, and modern languages.

"The seven groups for which, in accordance with these principles, arrangements are now made, are these:—

"(1) Classical—corresponding closely with what has been hitherto known in this country as the usual college course.

"(2) Mathematical-Physical—which meets the wants of those who are expecting to enter upon the modern vocations in which rigid mathematical discipline is indispensable.

"(3) Chemical-Biological—which is adapted to those, among others, who expect to enter upon the subsequent study of medicine.

"(4) Physical-Chemical—which is most likely to be followed by students preparing for those scientific pursuits which are neither chiefly mathematical nor chiefly biological.

"(5) Latin-Mathematical—which affords a good fundamental training, without prolonged attention to the study of Greek.

"(6) Historical-Political—which furnishes a basis for the subsequent study of law.

"(7) Modern Language—where French, German, English, and in exceptional cases, other modern languages, take the place of Latin and Greek in the traditional classical course.

"It cannot be said that this arrangement is perfect, but it has worked well, and great effort is made to have it at once liberal and adapted to the exigencies of active life. I should like all the time at my disposal to expand more fully this slight sketch of the college course which lies near my own heart, but must content myself with stating that it has from the first attracted our own boys, to whom great inducement has been held out, and who have proved some of our most enthusiastic and successful students, have won for themselves many of our own Fellowships, and have gone out to positions of importance and emolument. Their number is rapidly increasing, and the University is constantly endeavouring to make closer the connection between the high schools, whether private or public, and the collegiate department of the Johns Hopkins University.

"Various free scholarships are annually offered to students coming from Maryland, Virginia, and North Carolina, and have been held by 150 students from these States. The existence in our midst of such advantages is stimulating our young men to avail themselve of them, and is increasing the number and efficiency of preliminary schools. We have now in the collegiate department 130 students.

"I have thus, in the briefest and most prosaic manner, endeavoured to summarise the work of ten years into the space of twice as many minutes. It has been impossible, although I have not even glanced at the various literary and scientific Societies formed for themselves by the members of the University, nor alluded to the common college life, nor spoken of the work of the Christian Association of the University, which has served an excellent purpose; but yet I think that I have shown that something has been done to bring together great teachers, to start liberal courses of study, to attract students, to collect libraries and apparatus, to stimulate research, to publish results, and have stated in what manner this work has been recognised, and how the needs of this community have been considered.

"But I am sure that in reaching these conclusions you must feel how little has been done in comparison with what is practicable with longer time and greater resources. The perpetuation and enlargement of the University on a broad and liberal foundation should be the pride of every citizen. It is a great trust to be handed down to those who shall succeed us. Let us be careful to see that no detriment happen to it.

"Amidst the jarring of contending factions and classes there needs must be thoughtful men trained to habits of patient investigation and quiet study—amidst the rush of business and competition, men who in secluded laboratories pass hours and days in subtile experiments—amidst the selfishness of politicians and placemen, historians and philosophers and teachers who can recall the lessons of past ages and vindicate the great moral principles which underlie all true progress.

"For these and other great purposes Universities should exist and be richly endowed. They should be few, but strong.

"A president of a growing Western College, last week in Baltimore, emphasised most strongly the importance of adding efficiency to existing Universities in order to make them great centres for training and research. The possessors of great wealth, most frequently in this country accumulated in the course of a single life, have often felt their responsibility in its ultimate destination. They have in many instances, amongst which the course of Johns Hopkins is conspicuous, returned their accumulated gains to the community in noble gifts, founding great institutions of learning and great charities for the training of the future citizen and for the alleviation of human suffering. These should be fostered and enlarged, as has been done at Harvard and at Cornell, in order that the greatest good may be accomplished.

"The training of *men* is after all the most important end of all educational effort. It is to you, young men, the sons of this new foundation, that your teachers and friends look as the best evidence of the success of their endeavour. Your learning, your usefulness, your accomplishments, your high aims and noble character, your achievements, whether in the pulpit or the forum, the college or the laboratory, at home and abroad will afford a continual and living reminder of this, the place of your training.

"To a State founded on the beneficent precepts of Christianity, the walls of its defence must be not the physical strength of its citizens but their moral character. In vain will science harness the powers of the universe unless they are yoked to the chariot of peace and goodwill. In vain will learning and training give efficiency to individual influence and native genius, unless the purposes of the man are noble and far-reaching. The truth which sets free is the truth which warms the heart and expands the sympathies, as well as enlightens the intellect, which is of

Him who is the truth Himself. Let us have confidence in the supremacy of truth. Such has hitherto been the guiding lamp of the Johns Hopkins University. May it ever be the beacon of the future."

SCIENTIFIC SERIALS

A LARGE space in the June number of the *Journal of Botany* is occupied by a long biographical notice, by the editor, of the late Rev. W. W. Newbould, of whom an excellent portrait is also provided. The other articles are almost entirely of local interest.

THE *Journal of the Franklin Institute*, vol. cxxi. No. 724, April 1886.—Lieut. J. P. Finley, tornado study: a useful summary of the principal facts scientifically known respecting tornadoes.—F. E. Galloupe, rapid transit and elevated railroads. This concludes the discussion on this topic.—G. E. Waring, Jun., mechanical appliances in town sewerage; discusses the systems employed in several American cities.—Prof. R. H. Thurston, construction of a large Prony brake. Gives an account of a brake capable of absorbing 540 horse-power.—Dr. W. H. Wahl, summary of engineering and industrial progress for 1885.—Report of Committee on Delany system of multiplex telegraphy.

No. 725, May.—J. M. Hartman, the blast-furnace: a very concise summary of present methods of construction and theories.—Lieut. A. B. Wyckoff, hydrographic work of the U.S. Navy. J. Shinn, the cultivation of flax in the United States.—L. D'Auria, the law of cylinder condensation of steam-engines. The new formulæ indicate that the proper way to decrease cylinder condensation is to increase piston speed.—C. J. Kintner, history of the electrical art in the U.S. Patent Office. An interesting account, in which two early forms of storage battery are described. The author, however, appears to think that nothing can be called an invention unless it has been patented in the United States. There were secondary batteries prior to Kintner's, electric motors prior to Davenport's, and telephones prior to Graham Bell's.—Report of examiners of electrical exhibition on applications of electricity to art productions.

No. 726, June.—Chief Engineer Isherwood, an account of experiments made by Chief Engineers Zeller and Hunt to ascertain the economic effect of using in a non-condensing engine saturated steam alone, and of using it mixed with compressed hot air. No economic saving was effected by this process, as there was not sufficient time for the steam and hot air, which were delivered into the cylinder in separate masses, to become mixed, and the air failed to prevent condensation.—S. L. W., on the Oram system of marine propulsion. This system has twin screws placed forward at about 1/5 of the vessel's length from the bow, in recesses in the sides.—G. W. Chance, the South Street Bridge.—W. Lewis, experiments on the transmission of power by gearing. Valuable researches on the causes of loss of power in worm-gearing and spur-gearing.

No. 727, July.—C. Sellers, Jun., Oliver Evans and his inventions. A biographical notice of this remarkable man, whose prediction of the future of the steam-engine is well known.—O. E. Michaelis, the applications of electricity to marksmanship. This is the first part of the paper, and treats rather of mechanical methods of measuring speed of projectiles, such as Robins's ballistic method.—H. M. Dubois, tests of vehicle wheels.—F. E. Ives, colour-sensitive photographic plates. A compound sensibilisator of fresh blue myrtle chlorophyll with a little eosin is found to be the most sensitive to yellow and green.—Report of Committee on the Phelps induction telegraph. The Committee praise highly this invention for telegraphing to and from a moving train.—Report of Committee on the process and furnace for the reduction of refractory ores and the production of metals, alloys, and compounds, invented by E. H. and A. H. Cowles. Cowles's electric furnace, for reducing ore by means of the voltaic arc between carbon poles, is merely a development of the electric furnace devised by the late Sir W. Siemens. It appears to be eminently suitable for the production of aluminium compounds.—Prof. E. J. Houston, some additional facts concerning the Reis articulating telephone. Gives an account of some recent experiments transmitting speech with the identical apparatus manufactured by Reis and used by him in his lecture before the Physical Society at Frankfort in 1861.

Annalen der Physik und Chemie, vol. xxviii. No. 5, May.—R. Colley, on some new methods for observing electric oscillations, and some applications of them. This paper gives certain relations between the time of oscillation of discharges through a shunt having a great coefficient of self-induction and the capacity of the condenser. Using a standard coil the coefficient of self-induction of which could be determined by its geometrical form, and a normal guard-ring condenser made of three sheets of silvered glass, the capacity of which could equally be determined, the author made experiments from which he deduces a new value of the ratio v, which he gives as $3 \cdot 015 \times 10^{10}$ centims. per second.—Hans Jahn, on the relation of the chemical energy and the current energy of galvanic elements. A discussion of Helmholtz's expression for the secondary heat, together with some determinations made on Daniell's and De La Rue's cells.—E. Riecke, on the pyro-electricity of tourmaline. The first part of this paper summarises the previous researches of Gaugain and others upon the electricity of the tourmaline in relation to its section, length, rate of cooling, &c., and gives an account of some new and careful observations made upon three tourmalines. The second part of the paper is devoted to the development of a mathematical theory of the electricity of the tourmaline, based on the physical hypotheses that the molecules possess an initial electric polarisation, measurable in terms of the electric moment per unit of volume, and dependent upon the temperature, and that there is a surface-conductivity of a certain value. The formulæ appear to agree very well with the observed facts.—T. Ihmori, on the absorption of mercury vapour by spongy platinum. A quantity of platinum, deposited from chloride by formic acid, was found to increase in weight in presence of mercury. The author uses this increase of weight to investigate the figures given by Hertz and by Hagen, for the pressure of mercury vapour at different temperatures. His results are considerably lower than those of Hagen, and a little higher than those of Hertz.—C. Pulfrich, on the elastic reaction of a caoutchouc tube.—A. König, on a new method of determining the modulus of elasticity. Errors of observation are avoided by using two mirrors, the inclination of which altered by the loading of the bar under examination.—Karl Exner, on senseformulæ: lenticular action of non-homogeneous bodies. Discussion of lenticular action of cylindriform disks with parallel plane faces made of materials which, on being cast in moulds, cool non-homogeneously with refractive indices that increase or diminish from point to point below the surface. The formulæ deduced coincide with ordinary lens formulæ when the variation of refractive index is proportional to the square of the depth from the surface. This appears to be nearly the case in disks of cast glue.—W. Wien, researches on the absorption-phenomena occurring in the diffraction of light. This paper discusses diffraction in relation to the colours of metallic reflection. Incidentally it brings out an additional proof that the vibrations are perpendicular to the so-called plane of polarisation.—H. W. Vogel, on some colour-experiments, and on photography in natural colours. Two solutions, one of chrysanilin, the other of anilin blue, in alcohol, are respectively yellow and blue; but when superposed give red, not green. The violet of the spectrum appears to play a very unimportant part in colour-sensations; methyl-violet, and nearly all the so-called violet pigments and dyes owe their tint to mixtures of red and blue rays, not to rays of spectrum violet. For colour-printing at least six tints are found requisite, and in general sufficient. Hence the author thinks that all requirements of colour-photography would be met by six printings from six blocks produced by photography from plates of bromide emulsion, to which the following six substances have been used as "sensibilisators": *red*, naphthol blue; *orange*, cyanin; *yellow*, eosin; *green*, saffranin; *green-blue*, fluorescein; *blue*, chrysanilin or aurantin. The author further points out that as the actual tint of any colouring-matter is the complementary colour to that absorbed by the colouring-matter itself, it follows that each of the six plates ought to be printed off in the same dye-stuff that has served as photographic "sensibilisator."—P. Volkmann, note on Prof. Quincke's remarks on the determination of capillary constants of liquids.—R. Schultze, on a small improvement in Wiedemann's pyknometer.

No. 6, June.—A. Kundt and E. Blasius, remarks on some researches on the pyro-electricity of crystals. A convenient apparatus for heating crystals is described; also some observations on the amethyst. The effects of cracks in the crystals are also studied.—K. Mack, pyro-electric and optical observations

on Brazilian topaz. The author refers the phenomena to the presence of a single electric axis, inclined to each of the three principal axes of the crystal. He also points out that the two optic axes do not make equal angles with the greatest of the three principal axes.—W. Stscheglajeff, on the electro-magnetic rotation of the plane of polarisation in chloride of iron. Curves given show that in this substance Verdet's rule that the rotation is proportional to the intensity of the field is not observed.—H. Haga, experimental researches on the convection of heat by the electric current. Careful experiments described in this paper show that the Thomson effect in mercury is negative.—Fr. Stenger, on phenomena of fluorescence. Some doubt having been thrown upon the transmutation, by Magdala red and other bodies, of red rays into orange or yellow rays, the appearance of these higher rays being attributed to stray light, the whole matter has been repeated with the utmost precautions. It appears to be now established that eosin and fluorescein also have this property. All three bodies are exceptions to Stokes's rule that the transmutation in fluorescence is always a degradation in the spectrum scale. The author also discusses the matter in relation to Lommel's theory and to the influence of solvents.—E. Ketteler, a remarkable limiting case of crystalline reflection, and its investigation by the aid of the perfected total-reflectomer of Kohlrausch.—G. Hansemann, on a new method of determination of periods of oscillation of bar-magnets. A photographic camera and a mirror oscillating in conjunction with a seconds pendulum are applied to the ordinary apparatus for observing oscillations by reflection.—Werner Siemens, on the conservation of energy in the atmosphere of the earth. A discussion of atmospheric laws and of *vis viva* of the atmospheric masses, too involved for a brief abstract.—R. Gerhardt, on the tube-flute stop of the organ. An experimental and mathematical discussion of the effect of putting a small open tube into the closed top of the flute pipe.—W. Alexejew, on solutions. The author arrives at the following conclusions: solids dissolve better than liquids; at one given temperature a solid can give only one saturated solution, a liquid two; supersaturated solutions have two temperatures of decomposition, one at which decomposition may occur, another at which decomposition must occur. A large number of data of observation are plotted in curves.—E. Himstedt, reply to the observations of Lord Rayleigh upon my determination of the ohm.—L. Graetz, on the dependence of the elasticity of caoutchouc upon temperature, and its relation to coefficients of thermal expansion. From his observations the author predicts that a twisted rod of caoutchouc, when suddenly further twisted, will warm itself.—Fr. Stenger, simple demonstration of residual electric charges, by means of an exhausted tube used as a Leyden jar.—A. Oberbeck, remarks on my work on the resonance of electric oscillations. An acknowledgment of the priority of Dr. J. Hopkinson.—A. König, on an observation respecting the empirical basis of our perception of space. This basis is the apparent extent of the objects in the field of vision as distributed over the retina, and their relative apparent displacement when the eye is turned about.—Fr. Stenger, correction to the memoir on the properties of calc-spar in a homogeneous magnetic field.

Rivista Scientifico-Industriale, June 30.—On some new registering thermic instruments, by Prof. Filippo Artimini. A full description, with illustrations, is given of the author's registering thermo-pyrometer, in which the degrees of temperature are accurately recorded by an ingenious application of electricity.—Telephoning at great distances, by F. van Rysselberghe. The author gives a summary of his recent experiments in the United States, showing the possibility of telephoning at any distance and establishing a regular international telephonic service between all the great cities of the world. The telegraphic wires now in use may be utilised for the simultaneous transmission of telegraphic and telephonic messages.

SOCIETIES AND ACADEMIES

Sydney

Royal Society of New South Wales, June 2.—Chr. Rolleston, C.M.G., President, in the chair.—Papers read:—A new species of Ardisia from New Guinea, by Baron Ferd. von Mueller, K.C.M.G., F.R.S., descriptive of the only specimen as yet found so far north.—A comparison of the dialects of East and West Polynesia, Malay, Malagasy, and Australian, by the Rev. George Pratt, author of a dictionary of the Samoan language. This was a valuable contribution towards a polyglot of the languages of Polynesia, some of which have already passed away, and most of which are changing through the introduction of new words and the rapid intermingling of various races. The Royal Society of New South Wales was urged to take steps to preserve these records and customs not only of Polynesia, but of the fast-diminishing tribes of Australia.—The discovery of a poison in three species of Daphnandra, a genus of plants of the order Monimiaceæ, by T. L. Bancroft, M.B. (Edin.), F.L.S. (Brisbane).—On some new poisonous substances discovered on the Johnstone River, North Queensland, also by Dr. Bancroft. These papers were descriptive of experiments of the therapeutic action on the guinea-pig.—Prof. Liversidge, F.R.S., exhibited and described: (1) a meteorite, the third one known to be found in New South Wales, composed of iron principally, nickel, cobalt, sulphur, phosphorus, and carbon; (2) the matrix of the rock of the tin deposits of Tasmania, in which the cementing material is topaz; (3) a collection of New South Wales silver ores (38 specimens), collected by Mr. McGarvie Smith; (4) shale from the Hawkesbury sandstone showing worm tracks and perforations, the first time noticed in these rocks.—Mr. Lawrence Hargrave exhibited a model illustrating the undulatory motion of serpents, based upon the trichoidal plane, a continuation of similar studies by him on the motion of fishes and the flight of birds.

Microscopical Section.—The following papers were read during the year 1885:—The *Phylloxera vastatrix*, by Dr. Morris, illustrated with specimens of the diseased vine from the neighbourhood of Sydney; and by the same author, Notes on mounting diatoms in highly refractive media.—Specimens of *A. pellucida* were exhibited mounted in piperine, picric acid, chlorides of tin, and thallium, and sulphur in combination with disulphide of arsenic. These slides were exhaustively tested against the American methods, viz. Dr. Chase's metallic silver and realgar, also Prof. Smith's (Geneva, N.Y.) specimen slide. The microscopes and accessories were of the latest make, and the objectives used were homogeneous immersions by Tolles, 1/10, 1/25; Spencer, 1/12; Powell, 1/12, 1/20. The slides of Dr. Morris's sulphur and arsenic combination gave the best results. In addition to the above-mentioned objectives the following choice glasses were acquired by members of the section during the year:—Powell's, 1/6; Hom N.A., 1·5; Green (late Tolles), 1/2″ 30°; Bausch and Lomb's, 1/16 immersion; Wales, 1/12 immersion, 170°. A valuable Ross Zentmeyer binocular stand with apparatus was presented by H. G. Wright, M.R.C.S.E., whilst immersion and high-angled condensers were purchased. Amongst the numerous slides exhibited were some of new mosses found in the neighbourhood of Sydney by Mr. Whitelegge, and a bacillus found by Dr. Morris in the ulcerated intestine of a foal, the cause evidently of a widely spread epidemic prevalent throughout the colony amongst young foals only.

Paris

Academy of Sciences, August 2.—M. Jurien de la Gravière, President, in the chair.—On the relations of geodesy to geology, by M. H. Faye. In this second communication it is shown that the harmony of the two sciences results in the remarkable law determining the constancy of the mathematical figure of the globe throughout the whole series of geological evolutions, a law which sooner or later will enable us to form a clear idea regarding the thickness of the present crust of the earth. At the same time the problem cannot be completely solved by the resources of geometry alone, and recourse must be had to the other sciences bearing on the subject.—On the displacement of ammonia by other bases, and on its quantitative analysis in the soil, by MM. Berthelot and André. From their further researches the authors conclude that magnesia cannot be safely employed for the quantitative analysis of ammonia in the analysis of the earths and other organic products containing insoluble double ammoniacal salts. Certain derivatives from the aldehydes are in the same position, as are also probably the ammoniacal salts formed by the humic and allied acids.—On the quantitative analysis of ammonia (continued), by M. Th. Schlœsing. Having previously shown (July 26) that by distillation on magnesia all the ammonia may be extracted from the solution of its hydrochlorate, or from that of the ammoniaco-magnesian phosphate, the author completes his demonstration by causing the magnesia to act on the double ammoniacal salts, and especially on the chlorides containing magnesium and zinc.—Observations on the oldest sedimentary groups in the north-west of France (continued), by M. Hébert. A careful study of the whole region

leads to the conclusion that North Brittany, north of a line drawn from Quimper to Rennes, and West Normandy, north of a line drawn from Pontorson to Domfront and Falaise, are mainly constituted by the vertical clay-slates of Saint-Lô, overlain by the purple conglomerates, schists, and nearly horizontal red sandstones. Thus is confirmed the general conclusion announced by Dufrénoy in 1835.—On the presence of microscopic mineral crystals of the feldspar group in certain Jurassic limestones of the Alps, by M. Ch. Lory. The crystals here described yield on analysis about 47 per cent. of potassic and sodic feldspar mixed with a little albite; 45 of quartz in bipyramidal crystals and pulverised; 8 of argile, analogous in its composition to that of the carbonates of lime of the same horizon.—On the operations prosecuted in Tunisia by Commander Landas since the death of Col. Roudaire, by M. de Lesseps. The creation of an inland sea, the original object of these works, has been definitively abandoned, and attention is now devoted to the Wed Melah basin, which, by the sinking of Artesian wells, promises soon to recover its former productiveness.—Note on M. Marcel Deprez's experiments relating to the transmission of force between Creil and Paris, by M. Maurice Lévy. In this note is embodied the report of the sub-committee appointed to verify the results already obtained by M. Deprez during the course of the experiments carried out by him at Creil since November 1885. The main object of these experiments was to show the possibility of transmitting electrically to the Paris terminus, a distance of 56 kilometres, a force of 200 horse-power generated at Creil on the Great Northern line, with an effective yield of 50 per cent. The preliminary operations, concluded on May 24, show that the force consumed at Creil varied from 67 to 116 horse-power, that received at Paris from 27 to 52 horse-power, the yield being from about 41 to 45 per cent., and increasing with the transmitted force. The experiments, conducted at the expense of MM. Rothschild, show conclusively that with a single generator and a single receiver force may be profitably transmitted to a distance of over thirty miles with a loss of not more than 55 per cent. on 52 horse-power, without exceeding a current of 10 amperes, an angular velocity of 216 revolutions per minute, or a peripheric velocity of 7·50 m. per second. With improved appliances the loss, mainly due to absorption by the machines themselves, will probably be reduced to 50 per cent., and to still less in operations conducted on a larger scale.—Measurement of the intensity of sound by means of the manometric flames, by M. E. Doumer. It is shown that this apparatus, hitherto used mainly as a method of demonstration and summary study of the *timbre* of vocal sounds, is susceptible of far more varied applications, and especially may rival the graphic method in determining the height or intensity of sound.—On the separation of arsenic, antimony, and tin, by M. Ad. Carnot. By employing oxalic acid and the hyposulphite of soda or ammonia, sulphurous acid and sulphuretted hydrogen, the author has succeeded in effecting these separations rapidly and accurately, as he had already effected the separation of copper, cadmium, zinc, and nickel. His new methods enable him greatly to simplify the analysis of the complex alloys, of which these metals are constituents.—Heat of formation of the crystallised seleniures and of the amorphous seleniures, by M. Charles Fabre. Here are treated the seleniures of iron, manganese, cobalt, nickel, zinc, cadmium, copper, thallium, lead, mercury, and silver. In general the heat of formation of the seleniures prepared at high temperatures is shown to be equal or slightly inferior to that of the corresponding precipitated sulphides.—On the combinations of chloral and of resorcine, by M. H. Causse.—On the composition of the element in the grease of sheep's wool which is soluble in water, by M. E. Maumené.—On the indirect innervation of the skin, by M. C. Vanlair.—Note on the arterial system of the scorpion, by M. F. Houssay.—Fresh researches on the production of monstrosities in the hen's egg by a modification of the germ before incubation, by M. Dareste.—Observations on the pollinisation of orchids indigenous in France, by M. Paul Maury.—A first survey of the vegetation in the French territory of the Congo, by M. Ed. Bureau. The botanical collections formed by the Mission of West Africa, which have already reached Paris, comprise two herbariums, one collected by MM. Thollon and Schwéhisch, the other by MM. J. de Brazza and Pecile. There are altogether 599 species, chiefly from the districts of Franceville, Brazzaville, Ossika, Diélé, Lékéti, and Nganshu, on the Ogoway, Alima, and Lower Congo.

BERLIN

Chemical Society, May 24.—Dr. W. Will reported on the utilisation of myristic acid for lauric acid. According to the investigations of C. Reimer and W. Will there was, in the nuts of *Myristica surinamensis*, an excellent material for obtaining large quantities of myristic acid. Herr Lutz, student, had obtained from it myristinamid, and in accordance with the method of Hofmann, had transferred that into myristintridecyl-urea, tridecylamin, tridecyl nitrite, tridecylamid, and the corresponding combinations of the twelfth series, inclusive of lauric acid.—Herr O. N. Witt reported on experiments for the local determination of the sulfo group in the naphthalic acids, which led him to the same results as those obtained by Clève.—Prof. C. Liebermann referred to a work undertaken but not yet completed, with a view to the elucidation of the constitution of opianic acid. He showed that there were reasons to support the assumption of an aldehyde group in this compound, as also for the opposite assumption, a fact which led to a discussion on the so-called atom migrations, in which Herren Hofmann, Klason, Liebermann, Krämer, Pinner, and Will took part. Prof. Hofmann then called attention to the fact that such molecular shiftings of place had been particularly in quite recent times observed by him whilst studying the ether of cyanuric acid. He described the formation of a 2/3 iso- and 1/3 ortho-ethyl ether of cyanuric acid which he had obtained from cyanurate of silver by means of ethyliodide. This ether transformed itself, even at the ordinary temperature, into isotriethyl-cyanurate.—In conclusion Prof. Tiemann communicated a work by N. Nagai, on an aromatic ketone isolated from the root of a Japanese peony, having the constitution—

$$C_6H_3(OCH_3)(OH)(COCH_3).$$

BOOKS AND PAMPHLETS RECEIVED

"Madagaskar," by Prof. R. Hartman (Freytag, Leipzig).—"Elements of the Comparative Anatomy of Vertebrates," by R. Wiedersheim; translated by W. N. Parker (Macmillan).—"Bulletin de la Société Impériale des Naturalistes de Moscou," No. 1, 1886 (Moscou).—"Publications of the Leander McCormick Observatory, Virginia," vol. i. part 2.—"Algebra," part 1, by G. Chrystal (Black, Edinburgh).—"The Gold Fields of Victoria: Reports of the Mining Registrars for the Quarter ended March 31, 1886" (Ferres, Melbourne).—"Science and Art Directory," revised to July 1886.

CONTENTS

THURSDAY, AUGUST 19, 1886

PHYSICAL HYPOTHESES

Le Ipotesi Fisiche. Analizzate da Giannantonio Zanon. (Venezia: Lorenzo Tondelli, 1885.)

AT its sitting of July 17, 1881, the Royal Venetian Institute of Sciences proposed as the subject of a prize an examination of recent hypotheses regarding the causes of luminous, thermal, electrical, and magnetic phenomena. The volume now before us is one of seven competing treatises produced by the end of March 1883. As to the vastness of its scope, and the extent of erudition displayed in it, we can fully ratify the sentence of the examiners officially deputed to pronounce upon its merits. It is an "attack all along the line," and one conducted with no despicable array of mustered forces. The author has read and pondered much on the subjects he treats of; he is a mathematician, and is hence alive no less to the value of mathematical evidence, than to the worthlessness of a mere hollow show of mathematical formulæ; while the hypotheses he criticises have usually been tried by the severe test of a serious endeavour to realise their consequences. Many of his objections we at once admit to be valid; indeed, no universal explanation of physical phenomena has yet been proposed of which the structure was not riddled with visible absurdities. The late Prof. Challis devoted his considerable abilities and his best energies to the elaboration of a hydrodynamical theory of the universe, in which physical effects of all kinds were referred to forms of pressure of a continuous elastic medium. But the suggestion that an indefinite ascending series of such media might, after all, be necessary to produce the required results, cannot be looked upon otherwise than as a confession of failure. Father Secchi's heroic effort, in his "Unità delle Forze Fisiche," to see right down to the very bottom of things, was scarcely more successful. The reasonings upon which it was founded (as our author, among others, rightly points out) were vitiated at the root by a misapplication of Poinsot's theorems on the resilience of rotating bodies; and the cosmical machinery put together with such ingenuity, and set going with such heedful solicitude, came at once to a deadlock. Nor do we anticipate any better results from the scheme which Prof. Zanon himself promises to expound in a forthcoming work. The glimpses of his views afforded in essays already before the public are not encouraging. There is absolutely nothing gained in devolving the responsibility of our ignorance upon phrases, and taking their obscurity for illumination. If we can find no adequate explanation of the activities manifested in the wonderful "frame of things," of which we are at once spectators and participators, let us, in the name of candour and common-sense, acknowledge our impotence; but let us not imagine that we in any sense repair or qualify it by talking of inherent qualities, "virtues," propagated "influences," "molecular tensions," and the like. This would be to fall back into the rut out of which Molière did something to help us with Argan's famous diploma-examination:—

"Mihi a docto doctore
Domandatur causam et rationem quare
Opium facit dormire.
A quoi respondeo,
Quia est in eo
Virtus dormitiva,
Cujus est natura
Sensus assoupire."

Undeniable; but uninstructive.

The work we are at present concerned with is divided into an historical and a critical section. Of hypotheses as to the constitution of matter and the causes of physical action, the human mind has in all ages been prolific. Their procession, lengthy as that which defiled before Bradamante in the grotto of Merlin, is here carefully, and as expeditiously as may be, marshalled for our benefit. From the aqueous world of Thales to the vortex-atom world of Sir William Thomson, we are enabled to trace the progress, or rather the vicissitudes, of thought. For it is not in this field that the amazing advances of modern science have been made. The subject of Prof. Zanon's sketch is, not the onward march of natural investigation, but the "stations and retrogradations" of speculative physics. More than once, indeed, he is forced to exclaim with Horace, "Multa renascentur, quæ jam cecidere." The incidental inclusion of some fragments of genuine scientific theory, legitimately verified by experience, does not alter this general character.

The narrative is disfigured by a few inadvertencies. At p. 44, for example, the origination of the wave-theory of light is ascribed to Malebranche; but in the very next page, and with better reason, to Hooke, whose "Micrographia," containing the assertion that "light is a very short vibrative motion," propagated in spherical waves of agitation through "an homogeneous medium," appeared in 1665, nine years before the first-fruits of Malebranche's meditations were given to the world. At p. 50, Lavoisier's *rationale* of combustion is inextricably involved with his *rationale* of the decomposition of water; and at p. 118 the point is strangely missed of Prof. Tyndall's illustrative explanation of the change from the liquid to the gaseous state. It is worth while to point out these slips in a book representing a large amount of painstaking original research.

No notice is taken in it of Isenkrahe's recent speculations concerning the cause of gravity. They belong to the same family with those of Le Sage, justly condemned as "a mere effort of imagination, defiant alike of the dictates of reason and the laws of nature" (p. 229). Shelter from molecular bombardment, in one form or the other, is the key offered by them to the standing riddle of the cosmos. They explain gravity as a *push*, not a *pull*. Central forces are replaced by the preponderant external impacts of "mundane" or "ultramundane" particles. Such theories write their own sentence. They include their own condemnation. For, as M. Isenkrahe with the utmost candour points out, the very form of his fundamental equation implies a contradiction of the law that gravity varies with mass. It is obeyed only when the value of the equation becomes equal to nothing—that is, when there is no action of the kind postulated.

In the critical section of Prof. Zanon's volume, theories of the constitution of matter are examined separately from theories of the physical forces. That perplexities

exist in both departments of research is a statement equivalent to the as yet undisputed assertion that human faculties are limited and human knowledge imperfect. Nevertheless, some of the difficulties raised by him are well worthy of thoughtful consideration. There can be no doubt, for instance, that the results of M. Gustav Hirn's experiments on the resistance to motion of air at different temperatures are gravely embarrassing to the kinetic theory of gases. The resistance *ought* to vary as the square root of the absolute temperature; it continues, nevertheless, practically unchanged at all temperatures, so long as the density remains the same (*Comptes Rendus*, t. xciv. p. 379).

Our author's strictures, however, although couched in terms of praiseworthy moderation and fairness, are somewhat too indiscriminate. His respect for thoroughgoing scientific work is evident and unfeigned; but it is almost impossible to take up the position of a sceptic *ex professo*, without at times incurring the reproach of perversity. We are totally unable to see the force of his reasons for preferring the obscure notion of a threefold spectrum containing chemical rays specifically distinct from thermal, and thermal from luminous, to the simple and intelligible view which substitutes for intangible differences of quality in radiations, measurable differences of wave-length. Nor can we believe that he would, on mature consideration, attempt to maintain the opinion that the planetary movements would remain unaffected by the progressive transmission of gravity. It is demonstrable that a species of aberration would thence ensue involving accelerative effects the insensibility of which so far proves (as Laplace has shown) that gravitative influence travels *at least* fifty million times faster than light. We owe the remark to Mr. William B. Taylor (*Smithsonian Report*, 1876, p. 212), that if one minute were spent on the journey from the sun to the earth, the consequent slight obliquity of traction, represented by an angle of 2″·4, would produce a shortening of the year perceptible while the sun was finishing a single round of the Signs.

Prof. Zanon appears to have bestowed much intelligent study on spectroscopic science. He carefully examines Mr. Lockyer's discovery of the independent "behaviour" of iron lines (amongst others) in spots and prominences, and though rejecting the inference of dissociation, he admits the necessity of assuming an *ad libitum* number of allotropic forms of the substance in question in order to explain anomalous appearances. He published moreover in 1880 a detailed analysis of Mr. Lockyer's "Studies in Spectrum Analysis," deriving therefrom confirmation in many particulars of his own views as to the nature of matter. Whatever we may think of these, there can be no doubt that our hope of penetrating the mysteries of molecular constitution must in future rest on the disclosures of the spectroscope. We do not in the least believe that these point towards the conclusion deprecated by Prof. Zanon of a fundamental unity of substance. On the contrary, Mr. Lockyer's striking observations indicate (if we may be allowed the expression) divergent, rather than convergent, simplifications.

It is perhaps inevitable in a work of the character of that now under consideration that the value to science of "working hypotheses" should be taken little account of. Yet the story of man's progressive knowledge of nature is a story of tentative efforts to represent facts to thought. Without some method of provisionally co-ordinating phenomena, indeed, the advance from a lower to a higher stage of induction, which we call discovery, could scarcely at all be effected. The true investigator is he who is never weary of collecting particulars to fit into his framework of theory, and of reconstructing his framework of theory to match fresh particulars. Without the power of thinking appearances into shape, no diligence in amassing the details of those appearances is of the slightest avail.

There is, it is true, the danger of a working hypothesis hardening unduly or prematurely into a theory—of a mere track, struck out for surveying purposes, turning into a fenced highway, without exit save in a quagmire. Against this peril the activity of such critics as Prof. Zanon is a very effectual safeguard. Their assaults may sometimes appear vexatious, but are really directed in the best interests of science, as hindering false security, and leading to a juster estimate of probabilities.

A. M. CLERKE

A MANUAL OF MECHANICS

A Manual of Mechanics; an Elementary Text-Book, designed for Students of Applied Mechanics. By T. M. Goodeve, M.A. (London: Longmans, 1886.)

THIS little work is evidently meant for readers whose knowledge of mathematics is small. The amount of mathematical knowledge supposed may, perhaps, be best inferred from an example quoted from the book itself. In p. 32 we have the problem to find the resultant of two equal rectangular forces, P. If R is the resultant, $R = P\sqrt{2}$. "Now $\sqrt{2} = \sqrt{\frac{50}{25}} = \sqrt{\frac{49}{25}}$ very nearly; ∴ $\sqrt{2} = \frac{7}{5}$ very nearly. Whence $R = \frac{7}{5}P$." When the angle between the lines of action of the forces is 120°, R should be equal to P; but it is proved in p. 33 to be equal to $P\sqrt{3}$, by an unaccountable error in the application of an elementary formula. However, $\sqrt{3}$ is found by the same process as before. Thus $\sqrt{3} = \sqrt{\frac{48}{16}} = \sqrt{\frac{49}{16}}$, very nearly, $= \frac{7}{4}$.

Now what naturally strikes one about beginners who have to be spoon-fed in this fashion is that it would be very much better to spend some time in teaching them trigonometry and algebra, than to push them into dynamics with the very slender knowledge of arithmetic and geometry which they possess. In our system of national education there is far too much effort employed in bringing branches of dynamics and physics *down* to the level of the ignorant, and too little in bringing students *up* to the standard of mathematical attainment required for a really intelligent knowledge of these subjects. The result of this system is that nearly all our elementary scientific books are very scrappy in character, presenting to their readers conceptions which they cannot possibly realise, and which must therefore be crammed into the mind as mere definitions devoid of real meaning.

In elementary scientific treatises it is necessary to insist, above all things, that the information given to the

reader shall be above reproach, so that if he further pursues the subject treated of, he may, at least, not be hindered or misled by erroneous definitions or ideas placed before him at the outset.

The work before us, when it has been subjected to a slight revision, will fairly satisfy the conditions of an elementary text-book which is at once strictly scientific in its ideas and rich in practical applications.

Indeed, the practical portion of the work is, as was to be expected, very good. The portions dealing with mechanism are well suited to illustrate the scientific principles, and will be found very useful.

We shall confine ourselves to pointing out some of the blemishes which disfigure the theoretical portion.

P. 1. "When the tendency which force exerts to move a body is counteracted, so that the body remains at rest, the force is commonly called a *pressure*." It may be safely said that no such limited use is ordinarily made of the term *pressure*.

P. 2. The statement that two lumps of sugar placed in contact on a table do not adhere, because "the attraction emanates from their centres, which may be an inch apart," is not altogether conducive to sound notions.

In p. 19 we have the usual definition of the measure of a variable velocity, viz. "the space which would be described in a unit of time if the body retained throughout that unit the velocity which it has at the instant considered." When will people find out that this definition really defines nothing? It is truly Gladstonian in its elusiveness. Of course we find exactly the same "definition" of variable angular velocity (p. 62), variable acceleration (p. 99), and variable pressure (p. 176).

In p. 26 we have a most confusing exposition of Newton's Second Law. What, for instance, is the beginner to understand by this?—"Since we are dealing with a body in motion, it is clear that we may consider (1) the weight of the body to be constant, and its velocity to vary; (2) the weight of the body to vary, and its velocity to remain constant." The discussion of this law seems to show that its object is entirely misconceived by the author. For, while the law aims at giving a complete measure of *force*, the author merely *deduces from it* (by reasoning in a circle) a definition of momentum, his discussion ending with the words, "Hence, quantity of motion = (mass) (velocity)."

Again, in p. 28, "the explosive force of powder in a gun is *action*, and the momentum generated in the projectile is *reaction*." This is plainly open to the objection of equating force to momentum—a most mischievous notion.

In p. 48 the full meaning of the *centre of parallel forces* is by no means brought out, and the rule for finding its distance from a given line is incorrect: "Multiply each force into its perpendicular distance from a given line," &c. The rule ought to be enunciated with reference to a *plane*, and not to a *line*, and instead of the unmeaning term "distance of a force from the plane" we should say "distance of its point of application from the plane."

It is surely incorrect to say, as in p. 61, that "it is the universal practice to appropriate the Greek letter ω (omega) for expressing the unit of angular velocity."

It is very important to distinguish, in the mind of the beginner, between *force* and *power*. In p. 72 power is correctly defined as [time-] rate of doing work; but the language immediately following loosens this idea. Thus, "the number 33,000 foot-pounds is the *unit* of horse-power." There is tautology in the expression "Watt estimated that the sustained work of a horse continued for one minute would raise 33 000 lbs. through one foot in one minute." Again, the term *power* is used in p. 94 to designate a force applied to a machine.

In p. 73 the principle of work as applied to equilibrium is thus enunciated: "If a system of bodies be at rest under the action of any forces, and be moved a very little, no work will be done." It may well be doubted whether this enunciation is explicit enough to give a useful idea of the great statical principle. Indeed, the cases selected for illustration bear out in no way the proviso that the bodies must "be moved a very little."

Passing on to p. 98, we find acceleration of velocity discussed, and we have the formal definition: "the unit of acceleration adds a velocity of 1 foot in 1 second," which, as the words stand, has absolutely no meaning. The beginner ought to be taught that acceleration has a double reference to time, and it should never for a moment be spoken of otherwise than as "feet per second per second," or "miles per hour per minute," or by some other equivalent phrase. In p. 102, g is spoken of as 32·2 feet per second.

Finally, in p. 113 the beginner is introduced to the discussion of a compound pendulum formed by a straight uniform bar. Its time of oscillation depends on a radius of gyration. What idea is our beginner likely to have of this? The definition of k, the radius of gyration of the bar AB about an axis through its centre of gravity, G (p. 114), is quite erroneous—"let k^2 be the sum of the squares of the distances of each particle of AB from the point G." This sum is, of course, infinite. What is meant by k^2 is the *mean* square of distance from G. But clearly such a question is wholly unsuited to beginners.

The book concludes with a large collection of examples, taken from the Science Examinations, which will be useful to students. GEORGE M. MINCHIN

OUR BOOK SHELF

Mémoire sur les Volumes moléculaires des Liquides, avec un Avant-propos. By Hermann Kopp. Pp. 47. (Heidelberg: C. Winter, 1886.)

THIS is a *brochure* one-third of which is devoted to an "avant-propos" explaining why the paper does not appear in the *Annales de Chimie et Physique*, and making certain statements relating to M. Berthelot's recent work, "Les Origines de l'Alchimie." The remaining part contains a discussion of a paper on molecular volumes of liquids which appeared in the *Annales*. In 1869 Prof. Kopp published, in succession to his "Geschichte der Chemie," a volume of "Beiträge" to the history, explaining more fully the principal epochs in chemical history, more especially in relation to alchemy. In 1885 M. Berthelot published his work, which had occupied more or less of his attention since 1869. Its general character is very similar to the "Beiträge," though much less complete, and Kopp complains that no reference whatever is made to his book. Berthelot, in reply, states that he was unaware of the existence of the "Beiträge," yet he quotes references to works in which this "cahier" (as he calls it) is repeatedly mentioned. The oversight is a very grave one, when we remember that the "cahier" is a contribution of over

800 pages, and when we consider the position which Prof. Kopp holds as an historian of chemistry, a position which demands that due regard should be paid to his writings.

The more immediate object of the pamphlet is, however, to reply to the strictures of MM. Bartoli and Stracciati on the law of molecular volumes. These gentlemen have criticised somewhat severely Kopp's work in this department without taking into consideration the obstacles which, at the time it was carried out (1855) stood in the way of accurate and definite investigation. Singularly enough, they do not escape the same charge, having themselves in some cases made use of materials of an insufficient degree of purity. It is likewise pointed out that they are labouring under a complete misapprehension of the views held by Kopp and the significance of his deductions, nor do they seem to have appreciated the difficulties that surround the establishment of a "physical law" of general application. G. H. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Organic Evolution

ALLUDING to the first instalment of your abstract of my recently published paper on "Physiological Selection," the Duke of Argyll remarks :—

"I rejoice that the author has at last discovered that 'natural selection has been made to pose as a theory of the origin of species, whereas in point of fact it is nothing of the kind.' This has been my contention for many years" (NATURE, vol. xxxiv. p. 335).

These words seem to imply that my views with regard to natural selection have now undergone an important change, and one which brings them into conformity with those that have for many years been contended for by the Duke. It therefore seems desirable to state that such is not the case; and as I can only attribute the misunderstanding of so able and friendly a reader to some ambiguity in the condensed abstract of my paper to which he refers, I may invite him to consult the paper itself, where the matter to which he alludes is more fully explained. He will there find that my views upon the subject of natural selection are the same now as they have been during the last fifteen years; that in all essential respects they still coincide with those that were held by Mr. Darwin; and that my "additional suggestion on the origin of species, although quite independent of natural selection, is in no way opposed to natural selection," but is to be regarded as indicating "a factor supplementary to natural selection."

The state of matters, then, is simply this. Mr. Darwin himself has freely acknowledged that his theory of natural selection is not in itself a sufficient explanation of the origin of species. He therefore supplemented the natural causes which are together comprised under this term [1] by sundry other causes of a similarly natural kind. Of these he attributed most importance to use, disuse, sexual selection, correlated variation, and prolonged exposure to uniform conditions of life. To these supplementary causes Moritz Wagner added independent variation in the absence of intercrossing with parent forms, while I have myself added physiological selection. Now, the whole body of Mr. Darwin's followers have agreed with him in holding that the theory of natural selection is not in itself a sufficient explanation of the origin of species. But many of these followers differ from Mr. Darwin, and also differ among themselves, as to the proportional part which the principle of natural selection is to be considered as having played in the evolution of species. Mr. Darwin thought that in this respect natural selection plays a more important part than any other principle [therefore it is hard to see how in this respect any of "the successors of Darwin" can possibly "have run quite wild from the teaching of their master"], while in the opinion of many of his followers this principle should be regarded as of a value subordinate to the others. Of all the writers who have taken the latter view, the most clear-headed, as well as the earliest and most persistent, is Mr. Herbert Spencer. He more than any other author has been instant, both in season and out of season, in giving reasons for the scepticism that is in him. I confess, therefore, to not understanding the Duke of Argyll when he says that in the two articles recently published by Mr. Spencer "we have for the first time an avowed and definite declaration against some of the leading ideas on which the Mechanical Philosophy depends." So far as I can see, these two articles convey little more than a reiteration of the characteristically Spencerian view that, in the course of organic evolution the processes of "direct equilibration" have been of more importance than those of "indirect equilibration." By the first of these terms Mr. Spencer means use, disuse, and all other causes tending directly to the production of adaptive structures, while by the second he means natural selection. Now, from the time when Mr. Darwin first published his "Origin of Species" the main point of difference between his views and those of Mr. Spencer has uniformly consisted in the estimates which they have formed of the relative importance of these two kinds of equilibration. Surely, therefore, this cannot be the respect in which it is said that Mr. Spencer has "now for the first time opened his eyes and his mouth." Yet, if not, I do not understand the allusion. The Duke seems to imagine that, in some way or another, Mr. Spencer has taken a new and important "point of departure" in the course of his speculative thinking, and one which is "fatal to the adequacy of the Mechanical Philosophy as any explanation of organic evolution." It would be a matter of great interest to me—and I am sure to many others who have read the articles in question—to be told in what respect Mr. Spencer has committed himself to so great a change of doctrine; and it would certainly be a matter of profound astonishment to all evolutionists if Mr. Spencer can be shown to have so much as insinuated that his "direct equilibration" differs from Mr. Darwin's natural selection in not belonging to the system of so-called Mechanical Philosophy—or that those factors of organic evolution on which he has mainly relied differ from those on which Mr. Darwin has mainly relied in lending better countenance to the Supernatural Philosophy of Design.

My own attitude with regard to all these questions is perfectly plain and simple. In common with Darwin, Spencer, and the great majority of evolutionists, I believe that in the origin and development of adaptations—whether structural or instinctive—two sets of strictly natural causes have been at work: I agree with Mr. Darwin in thinking that of these two sets of causes the "indirectly equilibrating" have been of more importance than the "directly equilibrating": but I differ from other evolutionists, both of the Darwinian and Spencerian schools, in expressly drawing a marked distinction between causes of either kind which have been operative in the evolution of *adaptations* and those which have been operative in the evolution of *species*; and, lastly, I claim to have shown that when once this distinction is recognised, the theory of natural selection ceases to be, properly speaking, a theory of the origin of species; that it is thus liberated from all the difficulties with which it has hitherto been entangled on account of its having been made to "pose" as such; and that it is therefore placed in a position of greater

[1] In common with many other critics of Mr. Darwin's work, the Duke of Argyll has always contended that the theory of natural selection is "fundamentally erroneous" in that it assumes "variations to arise by accident," and merely "groups under one form of words, highly charged with metaphor, an immense variety of causes, some purely mental, some purely vital, and others purely physical or mechanical." This, however, is no valid criticism of the theory, which for the first time did comprise under one general point of view all the causes which together go to produce the results. In the opinion of the Duke, the weakest element of the theory consists in its inability to explain the causes of those variations on the occurrence of which the theory depends (NATURE, *loc. cit.*, p. 336). But it is clearly no valid objection to a theory which explains one set of causes that it is unable to explain another and ulterior set. So long as variations of all kinds are known to be matters of fact, they are available for the theory of natural selection, even though the ulterior or physiological causes of variation should never be discovered. And for all the purposes of this theory it makes no difference whether the variations which are seen to take place in all directions, *with and without respect to utility*, are spoken of as "accidental" or as due to hidden causes. All that this theory has to do is to take the principle of promiscuous variation in all directions as a datum supplied by observation, and from this fact to show how the further principles of heredity, struggle, and survival are competent to *select* the variations which happen to be useful from those which are not. One might as well object to the physical explanation of specific gravity in selecting or sorting the different materials of a sea-shore, on the ground that we do not know the causes either of gravity in general or of the variations that are observable among specific gravities in particular.

logical security than it has ever occupied before. Far, then, from joining the "contention" of my critic in seeking to depose natural selection as a theory of the genesis of adaptive structures and instincts, I have expressly sought to fortify that theory as a "mechanical" explanation of these phenomena. Whether or not I have been successful I must leave others to judge, *after they have done me the justice to read my original paper.* But, be this as it may, the ambiguity of that paper must indeed be extraordinary, if it leads any one to suppose that my argument is precisely the opposite of what it is intended to be.

Geanies, Ross-shire, August 16 GEORGE J. ROMANES

Meteorology and Colliery Explosions

THE catastrophe at Woodend Colliery on Friday last again directs attention to the connection generally believed to exist between explosions of fire-damp and atmospheric changes. The real nature of this connection is but little, if at all, understood. From time to time observations have been taken with the view of throwing some light on the subject, but as the observations in one mine were discussed without reference to what may have been occurring in other districts, the results have not added much to our knowledge. Meteorology, however, is now sufficiently advanced to permit the adoption of another and more satisfactory method of dealing with the question.

The idea so long prevalent that certain fixed points on the barometric scale indicated certain kinds of weather has now been discarded, the examination of synchronous weather charts showing conclusively that the weather changes are not so much dependent upon the height of the barometer at any one place as upon the relations existing between readings over a tract of country; the direction in which the highest and lowest readings lie, and the difference of pressure (the barometric gradient) between neighbouring places—these form the basis of our modern weather knowledge. Nothing of this kind has hitherto been attempted when dealing with observations from collieries. If the presence of gas in mines is in any way regulated by changes of atmospheric pressure, it would be well to see if, like the weather, it is distributed in areas, and whether in these areas again some localities would have more gas than others, dependent more upon the distribution of pressure than upon local readings. With the object of discovering some law or laws governing the presence of gas, I appeal to colliery officials in every coal-field in Great Britain and Ireland to supply me with the few observations detailed below for a period of four months—from September 1 to December 31, the best part of the year for such work.

The Meteorological Office Weather Charts issued daily (Sundays included) at 8 a.m. and 6 p.m. show the distribution of pressure, winds, temperature, and weather. To these I propose adding the information supplied from mines *at the same hours.* Those who cannot arrange for two observations daily, to give preference to the morning set. The gas observation being the most important, I would be glad if precise information can be given. Absolute uniformity cannot be expected, but I would suggest that, where possible, a disused gallery favourable to the object in view should be used, one where the changes in the quantity of gas can be stated in yards or feet, thus turning the gallery into a gas barometer somewhat similar to the one at Seaham Colliery after the explosion of a few years ago. Those who have not the facilities for such measurements can still give valuable data if they do no more than note the increase or decrease of gas as "slight," "moderate," or "great." The appended specimen of the form for recording the observations

Form for Recording Observations

.... Colliery, near ... .

Date, September 1886	Top of shaft, ... feet above sea-level		In underground workings, ... yards from shaft ...... feet below pit-bank			Gas in gallery, yards	Remarks
	Temperature in the shade	Weather	Barometer	Temperature	Quantity of air passing		
1st, 8 a.m.							
6 p.m.							
2nd, 8 a.m.							
6 p.m							

has columns for the air-temperature in the shade at the pit-bank, and the weather; while underground, in addition to the gas-record, the reading of a barometer and thermometer, and quantity of air passing at a fixed point some distance in the workings; also, remarks on the variations of the gas and ventilation at other than the regular hours. A sheet of close-ruled foolscap, arranged as indicated, will contain the data for one month, and, as soon as filled up, should be forwarded to me at the Meteorological Office, London, S.W. On the back of the first return particulars are required of the geographical position of the mine, the name and address of the manager, whether the barometer is a mercurial or an aneroid, together with the readings of the same at the pit-bank at 8 a.m. daily for a week before taking it underground, and describing the plan adopted in measuring the gas. It must be understood that I am undertaking the discussion as my own work, for which the Meteorological Council is not responsible. HY. HARRIES

August 16

Railway Weather Signals

WITH reference to the notice given in NATURE, vol. xxxiv. p. 347, of the ingenious plan adopted by the Norwegian Meteorological Institute for disseminating its weather reports, it may not be known to all of your readers that a similar system of signals has been in use for some time on the railroads in Ohio, Pennsylvania, and Canada. The day signals there consist of sheet-iron disks about three feet in diameter, and are displayed on the side of baggage-cars. The signals are shaped like the sun, a crescent, or a star, and differ in colour, being red or blue. The red colour refers to the temperature, and the blue to the state of the weather, as rainfall or snow. This system of signals was first brought into practical operation by Prof. T. C. Mendenhall, Chief of the Ohio Meteorological Bureau.

A system of night-signals for railways is also under trial in Pennsylvania: they are in the form of rockets or an exploding cartridge, which, when fired, may be seen from six to ten miles. CHAS. HARDING

August 17

Tornaria and Actinotrocha of the British Coasts

THREE species of Balanoglossus are known to occur on the shores of North-West Europe. *Balanoglossus kupfferi* was taken by Willemoes Suhm at Helleback, in the Sound, that is, on the coast of Zealand (*Zeit. f. wiss. Zool.* vol. xxi. 1871); *Balanoglossus salmoneus*, Giard, and *B. robinii* occur, according to Mr. Bateson's last paper in the *Quarterly Journal of Microscopical Science*, at Concarneau, in Finistère, and I believe also at the Channel Islands. But no Balanoglossus has yet been found on the shores of Britain. It will therefore be of some interest to British naturalists to learn that in August 1884 I obtained in the tow-net a larva which seemed to possess the distinctive characters of Tornaria. I had not leisure at the time to study the specimen with much attention, but I made a hurried sketch of it, which shows the presence of two parallel longitudinal bands of cilia anteriorly, and a single transverse band posteriorly. At the posterior end is a conical protuberance resembling the adhesive organ described by Bateson in his creeping larva. The position of the mouth was not ascertained, but was probably between the two anterior bands of cilia. The water vessel and tuft of cilia at the apex of the præoral lobe were not observed. This larva may not have been Tornaria, but I think it really was that form; and naturalists who are spending an autumn holiday at the sea-side would probably, if they undertook the search, succeed in finding Balanoglossus in the littoral sands, and its larvæ in the shore waters.

Phorenis is also at present, I believe, excluded from the British littoral fauna, but is certainly present on our shores, though no adult specimens have been taken. I took large numbers of Actinotrocha in the tow-net, close to the shore, in September 1883, a little to the north of the mouth of the Cromarty Firth. If I am mistaken in supposing that adult Phorenis and Balanoglossus have never been found on the coast of Britain, I shall be glad to be corrected. J. T. CUNNINGHAM

Mock Suns

As I observe the omission from my letter on the parhelia or mock suns of last month (p. 313) of the diagram which was

intended to describe the real phenomena, may I ask you to allow me a few lines of space for the following :—

The real sun was surrounded at a short distance by a halo or rainbow circle of great beauty, with a mock sun of the same apparent magnitude and brightness on the right and left ; and *partially* formed suns above and below the ring : all of them being slightly opalescent. From the two perfected suns proceeded cones of intense light, about 3 diameters of the sun in length, and with their apices pointing east and west. These were rather more opalescent than the mock suns from which they seemed to originate. A second rainbow ring at a considerable distance outside of these extended to the zenith. The period of greatest beauty and brightness, when they were as rich in colouring as a real rainbow, lasted about 5 minutes. I was able to watch the whole of the phenomena from a little after 4 to nearly 6 o'clock.

ROBERT H. F. RIPPON

Jasper Road, Upper Norwood

PHYSIOLOGICAL SELECTION: AN ADDITIONAL SUGGESTION ON THE ORIGIN OF SPECIES[1]

III.

Argument from the Prevention of Intercrossing.—This argument is the same from whatever cause the prevention of intercrossing may arise. Where intercrossing is prevented by geographical barriers or by migration, it is more easy to prove the evolution of new species as a consequence than it is when intercrossing is prevented by physiological barriers ; for in the latter case the older and the newer forms will probably continue to occupy the same area, and then there will be no independent evidence to show that the severance between them was due to the prevention of intercrossing. Nevertheless, all the evidence I have of the large part that geographical barriers have played in the evolution of species by preventing intercrossing with parent forms goes to show the probable importance of physiological barriers when acting in the same way. Hence it will be better to postpone this line of argument in favour of physiological selection until the appearance of my next paper, where I shall hope to show, from evidence furnished by the geographical distribution of species, how predominant a part the prevention of intercrossing has played in the evolution of species. Here, therefore, I will merely remark that wherever intercrossing with parent forms is prevented, in the proportion that it is prevented a better opportunity is given to natural selection for seizing upon any beneficial variations that may happen to arise. On this account physiological selection probably lends important aid to natural selection, thus becoming indirectly instrumental in the evolution of useful as well as of useless structures.

There is also another respect in which these two kinds of selection probably co-operate. For Mr. Darwin shows that "it would be clearly advantageous to two varieties, or incipient species, if they could be kept from blending, on the same principle that, when man is selecting at the same time two varieties, it is necessary that he should keep them separate." But he proceeds to show that this advantage cannot be conferred by natural selection, and hence that the sterility which is so generally characteristic of species cannot be attributed to this agency. We have, however, just seen that this sterility is in all likelihood due to physiological selection ; and therefore, if it be true, as Mr. Darwin thought, that "it would profit an incipient species if it were rendered in some slight degree sterile with its parent form," physiological selection and natural selection may mutually assist one another. For, although the benefit of this sterility could not have been initially conferred by natural selection, yet when it once arises from an independent variation in the reproductive system, there is no reason why it should not forthwith be favoured by natural selection, just as is the case with advantageous variations in general.

Feeling how grave a difficulty was presented to his theory of the origin of species by the general sterility of species, Mr. Darwin was extremely anxious to find some way in which natural selection might be seen to have brought about this result. Had it occurred to him that this result was probably nothing more than the necessary expression of a particular kind of variation on the part of the reproductive system, I cannot doubt that he would have felt the theory of natural selection to have been relieved of one of its greatest disabilities.

Argument from the Inutility of Specific Differences.—After what has already been said on this subject, I will here only deal with one question, namely, Why is it that apparently useless structures occur in such profusion among species, in much less profusion among genera, and scarcely at all among families, orders, and classes? It may be answered that the points wherein species differ from species are usually points of smaller detail than those which distinguish genera, families, &c., and thus may well actually be as a rule less useful, although still not absolutely useless : natural selection, it may be urged, is better able than is the naturalist to diagnose utility. But here again we have a most unwarranted appeal to the argument from ignorance ; whereas, according to my view, it is quite intelligible that when a varietal form is differentiated from its parent form by the bar of sterility, isolation, or migration, any little meaningless peculiarities of structure (or of instinct[1]) should at first be allowed to arise, but should eventually be eliminated as so much surplusage in the struggle for existence, by economy of growth, or even by independent variation when undirected by natural selection. A greater or less time would in different cases be required to effect this reduction, and thus we can understand why they are sometimes allowed to persist into genera, but rarely into families.

Again, if apparently useless specific characters (whether these be new structures or modifications of old ones, slight changes in form, colour, and so forth) are thus regarded as really useless, we should expect that they ought to be of a kind which do not impose much physiological tax upon the organism, since otherwise natural selection would not have allowed them to become so much as specific characters. Well, I have applied this test, and find it is a most general rule that specific characters the utility of which cannot be perceived are such as do not impose any considerable demand for nourishment : either on account of their small size or of their organically inexpensive material, they do not impose much tax upon the organism. Now it is obvious that there can be no connection between utility as disguised and smallness of size or inexpensiveness of material ; while it is no less obvious that there is a close connection between these things and a real inutility.

Lastly, our domesticated varieties occasionally exhibit well-marked and more or less constant characters of a useless kind. Here there can scarcely be any question about the genuineness of the inutility, seeing that the characters have arisen only under domestication, or in the absence of any struggle for existence. Yet these structures are sometimes of the most curious and complex morphology—even more so than innumerable apparently useless structures in the case of natural species.[2]

Argument from Divergence of Character.—Any theory of the origin of species in the way of descent must be prepared with an answer to the question, Why have species *multiplied?* Why have they not simply become transmuted in linear series instead of ramifying into branches? This question Mr. Darwin seeks to answer

[1] Abstract of a Paper read before the Linnean Society on May 6, by George J. Romanes, M.A., LL.D., F.R.S. &c. Continued from p. 340.

[1] For instances of useless instincts see Mr. Darwin's posthumous essay published in my "Mental Evolution in Animals." It is suggestive in the present connection that, just like useless structures, useless instincts, so far as I can find, only occur in species and genera : never in families, orders, or classes.

[2] For a good instance of this see "Variation of Plants and Animals under Domestication," vol. i. pp. 78-79.

"from the simple circumstance that the more diversified the descendants from any one species become in structure, constitution, and habits, by so much will they be better enabled to seize on many and widely diversified places in the economy of nature, and so be enabled to increase in numbers."[1] And he proceeds to illustrate this principle by means of a diagram, showing the hypothetical divergence of character undergone by the descendants of seven species. Thus, he attributes divergence of character exclusively to the influence of natural selection.

Now, this argument appears to me unassailable in all save one particular; but this is a most important particular: the argument wholly ignores the effect of intercrossing with parent forms. Granting to the argument that intercrossing with parent forms is prohibited, and nothing can be more satisfactory. The argument, however, sets out with showing that it is in limited areas, or in areas already overstocked with the specific forms in question, that the advantages to be derived from diversification will be most pronounced. Or, in Mr. Darwin's words, it is where they "jostle each other most closely" that natural selection will set a premium upon any members of the species which may depart from the common type. Now, inasmuch as this jostling or overcrowding of individuals is a needful condition to the agency of natural selection in the way of diversifying character, must we not feel that the general difficulty from intercrossing previously considered is here presented in a special and aggravated form? At all events, I know that, after having duly and impartially considered the matter, to me it does appear that, unless the swamping effects of intercrossing with the parent form on an overcrowded area is in some way prevented to begin with, natural selection could never have any material supplied by which to go on with. Let it be observed that I regard Mr. Darwin's argument as perfectly sound where it treats of the divergence of *species* from one another—*i e.* of the rise of genera, families, &c.; for then physiological barriers are present to prevent intercrossing. But in applying the argument to explain the divergence of *individuals* into *varieties* it seems to me that here, more than anywhere else, he has lost sight of the formidable difficulty in question. For in this particular case so formidable does the difficulty seem to me, that I cannot believe natural selection alone could produce any divergence of character so long as all the individuals on an overcrowded area occupy that area together. Yet if any of them quit that area, and so escape from the unifying influence of free intercrossing, these individuals also escape from the conditions which Mr. Darwin names as those that are needed by natural selection in order to produce divergence. Therefore it appears to me that, under the circumstances supposed, natural selection alone could not produce divergence; the most it could do would be to change the whole specific type in some one direction (the needful variations in that one direction being caused by some general change of food, climate, habit, &c., affecting a number of individuals simultaneously), and thus induce transmutation of species in a linear series—each succeeding member of which might supplant its parent form. But, in order to secure diversity, multiplication, or ramification of species, it appears to me obvious that the primary condition required is that of preventing intercrossing with parent forms at the origin of each branch—whether the prevention be from the first absolute, or only partial. And, after all that has been previously said, it is needless again to show that the principles of physiological selection are at once the only principles which are here likely to be efficient, and the principles which are fully capable of doing all that is required. For species, as they now stand, unquestionably prove the fact of ramification; and it appears to me no less unquestionable that ramification, as often as it has occurred, can only have been permitted to occur by the absence of intercrossing with parent forms. But apart from geographical barriers (which, according to Mr. Darwin's argument, would be inimical to the divergence of character by natural selection), the ramification can only take place as a consequence of physiological selection, or as a consequence of some change in the reproductive system which prevents intercrossing with unchanged (or differently changed) compatriots. But when once this condition is supplied by physiological selection, I have no doubt that divergence of character may then be promoted by natural selection, in the way that is explained by Mr. Darwin.

From which it will be seen that the theory of physiological selection has this advantage over the theory of natural selection in the way of explaining what Mr. Darwin calls diversification of character, or what I have called the ramification of species. This diversification or ramification has reference chiefly to the secondary specific distinctions, which, as we have seen, the theory of natural selection supposes to be the first changes that occur, and, by their occurrence, to induce the primary distinction of sterility. My theory, on the other hand, inverts this order, and supposes the primary distinction to be likewise (in most cases) the primordial distinction. Now, the advantages thus gained are twofold. In the first place, as just shown, we are able to release the principle of natural selection from what appears to me the otherwise hopeless difficulty of effecting diversification of character on an overcrowded area with nothing to prevent free intercrossing. And, in the next place, as we can now see, we are able to find an additional reason for the diversification of character, over and above the one that is relied upon by Mr. Darwin. For, by regarding the primary distinction of sterility as likewise the primordial distinction, we are able to apply to an incipient variety, inhabiting even an overcrowded area, the same principles which are known to lead to diversification by geographical barriers or by migration, as previously explained. In other words, if once we regard the primary distinction of sterility as also the initial distinction, instead of the incidental result of secondary distinctions, Mr. Darwin's argument touching the causes of diversification is not merely saved: it is notably extended by the addition of an independent principle, which, as we know from other evidence, is a principle of high importance in this respect.

Argument from Geographical Distribution.—The body of evidence under this head is too large to be given in an abstract; but the following are some of the chief points.

Mr. Darwin took a great deal of trouble to collect evidence on the two following facts, namely, (1) that "species of the larger genera in each country vary more frequently than the species of smaller genera"; and (2) that "many of the species included within the larger genera resemble varieties in being very closely, but unequally, related to each other." By larger genera he means genera containing many species, and he accounts for these general facts by the principle "that where many species of a genus have been formed, on an average many are still forming." But how forming? If we say by natural selection alone, we should expect to find the multitudinous species differing from one another in respect of features presenting utilitarian significance; yet this is precisely what we do not find. For Mr. Darwin's argument here consists in showing that "in large genera the amount of difference between the species is often exceedingly small, so that in this respect the species of the larger genera resemble varieties more than do the species of the smaller genera." Therefore the argument, while undoubtedly a very forcible one in favour of the fact of *evolution*, appears to me scarcely consistent with the theory of *natural selection*. On the other hand, the argument tells strongly (though unconsciously) in favour

[1] "Origin of Species," p. 87.

of physiological selection. For, the larger a genus, or the greater number of species it contains, the greater must be the opportunity afforded for the occurrence of that particular kind of variation on which the principle of physiological selection depends. All the species of a genus may be regarded as so many varieties which have already been separated from one another physiologically: therefore each of them may now constitute a new starting-point for a further and similar separation—particularly as, in virtue of their previous segregation, many of them are now exposed to different conditions of life. Thus, it seems to me, we can well understand why it is that genera already rich in species tend to grow still richer; while such is not the case in so great a degree with genera that are poor in species. Moreover, we can well understand that, multiplication of species being in the first instance determined by changes in the reproductive system alone, wherever a large number of new species are being turned out, the secondary differences between them should be "often exceedingly small"—a general correlation which, so far as I can see, we are not able to understand on the theory of natural selection.

Another general fact mentioned by Darwin, and now well recognised by all naturalists, is that closely allied species, or species differing from one another in trivial details, usually occupy contiguous areas; or, conversely stated, that contiguity of geographical position is favourable to the appearance of species closely allied to one another. Of course this fact speaks in favour of evolution; but where the question is as to method, I confess that the theory of natural selection appears to me wholly irrelevant. For, in most of the numberless cases to which I allude, the points of minute detail wherein the allied species differ in respect of secondary distinctions, are points which present no utilitarian significance. And, as previously argued, it is impossible to believe that there can be any general or constant correlation between disguised utility and insignificance of secondary distinction.

Now the large body of facts to which I here allude, but which I have not space to detail, appears to me to constitute perhaps the strongest of all my arguments in favour of physiological selection. Take, for instance, a large continental area, and follow across it a chain of species, each link of which differs from those on either side of it by the most minute and trivial distinctions of a secondary kind; but all the links of which differ from one another in respect of their reproductive systems, so that no one member of the series is perfectly fertile with any other member. Can it be supposed that in every case this constant primary distinction has been superinduced by the trivial secondary distinctions, distributed as they are over different parts of all these kindred organisms, and yet nowhere presenting any but the most trifling amount of morphological change? Or, even if we were to suppose this, we have still to meet the question, How were all these trifling changes produced in the face of free intercrossing on the continental area? Certainly not by natural selection, seeing that they are all useless to the species presenting them. Let it then be by changes in the conditions of life, whether of food, of climate, or of anything else. I can conceive of no other alternative. Yet, if we accept this alternative, we are but espousing—in a disguised and roundabout way, to be sure—the theory of physiological selection. For we are thus but hypothetically assigning the causes which have induced the primary distinction in each case, or the causes which have led to the mutual sterility. For my own part, I believe that the assignation would be, in the great majority of such cases, incorrect. That is to say, I do not believe that in the great majority of such cases the trivial secondary distinctions—however these were caused—can have had anything to do with the great primary distinction. What I believe is that all the closely-allied species inhabiting our supposed continent, and differing from one another in so many points of minute detail, are but so many records of one particular kind of variation having taken place in the reproductive systems of their ancestors, and which, so often as it did take place, necessarily gave birth to a new species. The primary distinction thus became the constant distinction, simply because it was in virtue of this distinction—or in virtue of the variation which first originated this distinction—that the species became species; and the secondary distinctions thus became multitudinous, minute, and unmeaning, simply because they were of later origin, the result of spontaneous variability, unchecked by intercrossing with the parent forms, and, on account of their trivial (*i.e.* physiologically harmless) nature, unchecked also by natural selection, economy of growth, or any other principle which might have prevented spontaneous variability of any other kind.

There are many other general facts relating to geographical distribution which lend the strongest countenance to the theory of physiological selection—in particular I may mention the difficulty which Mr. Darwin experiences in accounting for the absence or rarity of transitional varieties between species inhabiting contiguous areas (*loc. cit.*, p. 134), which is just what might have been expected on my theory—but it is time that this abstract should draw to a close.

Relations between the Theories of Natural Selection and Physiological Selection.—The two theories resemble one another in the kind of evidence by which they are each supported. For in neither case is this evidence that of direct observation of the transmutation of species under the influence of the agency supposed: the evidence in each case consists in first proving the facts on which the principle depends, and then showing that the phenomena of organic nature are such as they ought to be if the principle in question has had any large share in their production. But the two theories differ in that while natural selection is a theory of the origin of genera, families, orders, and classes even more than it is of the origin of species; the theory of physiological selection is almost exclusively a theory of the origin of species. Again, the latter theory differs from the former in that the variations on the occurrence of which it depends are variations of a comparatively unuseful, or non-adaptive, kind. Nevertheless, physiological selection must be quite as vigilant as natural selection, and it seizes upon the comparatively unuseful variation of sterility with even more certainty than natural selection can seize upon any useful variation. Lastly, as will have been gathered from the foregoing abstract, the two theories are in no way opposed to one another: they are, in fact, complementary, and the principles with which they have to deal co-operative. For, on the one hand, without the assistance of physiological selection, natural selection would, I believe, be all but overcome by the adverse influences of free intercrossing—influences all the more potent under the very conditions which are required for the multiplication of species by divergence of character. On the other hand, without natural selection, physiological selection would be powerless to create any differences of specific type other than those of mutual sterility and trivial details of structure, form, or colour—differences wholly without meaning from a utilitarian point of view. But in their combination these two principles appear to me able to accomplish what neither can accomplish alone—namely, a full and satisfactory explanation of the origin of species.

Conclusion.—It has not been possible to do justice to the theory of physiological selection within the limits of this abstract. But perhaps enough has been said to show that there is a great deal of evidence in its support; that by regarding mutually sterile species as records of variation in reproductive systems, we are at work, so to speak, on the foundation of the matter; and that we are thus able to explain a number of general facts which do not

admit of being explained by any previous theory. It only remains to add that, if true, the present theory ought to admit of experimental verification. Let well-marked natural varieties of plants growing on the same area be systematically tested with regard to their relative *degrees* of fertility, first within themselves, and next towards one another: let these experiments be made in successive years over a number of natural varieties, by carefully-conducted artificial fertilisation, and by counting the seeds and tabulating the results. In this way experimental evidence would probably be obtained of degrees of sterility between even slight though constant varieties growing on the same areas; and, if so, such evidence would serve as further proof of the present theory. But experiments of this kind, in order to be satisfactory, ought to be conducted by a number of observers in different geographical areas; and my object in publishing so lengthy an abstract of my views in this periodical is that of inducing naturalists in other parts of the world to co-operate with me in carrying out this research. The paper itself, which furnishes fuller particulars as to the way in which such experiments should be carried out, is published in a separate form by the Linnean Society.

THE WOODEND COLLIERY EXPLOSION

QUI s'excuse s'accuse will occur to the minds of many who have followed the details of the disastrous explosion which took place at Woodend or Bedford Colliery on Friday last. We read in the *Times* of the 16th inst.:—"The Four-foot or Crombonke Mine is a very dusty one, and it is considered that at the Woodend pit the dust has increased the extent of the damage." "But to water the mine, as suggested by the Commission, would here be a very difficult operation, because the floor of the mine consists of a species of fire-clay which, as it absorbs the water, causes a lifting of the ground, and so prevents mining operations being conducted." Inasmuch, however, as the floor of perhaps ninety-nine out of every hundred mines consists of the same kind of material, the same argument against watering would hold equally good in most cases, and, if it is allowed to pass, this recommendation of the Commissioners is likely to come to nothing. It has been pointed out more than once in NATURE that the amount of water required to lay the dust is very small—far less than would be necessary to materially affect the floor of a mine in the manner suggested, and it would perhaps be wiser to try the effect in the first place and judge by results rather than to meet the proposition with a simple *non possumus*. We speak thus plainly here, because many of the witnesses who gave evidence before the Commissioners brought forward the very same argument with the same degree of plausibility, and we have reason to believe without having put the matter to a practical test. Many of those who now water regularly, for the express purpose of laying the dust on floors consisting of fire-clay, admit that the water produces no appreciable difference when properly and carefully distributed.

The bursting of the gauze of a safety-lamp, described by one of the survivors, is so contrary to all reason and experience that it cannot be accepted as an explanation of the origin of the explosion. Hundreds, if not thousands, of safety lamps are placed in explosive gas every day when the mines are being tested for the presence of fire-damp, and yet no parallel case has ever been recorded. Under these circumstances we prefer to attribute it to some other still unknown cause. We have yet to learn whether shots were fired in the mine, and if so we have probably not far to look for the explanation.

Up to the present all we know with certainty is that the mine produced very little gas, that it was dry and dusty, and that the explosion was violent but not universal. It would be most interesting, as well as instructive, to ascertain whether any natural local dampness curtailed its extent; but as this is a feature that has not hitherto attracted or received much attention, we are not sanguine that it will be carefully inquired into in the present case. We shall, however, watch the future course of the inquiry, and perhaps again comment upon it for the benefit of our readers.

W. G.

ON THE DIFFERENTIAL EQUATION TO A CURVE OF ANY ORDER

TO Mr. Samuel Roberts (see Reprint of *Educational Times*, vol. x. p. 47) is due the credit of having been the first to show that a direct method of elimination properly conducted leads to the differential equation for a cubic curve: but he has not attempted to obtain the general formula for a curve of any order. By aid of a very simple idea explained in a paper intended to appear in the *Comptes rendus* of the Institute, I find without calculation the general form of this equation. The eft-hand member of it may be conveniently termed the differential *criterion* to the curve. One single matrix will then serve to express the criteria for all curves whose order does not exceed any prescribed number. For instance, suppose we wish to have the criteria for the orders 1, 2, 3, 4:—

Let $m\,\mu$ be used in general to denote the coefficient of

$$h^m \text{ in } \left(\frac{1}{1.2}y''h^2 + \frac{1}{1.2.3}y'''h^3 + \frac{1}{1.2.3.4}y''''h^4 + \ldots\right)^m.$$

Write down the matrix—

$$\begin{array}{cccccccccc}
2\cdot1 & 3\cdot1 & 3\cdot2 & 4\cdot1 & 4\cdot2 & 4\cdot3 & 5\cdot1 & 5\cdot2 & 5\cdot3 & 5\cdot4 \\
3\cdot1 & 4\cdot1 & 4\cdot2 & 5\cdot1 & 5\cdot2 & 5\cdot3 & 6\cdot1 & 6\cdot2 & 6\cdot3 & 6\cdot4 \\
4\cdot1 & 5\cdot1 & 5\cdot2 & 6\cdot1 & 6\cdot2 & 6\cdot3 & 7\cdot1 & 7\cdot2 & 7\cdot3 & 7\cdot4 \\
5\cdot1 & 6\cdot1 & 6\cdot2 & 7\cdot1 & 7\cdot2 & 7\cdot3 & 8\cdot1 & 8\cdot2 & 8\cdot3 & 8\cdot4 \\
6\cdot1 & 7\cdot1 & 7\cdot2 & 8\cdot1 & 8\cdot2 & 8\cdot3 & 9\cdot1 & 9\cdot2 & 9\cdot3 & 9\cdot4 \\
7\cdot1 & 8\cdot1 & 8\cdot2 & 9\cdot1 & 9\cdot2 & 9\cdot3 & 10\cdot1 & 10\cdot2 & 10\cdot3 & 10\cdot4 \\
8\cdot1 & 9\cdot1 & 9\cdot2 & 10\cdot1 & 10\cdot2 & 10\cdot3 & 11\cdot1 & 11\cdot2 & 11\cdot3 & 11\cdot4 \\
9\cdot1 & 10\cdot1 & 10\cdot2 & 11\cdot1 & 11\cdot2 & 11\cdot3 & 12\cdot1 & 12\cdot2 & 12\cdot3 & 12\cdot4 \\
10\cdot1 & 11\cdot1 & 11\cdot2 & 12\cdot1 & 12\cdot2 & 12\cdot3 & 13\cdot1 & 13\cdot2 & 13\cdot3 & 13\cdot4 \\
11\cdot1 & 12\cdot1 & 12\cdot2 & 13\cdot1 & 13\cdot2 & 13\cdot3 & 14\cdot1 & 14\cdot2 & 14\cdot3 & 14\cdot4
\end{array}$$

The determinant of the entire matrix, which is of the tenth order, is the criterion for a quartic curve. The determinant of the minor of the sixth order, comprised within the first six lines and columns is the criterion for a cubic. The determinant of the third order, comprised within the first three lines and columns (subject to a remark about to be made) will furnish the criterion for a conic, and the apex of the matrix is the criterion for the straight line. By adding on five more lines and columns, according to an obvious law, the matrix may be extended so as to give the criterion for a quintic; then six more lines and columns a sextic, and so on as far as may be required.

The remark to be made concerning the determinant of the third order referred to is that it contains the irrelevant factor $2\cdot1$, *i.e.* $\frac{y''}{2}$, so that the criterion for a conic (Monge's) is this determinant divested of such factor. It is *certain* that the next determinant is indecomposable, and is therefore the criterion for a cubic. There is no reason that I know of to suppose that any other determinant except that one which corresponds to the conic, is decomposable into factors. If this is made out, then, observing that the single term which is the criterion for the right line is indecomposable, we have another example of what may be called, in Babbage's words, a miraculous exception to a general law.

A well-known similar case of such miraculous exception I had occasion many years ago to notice in connection with the criteria for determining the number of real and imaginary roots in an algebraical equation. Such criteria may, with one single exception, be expressed

by means of invariants. The case of exception is the biquadratic equation, for which it is impossible to assign an invariantive criterion that shall serve to distinguish between the case of all the roots being real and all imaginary.

It is proper to notice that it follows, from the definition of the symbol $m.\mu$, that its value is zero whenever m is less than 2μ. Thus, in the matrix written out above, the symbols 3·2, 4·3, 5·3, 5·4, 6·4, 7·4 may be replaced by zeros.

The above general result for a curve of any order is actually obtained by a far less expenditure of thought and labour than was employed by Monge, Halphen, and others to obtain it for the trifling case of a conic. I touch a secret spring, and the doors of the cabinet fly wide open.[1] J. J. SYLVESTER

New College, Oxford, August 6

CAPILLARY ATTRACTION[2]

III.

In these other diagrams, however (Figs. 13 to 28), we have certain portions of the curves taken to represent real capillary surfaces shown in section. In Fig. 13 a solid sphere is shown in four different positions in contact with a mercury surface; and again, in Fig. 14 we have a section of the form assumed by mercury resting in a circular V-groove. Figs. 15 to 28 show water-surfaces under different conditions as to capillarity; the scale of the drawings for each set of figures is shown by a line the length of

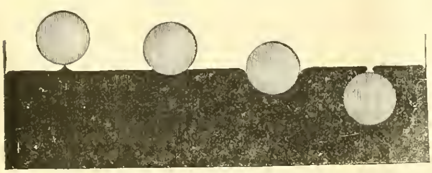


FIG. 13.—Mercury in contact with solid spheres (say of glass).

which represents one centimetre; the dotted horizontal lines indicate the positions of the free water-level. The drawings are sufficiently explicit to require no further reference here save the remark that *water* is represented by the lighter shading, and *solid* by the darker.

We have been thinking of our pieces of rigidified water as becoming suddenly liquified, and conceiving them inclosed within ideal contractile films; I have here an arrangement by which I can exhibit on an enlarged

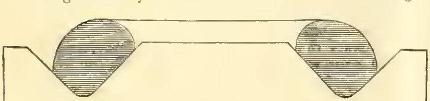
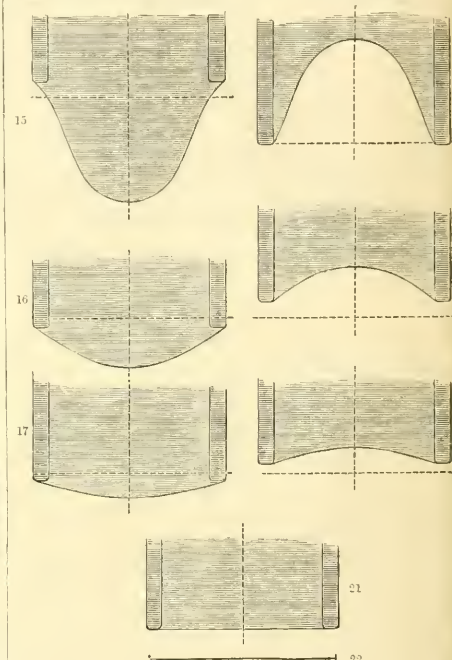


FIG. 14.—Sectional view of circular V-groove containing mercury.

scale a pendant drop, inclosed not in an *ideal* film, but in a *real* film of thin sheet india-rubber. The apparatus which you see here suspended from the roof is a stout metal ring of 60 centimetres diameter, with its aperture closed by a sheet of india-rubber tied to it all round, stretched uniformly in all directions, and as tightly as could be done without special apparatus for stretching it and binding it to the ring when stretched.

I now pour in water, and we find the flexible bottom assuming very much the same shape as the drop which you saw hanging from my finger after it had been dipped into and removed from the vessel of water (see Fig. 16).



FIGS. 15-21.—Water in glass tubes, the internal diameter of which may be found from Fig. 22, which represents a length of one centimetre.

I continue to pour in more water, and the form changes gradually and slowly, preserving meanwhile the general form of a drop such as is shown in Fig. 15, until, when a certain quantity of water has been poured in, a sudden change takes place. The sud-

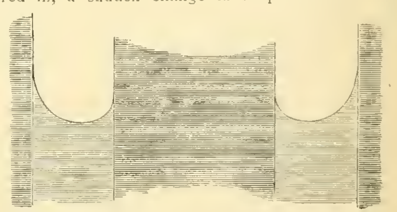


FIG. 23.—Water resting in the space between a solid cylinder and a concentric hollow cylinder.

den change corresponds to the breaking away of a real drop of water from, for example, the mouth of a tea-urn, when the stopcock is so nearly closed that a very slow dropping takes place. The drop in the india-rubber bag, however, does not fall away, because the tension of the india-rubber increases enormously when

[1] Adopting the convention for degree and weight of a differential coefficient usual in the theory of reciprocants the deg : weight of the differential criterion of the nth order will be easily found to be—

$$\frac{n\,.\,\overline{n+1}\,.\,\overline{n+2}}{6} : \frac{\overline{n-1}\,.\,n\,.\,\overline{n+1}\,.\,\overline{n+2}}{8}$$

except that for $n = 2$ it is 3 : 3 instead of 4 : 3.

[2] Lecture delivered at the Royal Institution. Revised and extended by the Author. Continued from p. 294.

the india-rubber is stretched. The tension of the real film at the surface of a drop of water remains constant, however much the surface is stretched, and therefore the drop breaks away instantly when enough of water has been supplied from above to feed the drop to the greatest volume that can hang from the particular size of tube which is used.

I now put this siphon into action, gradually drawing off some of the water, and we find the drop gradually diminishes until a sudden change again occurs and it assumes the form we observed (Fig. 16) when I first poured in the water. I instantly stop the action of the siphon, and we now find that the great drop has two possible forms of stable equilibrium, with an unstable form intermediate between them.

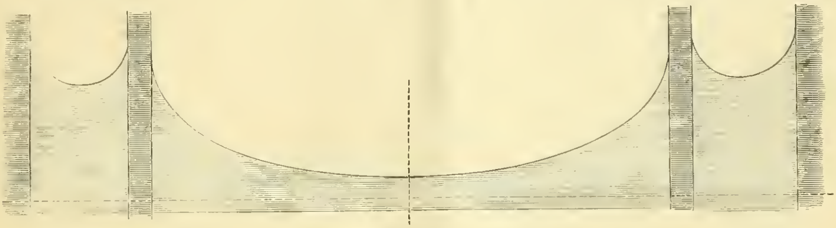


FIG. 24.—Water resting in two co-axial cylinders; scale is represented by Fig. 28.

Here is an experimental proof of this statement. With the drop in its higher stable form I cause it to vibrate so as alternately to decrease and increase the axial length, and you see that when the vibrations are such as to cause the increase of length to reach a certain limit there is a sudden change to the lower stable form, and we may

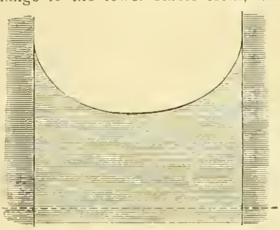


FIG. 25.

now leave the mass performing small vibrations about that lower form. I now increase these small vibrations, and we see that, whenever, in one of the upward (increasing) vibrations, the contraction of axial length reaches the limit already referred to, there is again a sudden change, which I promote by gently lifting with my hands, and the mass assumes the higher stable form, and we have it again performing small vibrations about this form.

The two positions of stable equilibrium, and the one of unstable intermediate between them, is a curious peculiarity of the hydrostatic problem presented by the water supported by india-rubber in the manner of the experiment.

I have here a simple arrangement of apparatus (Figs. 29 and 30) by which, with proper optical aids, such as a cathetometer and a microscope, we can make the necessary measurements on real drops of water or other liquid, for the purpose of determining the values of the capillary constants. For stability the drop hanging from the open tube should be just less than a hemisphere, but for convenience it is shown, as in the enlarged drawing of the nozzle (Fig. 30), exactly hemispherical. By means of the siphon the difference of levels, h, between the free level surface of the water in the vessel to which the nozzle is attached, and the lowest point in the drop hanging from the nozzle, may be varied, and corresponding measurements taken of h and of r, the radius of curvature of the drop at its lowest point. This measurement of the curvature of the drop is easily made with somewhat close accuracy, by known microscopic methods. The surface-tension T of the liquid is calculated from the radius, r, and the observed difference of levels, h, as follows:—

$$\frac{2T}{r} = h;$$

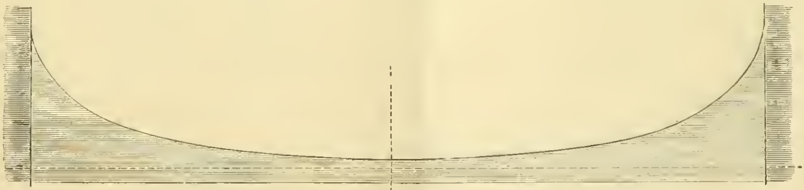


FIG. 26.

FIGS. 25 and 26.—Water resting in hollow cylinders (tubes); scale is represented by Fig. 28.

for example, if the liquid taken be water, with a free-surface tension of 75 milligrammes per centimetre, and $r = \cdot 05$ cm., h is equal to 3 centimetres.

Many experiments may be devised to illustrate the effect of surface-tension when two liquids, of which the surface-tensions are widely different, are brought into contact with each other. Thus we may place on the surface of a thin layer of water, wetting uniformly the surface of a glass plate or tray, a drop of alcohol or ether, and so cause the surface-tension of the liquid layer to become smaller in the region covered by the alcohol or ether. On the other hand, from a surface-layer of alco-

hol largely diluted with water we may arrange to withdraw part of the alcohol at one particular place by promoting its rapid evaporation, and thereby increase the surface-tension of the liquid layer in that region by diminishing the percentage of alcohol which it contains.

In this shallow tray, the bottom of which is of ground glass resting on white paper, so as to make the phenomena to be exhibited more easily visible, there is a thin layer of water coloured deep blue with aniline: now, when I place on the water-surface a small quantity of alcohol from this fine pipette, observe the effect of bringing the alcohol-surface, with a surface-tension of only 25·5 dynes per lineal centimetre, into contact with the water-surface, which has a tension of 75 dynes per lineal centimetre. See how the water pulls back, as it were, all round the alcohol, forming a circular ridge surrounding a hollow, or small crater, which gradually

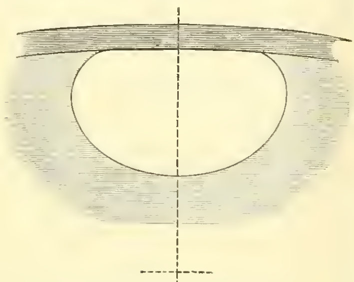


FIG. 27.—Section of the air-bubble in a level tube filled with water, and bent so that its axis is part of a circle of large radius; scale is represented in Fig. 28.

widens and deepens until the glass plate is actually laid bare in the centre, and the liquid is heaped up in a circular ridge around it. Similarly, when I paint with a brush a streak of alcohol across the tray, we find the water drawing back on each side from the portion of the tray touched with the brush. Now, when I incline the glass tray, it is most interesting to observe how the coloured water with its slight admixture of alcohol flows down the incline—first in isolated drops, afterwards joining together into narrow continuous streams.

These and other well-known phenomena, including that interesting one, "tears of strong wine," were described and explained in a paper "On Certain Curious Motions Observable on the Surfaces of Wine and other Alcoholic Liquors," by my brother, Prof. James Thomson, read before Section A of the British Association at the Glasgow meeting of 1855.

FIG. 28.—Represents a length of one centimetre for Figs 24 to 27.

I find that a solution containing about 25 per cent. of alcohol shows the "tears" readily and well, but that they cannot at all be produced if the percentage of alcohol is considerably smaller or considerably greater than 25. In two of those bottles the coloured solution contains respectively 1 per cent. and 90 per cent. of alcohol, and in them you see it is impossible to produce the "tears"; but when I take this third bottle, in which the coloured liquid contains 25 per cent. of alcohol, and operate upon it, you see—there—the "tears" begin to form at once. I first incline and rotate the bottle so as to wet its inner surface with the liquid, and then, leaving it quite still, I remove the stopper, and withdraw by means of this paper tube the mixture of air and alcoholic vapour from the bottle and allow fresh air to take its place. In this way I promote the evaporation of alcohol from all liquid surfaces within the bottle, and where the liquid is in the form of a thin film it very speedily loses a great part of its alcohol. Hence the surface-tension of the thin film of liquid on the interior wall of the bottle comes to have a greater and greater value than the surface-tension of the mass of liquid in the bottom, and where these two liquid surfaces, having different surface-tensions, come together we have the phenomena of "tears." There, as I hasten the evaporation, you see the horizontal ring rising up the side of the

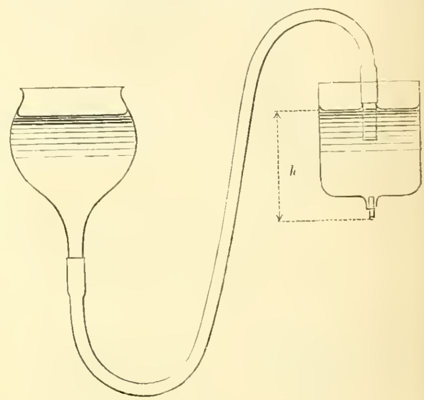


FIG. 29.

bottle, and afterwards collecting into drops which slip down the side and give a fringe-like appearance to the space through which the rising ring has passed.

These phenomena may also be observed by using, instead of alcohol, ether, which has a surface-tension equal to about three-fourths of that of alcohol. In using ether, however, this very curious effect may be seen.[1] I dip the brush into the ether, and hold it near to but not touching the water-surface. Now I see a hollow formed, which becomes more or less deep according as the brush is

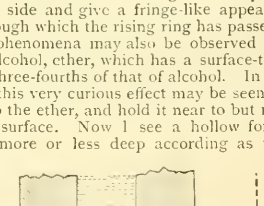


FIG. 30.

nearer to or farther from the normal water surface, and it follows the brush about as I move it so.

Here is an experiment showing the effect of heat on surface-tension. Over a portion of this tin plate there is a thin layer of resin. I lay the tin plate on this hot copper cylinder, and we at once see the fluid resin drawing back from the portion of the tin plate directly over the end of the heated copper cylinder, and leaving a

[1] See Clerk-Maxwell's article (p. 65) on "Capillary Attraction" ("Encyclopædia Britannica," 9th edition).

circular space on the surface of the tin plate almost clear of resin, showing how very much the surface-tension of hot resin is less than that of cold resin.

Note of January 30, 1886.—The equations (8) and (9) on p. 59 of Clerk-Maxwell's article on "Capillary Attraction" in the ninth edition of the "Encyclopædia Britannica" do not contain terms depending on the mutual action between the two liquids, and the concluding expression (10), and the last small print paragraph of the page are wholly vitiated by this omission. The paragraph immediately following equation (10) is as follows:—

"If this quantity is positive, the surface of contact will tend to contract, and the liquids will remain distinct. If, however, it were negative, the displacement of the liquids which tends to enlarge the surface of contact would be aided by the molecular forces, so that the liquids, if not kept separate by gravity, would become thoroughly mixed. No instance, however, of a phenomenon of this kind has been discovered, for those liquids which mix of themselves do so by the process of diffusion, which is a molecular motion, and not by the spontaneous puckering and replication of the boundary surface as would be the case if T were negative."

It seems to me that this view is not correct; but that on the contrary there is this "puckering" as the *very beginning* of diffusion. What I have given in the lecture as reported in the text above seems to me the right view of the case as regards diffusion in relation to interfacial tension.

It may also be remarked that Clerk-Maxwell, in the large print paragraph of p. 59, preceding equation (1), and in his application of the term potential energy to E in the small print, designated by *energy* what is in reality exhaustion of energy or negative energy; and the same inadvertence renders the small print paragraph on p. 60 very obscure. The curious and interesting statement at the top of the second column of p. 63, regarding a drop of carbon disulphide in contact with a drop of water in a capillary tube would constitute a perpetual motion if it were true for a tube not first wetted with water through part of its bore—". . . if a drop of water and a drop of bisulphide of carbon be placed in contact in a horizontal capillary tube, the bisulphide of carbon will chase the water along the tube."

Additional Note of June 5, 1886.—I have carefully tried the experiment referred to in the preceding sentence, and have not found the alleged motion.

WILLIAM THOMSON

OUR FOSSIL PSEUDO-ALGÆ

DURING the last half-century many palæontologists have described anomalous objects, some of which have been regarded as fossil marine Algæ, and others as tracks of various marine invertebrate animals; and since the publication of Darwin's theory of evolution various attempts have been made to utilise some of these in formulating a pedigree for the living types of vegetation. Amongst those who have tried to accomplish this object my distinguished friend the Marquis of Saporta, and his colleague, M. Marion, occupy the most prominent position. They have in several publications described and figured many objects which they believe to have been true marine Algæ, and out of which they have constructed the lower roots of their genealogical tree. But meanwhile there has grown up an enlarging school of palæontologists who look with strong suspicion upon these genealogies; who refuse to recognise the vegetable character of these objects; who believe most of them to be casts of various ridges and furrows, most of which have been tracks produced by creeping invertebrate animals or by the still more mechanical agencies of wind and water. At the head of this school Prof. Nathorst, of Stockholm, stands pre-eminent. An animated controversy sprang up some time ago between M. Nathorst and M. Saporta relative to this subject. Blast and counterblast have succeeded one another, and the latest discharge of palæo-botanical artillery has just been fired off by M. Nathorst in the form of a memoir entitled "Nouvelles Observations sur des Traces d'Animaux et autres Phénomènes d'Origine purement méchanique décrits comme 'Algues Fossiles.'"

Enjoying the privilege of an intimate and valued friendship with both these distinguished palæontologists, I am anxious to do full justice to both. But I must admit that my judgment and experiences bring me into closer agreement with the northern naturalist than with his French antagonist. The interesting subject discussed by them has long occupied my attention, and my conclusions respecting it have not been hastily arrived at.

The question in debate is not whether or not marine Algæ existed in Palæozoic and later geological epochs: on this point Nathorst and Saporta are agreed. The abundance of phytophagous marine mollusks found even in the Cambrian, as in most of the other fossiliferous strata, clearly demonstrates that there must have been submarine pastures upon which they could feed. The question is, are numerous objects, found in strata of marine origin, and believed by some to be fossilised marine Algæ, really such? To this query Saporta answers *Yes*; Nathorst's reply is an emphatic *No*. Hence the elaborate controversial literature of which these two *savants* are the authors. To condense their several articles into an abstract is not easy, but such an abstract of M. Nathorst's latest publication may be attempted, illustrating the general features of the discussion.

Throughout his memoir M. Nathorst rests prominently upon two general propositions which appear to me to be unanswerable. The first is that all or nearly all these debatable pseudo-Algæ stand out in bold demi-relief from the *inferior* surfaces of the rocky slabs to which they are attached, and that beyond their sculptured surfaces they as constantly consist of a mere extension of the rock of which they form a part. Hence Nathorst insists that they are merely convex casts of what were primarily concave grooves or channels on the surface of the subjacent stratum.

In reply to this opinion M. Saporta publishes figures of casts of vegetable fragments in demi-relief, the positions of which on the inferior surfaces of slabs are identical with those of the pseudo-Algæ under discussion. One of these is a fragment of what appears to be a twig of a Conifer, of which the lower side alone is preserved in demi-relief. Nathorst freely admits the possible existence of such specimens, but he regrets Saporta's explanation of them. *Imprimis*, he affirms with inexorable logic, that such examples are so rare and exceptional that they only prove the opposite of the rule which they are alleged by Saporta to sustain. Whenever fragments like these are found embedded in the rocks, they almost invariably display traces of both their upper and lower surfaces; whereas this is scarcely ever the case with the disputed Fucoids, and in the very few instances where such are supposed to have been met with, their entirely exceptional character suggests a very different explanation of them from that proposed by M. Saporta.

It is difficult to understand how a cylindrical object sufficiently dense to produce a deep concave impression upon hardening mud could do so without leaving some trace of its upper surface upon the opposite surface of the sand by which that mud became overlain. Saporta's theory explaining why it does not do so is surely untenable. That theory supposes that an organism half embedded in mud and overlain by sand began to decay at its *upper* surface, which decay ultimately reached the lower surface which rested on the mud; that, as the decay proceeded downwards, the superimposed sand would finally reach the concave mould in the mud which it would fill, and

of which it would permanently preserve the form. Nathorst replies that the decay would follow no such downward course; that it would commence in the softest tissues, wherever they were; and that in such objects as Bilobites and Cruziana, which Saporta believed to be cellular and fistular objects, whilst their outer surfaces were sufficiently hard and resisting to impress their sculpturings upon the underlying mud, the decay would commence in their interior, with the result, in each instance, not of destroying all traces of the upper surface whilst the lower one was preserved, but that both surfaces of the flattened organism would be preserved, as is the case with the Carboniferous Sigillaria and Lepidodendra. Once thus flattened, the organism would no longer be capable of producing the deeply concave groove in the clay to which the specimens in bold demi-relief have been due.

This part of the controversy furnishes Nathorst with another argument. However much compressed, embedded fragments of vegetable matter almost invariably retain some traces of their primitive carbonaceous constituents, however thin the film thus preserved may be. As in the case of many of the Monte Bolca Fucoids, this may be no more than a faint brownish stain on the surface of the stone; whereas such stains, suggestive of the former permanent presence of organic matter, are almost invariably, if not wholly, absent from the pseudo-Fucoids.

In his new memoir M. Nathorst then proceeds to examine in detail the claims of several well-known genera to rank as members of the vegetable kingdom. I must refer such as are interested in the details of this controversy to the original memoir of the Swedish palæontologist. I would only observe that, whilst M. Nathorst denies the accuracy of some of M. Saporta's statements as to the facts in certain instances, in others on which the two palæontologists are agreed he shows that the acknowledged facts are capable of such explanations as lead to conclusions diametrically opposed to those arrived at by M. Saporta.

One of the most important features of M. Nathorst's new memoir is seen in his illustrations. He has invented some simple instruments, by rolling which over some plastic materials he produces impressions, casts of which recall most strikingly the objects known by the generic titles of Cruziana and Harlania.

Whatever ultimate decisions may be arrived at respecting these debated objects, I am compelled to arrive at a conclusion which I have already announced on more than one previous occasion. When it is possible for two observers so experienced as are M. Nathorst and M. Saporta to study the same objects and to arrive at such opposite determinations as to their organic or inorganic character, we must at least conclude that objects capable of receiving such contradictory explanations can have no value when we are considering the evolution of the vegetable kingdom. The evidences of the witnesses in such a controversy must be clear in their testimony and indisputable as to their antecedents.

Manchester, August 11 W. C. WILLIAMSON

NOTES

A TELEGRAM from Grenada, August 16, states that the British observing party for the eclipse of the sun on the 29th inst. has arrived there, and has divided into two. Messrs. Lockyer, Turner, Perry, and Maunder are going to Green Island and Grenville Bay, on the east side of Grenada, and to Carriacou, a small island to the north. Messrs. Darwin, Thorpe, Schuster, and Lawrence will take up their station at Prickly Point, Hog Island.

THE seventeenth meeting of the German Anthropological Society was formally opened on the 11th inst. at Stettin. The gathering is described as a very representative and distinguished one.

THE Yorkshire Naturalists' Union fungus foray will take place on Thursday, September 30. On the following day there will be an exhibition of the specimens in the Leeds Museum, kindly lent for the purpose; and in the evening the usual dinner. Several distinguished mycologists have promised to be present, and no effort is being spared on the part of the officials to make it a success.

LORD DUFFERIN is, it is stated, about to address a memorandum to the Provincial Governments of India regarding technical education, pointing out where the present system fails, suggesting remedies, advising the adoption of a more practical system, and inviting opinions from the Provincial Governments on the whole subject.

WE have received the Smithsonian reports on the progress of physics and mineralogy for the past year. The former is by Prof. Barker, the latter by Prof. E. S. Dana. Physics is treated under the heads general, mechanics (with the subtitles solids, liquids, gases), acoustics, heat (sub-titles production of heat, expansion and change of state, conduction and radiation, specific heat), light (production and velocity, reflection and refraction, dispersion and colour, interference and polarisation), electricity (magnetism, electric generators, electrical units and measurements, electric spark and electric light), obituary, and bibliography. Mineralogy, similarly, is treated under the heads general, crystallography and physical mineralogy, chemical mineralogy, new mineral localities, new minerals, papers on mineral species, bibliography, and obituary.

THE Smithsonian Report for the past year contains a most interesting paper on the "Volcanic Eruptions and Earthquakes in Iceland within Historic Times," translated and condensed from the work of Th. Thoroddsen, by Mr. George Boehmer. The original work appears to be one of enormous research and thoroughness. Mr. Boehmer divides his subject into early accounts, position of the active volcanoes, of which there are eight groups, with a sketch of each, chronological list of volcanic eruptions and earthquakes in Iceland, and finally an extensive bibliography of the volcanoes, earthquakes, and geysers of Iceland.

THREE severe shocks of earthquake occurred at Malta, the first at 8.30 p.m. on August 14, the second at 3.45 a.m. on August 15, and the third at noonday. Three fresh shocks were felt on the evening of the 17th, the first at 5.45, the second at 6.20, and the third at 7.45. They were not so violent as those experienced on Saturday and Sunday, and no damage is reported.

MR. H. B. GUPPY is completing his work on the Solomon Islands and their natives, which will shortly be ready for the press. The work will refer chiefly to the anthropology and geology of this region. It will also treat of the botany, natural history, meteorology, and general resources of these islands, and there will be appended an account of the original discovery of the group as related in the British Museum manuscript of Gallego's "Journal." The chief value of Mr. Guppy's observations will lie in the circumstance that his collections have been examined by the leading authorities on the subjects to which they relate. He hopes to illustrate the work from his own photographs.

WITH reference to Mr. Verbeek's investigations into the Krakatão eruption, which were noticed in NATURE, vol. xxxiii. p. 560, we have received a letter from Herr Retgers, Mining Engineer in Samarang, stating (as indeed Mr. Verbeek had already stated in his work) that the whole of the mineralogical investigations into the composition of the volcanic ashes then thrown out were carried out by him.

The Committee of the Liverpool Naturalists' Field Club begin their report for the past year by observing that for twenty-five years past statistics have been exhausted and lectures also, so far as their usefulness is concerned. "Your Committee, therefore, on this occasion, will spare you figures and forbear admonition, contenting themselves with a bare record of the year's proceedings, and leaving members to draw their own conclusions as to what has been and what might have been done." With such a lugubrious commencement, one might expect that the affairs of the Club were in a "parlous" state, but this is far from being the case. One complaint is that the attendance at the excursions was not satisfactory, and therefore either the number will have to be cut down, or the distances travelled be less. The average attendance was about 60,—which many similar societies would consider an uncommonly good one, especially when it is remembered that some of the journeys were rather long. The Treasurer has the handsome balance of nearly 90*l.* in his hands; the President, the Rev. H. Higgins, delivered an excellent address on "Calcareous Sea-Weeds: an Essay in Comparative Phytology;" the lists of interesting plants noticed on some of the excursions show that the members who did go kept their eyes open; the competition for the prizes appears to have been pretty keen; and there is a tolerably long list of members,—so that, on the whole, notwithstanding the low spirits of the Committee, the case of the Society is far from hopeless. But we trust the members will attend in larger numbers when the Society next goes to the Cefn Caves, Caergwrle, or Humphry Head, notwithstanding the long distances. They will thereby not only add to their own enjoyment and instruction, but will bring relief to the minds of their depressed and anxious Committee.

The greatest balloon in the world has been lately constructed at San Francisco by a Mr. van Tassel. It will hold 150,000 cubic feet of gas, and has been made for the purpose of traversing the American Continent from ocean to ocean. From the bottom of the car to the top of the inflated balloon will be 119 feet, and when filled the diameter will be 68 feet. The car is 21 feet in circumference and has sides 34 inches high; 15 persons can be seated in it.

For several years attempts have been made in Sweden to extract tannic matter from the Swedish species of pine, similar in quality, &c., to that of the American hemlock (*Pinus canadensis*), but without satisfactory results, chiefly on account of the manner in which this is done not being known. Now, however, the question has been solved by a chemist, Dr. Laudin, who, having visited North America for this purpose, has, on his return to Sweden, succeeded in producing tannic matter by a chemical process, which has been found equal to the American, though the colour of the Swedish leather produced therewith is more yellow in colour than the American. It is hoped that this discovery will have the effect of causing a great tanning industry to spring up in Sweden.

The additions to the Zoological Society's Gardens during the past week include a Guinea Baboon (*Cynocephalus sphinx*), from West Africa, presented by Mr. C. Palgrave, F.Z.S.; an Alpine Marmot (*Arctomys marmotta*), two Tawny Owls (*Syrnium aluco*), European, presented by Mr. Lionel H. Hanbury, F.Z.S.; a Bank Vole (*Arvicola pratensis*), British, presented by Mr. G. T. Rope; two Derbian Screamers (*Chauna derbiana*) from the North Coast of Columbia, presented by Capt. H. Rigaud; a Peregrine Falcon (*Falco peregrinus*), European, presented by Mr. J. Howard; a Golden-crowned Conure (*Conurus aureus*) from South-East Brazil, deposited; three Long-fronted Gerbilles (*Gerbillus longifrons*), eight Elliot's Pheasants (*Phasianus ellioti*), bred in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *AUGUST* 22-28

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on August 22

Sun rises, 4h. 58m.; souths, 12h. 2m. 42·2s.; sets, 19h. 7m.; decl. on meridian, 11° 44′ N.: Sidereal Time at Sunset, 17h. 11m.

Moon (at Last Quarter) rises, 22h. 6m.*; souths, 5h. 21m.; sets, 12h. 46m.; decl. on meridian, 13° 18′ N.

Planet	Rises	Souths	Sets	Decl. on meridian
	h. m.	h. m.	h. m.	° ′
Mercury ...	... 4 13	... 11 19	... 18 25	... 12 1 N.
Venus ...	... 2 28	... 10 19	... 18 10	... 19 43 N.
Mars ...	... 10 47	... 15 40	... 20 51	... 12 3 S.
Jupiter ...	... 8 25	... 14 22	... 20 19	... 1 27 S.
Saturn ..	... 1 11	... 9 16	... 17 21	... 21 53 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich)

Aug.	Star	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image
			h. m.	h. m.	° °
23 ...	48 Tauri ...	... 6	... 1 23	... 1 54	.. 126 186
23 ...	γ Tauri ...	... 4	... 3 22	... 4 13	... 118 215
23 ...	58 Tauri ...	... 6	... 4 1	near approach	348 —

Aug.	h.	
22 ...	12 ...	Jupiter at greatest distance from the Sun.
25 ...	11 ...	Mercury stationary.
27 ...	20 ...	Venus in conjunction with and 3° 0′ north of the Moon.

Variable Stars

Star	R.A.	Decl.		
	h. m.	° ′		h. m.
U Cephei	0 52·2	... 81 16 N.	... Aug. 22,	21 8 *m*
			,, 27,	20 47 *m*
Algol	3 0·8	... 40 31 N.	... ,, 22,	21 7 *m*
V Tauri	4 45·4	... 17 21 N.	... ,, 27,	*M*
W Virginis	13 20 2	... 2 47 S.	... ,, 26,	0 0 *m*
U Coronæ	15 13·6	... 32 4 N.	... ,, 22,	0 4 *m*
			,, 28,	21 46 *m*
U Herculis	16 20·8	... 19 9 N.	... ,, 24,	*M*
R Draconis	16 32·4	... 67 3 N.	... ,, 26,	*M*
U Ophiuchi	17 10·8	... 1 20 N.	... ,, 22,	23 50 *m*
			and at intervals of	20 8
β Lyræ	18 45·9	... 33 14 N.	... Aug. 24,	2 0 *M*
R Lyræ	18 51·9	... 43 48 N.	... ,, 28,	*M*
δ Cephei	22 24·9	... 57 50 N.	... ,, 27,	2 0 *M*

M signifies maximum; *m* minimum.

Meteor Showers

Meteors have been observed at this time of the year from near κ Ceti, R.A. 53°, Decl. 0; near Castor, R.A. 110°, Decl. 32° N.; near ζ Draconis, R.A. 260°, Decl. 64° N.; and from near ο Draconis, R.A. 282°, Decl. 57° N.

GEOGRAPHICAL NOTES

We have before us Nos. 5, 6, 7, and 8 of *Petermann's Mittheilungen* for the present year, and Supplement No. 82. The last is a detailed account, by the late Herr Robert Schlagintweit, of the Pacific railways of North America. No. 5 contains a paper on the Xingu Expedition (concluded in No. 6), by Herr Claus, detailing the cartographical surveys and the physical and astronomical measurements made in the course of the explorations. The paper may be regarded as a supplement to the work of Dr. von den Steinen, "Durch Zentralbrasilien," lately published by Brockhaus. Dr. Oppel, in the same number, contributes a statistical paper showing the steady and enormous increase in the population of Europe. No. 7 contains two very interesting and original geographical papers—one by Herr Engelhard on the Island of Saleijer, a Dutch settlement in the Malay Archipelago, situated immediately south of Celebes. The island is described in an exhaustive way, its climate, people, situation, &c., being discussed in some detail. In the second

Dr. Posewitz refers to recent formations in the Island of Banka, off the east coast of Sumatra. This is an instance of an island, undisturbed by volcanic activity, in which erosion and denudation are constantly at work forming the coast. Herr Strass has a paper in the same number dealing with the statistics of emigration from Germany between 1871 and 1884. No. 8, which is the last published, contains a report by Herr Pohle on the expedition sent in 1884 to that part of the coast of South-Western Africa between the Orange River and Walfisch Bay, which came at that time into the possession of Herr Lüderitz. The expedition was intended mainly to ascertain what useful minerals existed in the new territory, and also to study its fauna, flora, and soil. The report is one of considerable length, and deals with all these points. The paper on the forests of North America is based on Prof. Sargent's report, contained in the ninth volume of the United States Census Report for 1884.

THE *Proceedings* of the Royal Geographical Society for August contain several papers of interest. In "Recent Portuguese Explorations in the Zambezi Region" two journeys are described—one between the Zambezi and Pungué, the other between Tete on the Zambezi and Makanga. Mr. J. W. Wells, in a short paper, contributes some information on the delta of the Tocantins, in Brazil, and there is also a summary (the first, we believe, that has appeared in English) of the Von den Steinen exploration of the Xingu. The results of this expedition confirm the conclusion with regard to the geology of the interior of Brazil arrived at by the late Prof. Hartt, and by Mr. Wells, that south of the Amazon valley the whole interior of Brazil was at one time an immense plateau, and that the changes which it has undergone are due to water denudation. But the most important communication in this month's *Proceedings* is the report of the measures adopted by the Council of the Society for the improvement of geographical education. These are of two classes, to be carried out with the co-operation or assistance of the two Universities and the Education Office respectively. Under the first head the Council offer to appoint and pay a lecturer or reader in geography to deliver courses of lectures at both Universities, arranged so as to suit students in the Honour Schools; or, in the alternative, to join with both Universities in appointing and paying a reader in geography. In addition, the Council offers to contribute the funds for an exhibition. In connection with the Education Office, the Council offers various prizes in money and books to pupil teachers. Further, a donation of 30*l.* for the present year is made in aid of the geographical lectures in the University extension courses; copies of the *Proceedings* are to be sent to various public school libraries, and travellers and geographers are to be put in communication with the head masters of public schools. The proposal for a readership in geography at the Universities is obviously the most important of these, and the result of the communications now passing between the President and the Vice-Chancellors will be awaited with much interest.

THE last number (Bd. xxix. No. 4) of the *Mittheilungen* of the Geographical Society of Vienna has for its first article a discussion, by Prof. Penck, of the proportion of the areas of land and water on the surface of the globe. The writer gives at the outset an interesting sketch of the history of the subject, and of the various theories which have prevailed from time to time on the subject, beginning with Columbus, who thought the proportion of land to water was as 6 to 1. Starting from the generally accepted proportion of Wagner as that of 1 to 2·76, Prof. Penck advances various reasons for believing this to be unreliable, especially our ignorance of the regions around the North and South Poles. The blanks on our maps are still too numerous and important to permit of any reasonable approach to accuracy being made. Dr. Paulitschke writes on the hydrography of the Upper Webi, one of the two hydrographic problems of the Somâli peninsula, the other being the Juba, which Capt. Cecchi calls *complicata e misteriosa*. The writer appears to throw much light on the first from his own explorations. He thinks we must seek the source of the Webi in one of the lakes of Gurage. Prof. Blumentritt criticises that part of Dr. Montano's recent work on the Philippines which deals with the ethnology of Mindanao. Accepting for his present purpose (though he refuses to do so as a general proposition) Montano's division of the inhabitants into three main heads—Negritos, Indonesians, and Malays—he advances various reasons for holding that that writer does not arrange the tribes of Mindanao accurately under these heads. These are based chiefly on the languages; but if it does nothing else, the paper demonstrates the wide knowledge which Prof. Blumentritt possesses of these regions. Indeed, for years past he has made every department of research connected with the Philippines his own, until now he is without a living rival.

A REPORT has been received at the Hydrographic Department of the Admiralty from Commander Moore, of the surveying-vessel *Rambler*, relative to the existence of an island lying between L'Echiquier group and Durour Island, recently discovered by Mr. Allison, commanding the British steamer *Fei Lung*, when on the passage from Sydney to Shanghai. This island is covered with trees, and appeared to be 2 or 3 miles long in a north-west and south-east direction, and 100 to 150 feet in height. Both Durour Island and this were visible at the same time from the *Fei Lung* when passing between them. The approximate position as reported is lat. 1° 25′ S., long. 143° 26′ E. The Hydrographer to the Admiralty says that, unless the positions of the islands already on the chart are more inaccurate than they are believed to be, there is little doubt that the island now reported is a new discovery.

THE August number of the *Scottish Geographical Magazine* contains a translation of Col. Fontana's lecture on the Patagonian Andes to the Argentine Geographical Institute, describing his recent journey from Chubut to the slopes of the Andes. The latter are mentioned with great enthusiasm: they teem with fertility, and Nature is as exuberant there as farther north in the Gran Chaco. Finally, the sub-Andean portions of Patagonia are described as the country of the future, being another added to the long list of countries of the future.

IN tome x. fascicule 5, of the *Bulletin* of the Geographical Society of Antwerp, M. van den Gheyn discusses the question whether there is unity, duality, or plurality of races in Australia, and comes to a conclusion in favour of unity. He thinks that the differences found amongst them are to be explained by mixture with the Indonesians on the one side and the Polynesians on the other.

THE French Minister of Public Instruction has intrusted M. Alfred Marche with a mission to the Marianne Islands to study the geography, natural history, anthropology, and ethnography of the Archipelago.

THE AUGUST PERSEIDS

THE shower of Perseids has been a fairly conspicuous one this year notwithstanding the somewhat unfavourable circumstances attending the display. On the nights of August 9, 10, and 11 the nearly-full moon was visible during the greater part of the time available for observation, and robbed the phenomenon of its chief prominence during the evening hours. Those, however, who continued to watch the heavens until after the moon set on the early morning of the 11th must have been rewarded by a tolerably rich exhibition of meteors. The number observable by one person fell little short of 100 per hour, and this rate compared with similar observations in past years proves the late display to have fully maintained its decided character. Numerically this shower of Perseids cannot be placed in the same category as the brilliant meteoric storms of November 13, 1866, and November 27, 1872 and 1885, but it must be remembered that the August shower is one which returns *annually*, and apparently without much variation in its leading features. Its frequent and regular appearances compensate for whatever it lacks in other respects, and it yields many fine meteors of the same type as the Leonids, flashing out with remarkable swiftness, and projecting lines of phosphorescence upon the background of the sky.

The importance of watching every recurrence of the leading meteor showers is acknowledged on all hands, for if we would successfully trace out the modern history and developments of these wonderful systems we must first carefully secure the materials to form the basis of such investigations.

With reference to the shower of Perseids this year, the observations were much interrupted by cloudy weather. Preparation had been made here to commence a look-out during the last week in July for *avant-couriers* of the stream, but the nights were persistently overcast, and it was not until August 2 that a good view was obtained. Clouds were, it is true, somewhat prevalent before midnight, but afterwards the firmament became very clear, and it remained uniformly serene until daybreak.

Watching the eastern sky between 10h. and $14\frac{1}{2}$h., I counted 50 meteors, and of these 12 were Perseids displaying the normal features. The radiant-point was, at $33^\circ + 55^\circ$, not very sharply defined. Some of the best observed paths were slightly discordant, and gave the impression that the focus of divergence was diffused over an area of 3° or 4° diameter.

The following night was cloudy, but August 4 came in very clear, and 58 meteors were seen between 10h. and $14\frac{3}{4}$h. Amongst these were 12 Perseids, and the radiant-point, more contracted and definite than on the 2nd, was now at $37^\circ + 57^\circ$, having increased 4° in R.A. in the interim of 48 hours. The shower exhibited no increase in numbers between the 2nd and 4th; indeed, there appeared to have occurred a slight falling off on the latter date. But on the 4th I saw a duplicate shower of Perseids, the companion radiant being at $48^\circ + 43^\circ$, between α and β Persei, and this position was accurately indicated from seven paths.

A cloudy period supervened between the 4th and 10th, but on the latter night the sky was very clear throughout, though the moon was up until 13h. 30m. Between 10h. and $14\frac{1}{4}$h. I observed 152 meteors, though the watch was not persistent during that interval. The number seen included 122 Perseids with a radiant at $44^\circ + 57\frac{1}{2}^\circ$. At 13h. 6 meteors were noted within 20 seconds, and after the moon had fallen below the horizon the shower developed into one of considerable activity. Between 14h. and 14h. 15m. I counted 22 meteors, so that they were coming at the rate of about 90 per hour for one observer. Some of them were unusually bright. At 13h. 34m. a splendid Perseid appeared in the northern sky, pursuing a path of 13° from $77^\circ + 67^\circ$ to $111^\circ + 67^\circ$. It lit up the whole heavens with a momentary flash, and left a luminous streak, near the end of its path, that remained visible to the eye for nearly 4 minutes. This was by far the most conspicuous meteor seen during the night, and it will probably have been recorded at many other places.

On August 11 the sky was partly clear between $10\frac{3}{4}$h. and $11\frac{1}{4}$h., and 22 meteors were noted, including 15 Perseids from $47^\circ + 57\frac{1}{2}^\circ$. Thus the position of the radiant showed a still further displacement towards the east. The shower had declined greatly since the preceding night, and offered little attraction in the presence of the bright moonlight.

The shifting radiant of the Perseids forms one of the most curious and important details of its display. I first mentioned this feature in NATURE, vol. xvi. p. 362, and have been much interested in reobserving it on many subsequent occasions. Comparing the four positions determined this year, and one obtained on August 13, 1885 (NATURE, vol. xxxii. p. 415), the character of the displacement is well shown, and corroborates the figures given in the *Monthly Notices*, December 1884, pp. 97–8:—

		° °	
1886, August	2	33 + 55	12 meteors
	4	37 + 57	12 ,,
	10	44 + $57\frac{1}{2}$	122 ,,
	11	47 + $57\frac{1}{2}$	15 ,,
	13	51 + 58	6 ,,

On the whole the recent shower may be justly regarded as one fully answering to expectation. It has been quite equal, if indeed it has not surpassed, the Perseid displays as I observed them in 1869, 1871, 1874, 1876, 1877, 1878, and 1880. It is, however, somewhat difficult to institute perfectly fair comparisons. The circumstances affecting two displays are rarely, if ever identical. In some years the shower escapes suitable observation owing to cloudy weather just at the important time. In others moonlight nearly obliterates it. We must also consider that, as the main richness of the stream is limited to a short interval, it will occasionally elude us by occurring in daylight. These varying conditions and hindrances render it unsafe to draw conclusions as to the relative aspect of the annual displays unless the evidence is very complete and satisfactory.

It is well known that an unusually large number of minor systems occur simultaneously with the August Perseids. The positions of many of these are now ascertained with considerable precision. The labours of Heis and Schmidt, ably supplemented by Greg, Alex. Herschel, Zezioli, and others, have furnished a multitude of observations which are satisfactorily accordant as to many of the secondary showers of the epoch. The results obtained in the present year have been extremely productive of tenuous radiants. I select five of these as affording instances of very definite showers:—

No.	1886	Radiant ° °	No. of meteors	Features
1	July 27–Aug. 11	291 + 51	14	Rather slow.
2	July 31–Aug. 11	350 + 51	11	Rather swift.
3	August 2–11	48 + 43	10	Swift, streaks.
4	August 2–4	26 + 42	6	Swift, streaks.
5	July 31–Aug. 2	20 + 58	7	Swift, streaks.

Nos. 1 and 2 I observed also in August 1885 (see NATURE, vol. xxxii. p. 415), when I derived their radiants at $292^\circ + 52^\circ$ and $345^\circ + 53^\circ$ respectively. No. 3, between α and β Persei, I observed in July and August 1877, and again on July 23 25, 1884 (*Monthly Notices*, December 1884, p. 107). No. 4, near γ Andromedæ, has also been pre-observed here in August 1877 and 1879; and No. 5 represents the Cassiopeiads, which have long been known as a pronounced companion shower to the Perseids.

The position No. 2 at $350^\circ + 51^\circ$ lies between Cassiopeia and Lacerta. It was the most prominent of all the minor streams of the August epoch in 1885, and in 1877 I had observed it well both in July and August. It has also been noticed by many others in recent years. Taking an average of fifteen different observations the radiant comes out at $350^\circ{\cdot}2 + 52^\circ{\cdot}1$. This particular shower, by its increasing activity during the past few years, appears to have supplanted Mr. Greg's *Lacertids* at $335^\circ + 52^\circ$, which have evidently not maintained their former strength. It is probable also that during the period of Mr. Greg's researches this August shower at about $350^\circ + 52^\circ$ was comparatively quiescent, for there is no reference to it in his catalogue of 1876. The same may also be said of the system of Cygnids at about $291^\circ + 51^\circ$ (near θ Cygni). Possibly, however, the latter may have been formerly confused with the Draconids (= Greg, No. 78). In the "Annuaire pour l'an 1885, publié par le Bureau des Longitudes" I find that two of the chief showers accompanying the Perseids on August 9–14 are stated as at $345^\circ + 50^\circ$ and $294^\circ + 52^\circ$. My recent observations just described confirm this pair of showers in the most definite manner, and they will doubtless be similarly corroborated wherever systematic observations of the Perseids are conducted.

W. F. DENNING

THE SWISS SOCIETY OF NATURAL SCIENCES

THE annual meeting for this year of the Swiss Society of Natural Sciences opened at Geneva on the 10th instant under the presidentship of Prof. Louis Soret. This precursor of all itinerant scientific societies was founded in 1815 in Geneva, and the present is its seventh meeting in the city of its birth. The members and visitors were received on the evening of the 9th in the *salons* of the celebrated Palais Eynard, which, after being long closed, were opened specially for the occasion. After the presidential address on the 10th, a new committee for the forthcoming period of six years was appointed, with its seat at Berne, the next meeting was fixed to take place at Frauenfeld, in Thurgau, and Prof. Grubenmann was elected president.

Prof. Soret in his address first referred to the advantages offered by Geneva to men who have taken science for their vocation, and then, under the title of "*Des impressions réitérées*," developed a series of new and original ideas on æstheticism analysed by the man of science. The repetition, he said, of the same design, whether in a symmetrical form, or in lined designs, such as we see in tapestry, furniture, or buildings, whether of the same dimensions, or of dimensions regularly decreasing, gives an agreeable impression. It is the same with regular curves, but the æsthetic sensation dwells less in the sensation itself than in the intuition which it gives us of a law. This applies not only to form, but also to sound and to colours. Developing these ideas, M. Soret insisted on the part played constantly by repetitions and similitudes because they evoke by intuition the idea of a law.

M. Marcel Deprez then read a paper on the transmission of force by means of electricity, in which he described his recent experiments between Creil and Paris and the results. M. Rilliet, of Geneva, read the report of a commission appointed to investigate the depth to which light penetrates water. Dr. Heim, of Zurich, read a paper on the deformation of fossils in mountains. He described the modifications which rocks undergo in form even after induration. Under the enormous pressure of the rocks above they may become laminated without any visible solution of continuity in the mass, or any rupture.

The effect on the fossils which they contain is similar; these are sometimes enlarged into the most grotesque forms, and hence Agassiz was misled in distributing the fossil fish of the older rocks into eighty distinct species, a considerable number of which were of the same species but deformed in various ways so as to appear different.

Subsequently the members were present at the inauguration of a monument erected to the memory of Gosse, the founder of the Society. Much interest was attracted by the new geological map of the Republic exhibited in the hall. It has just been completed, and is the fruit of twenty-seven years of the labour of a number of geologists under the superintendence of M. Alphonse Favre, who has now the satisfaction of seeing the end of this great task.

On the second day, in the Botanical Section, Prof. Muller, of Geneva, spoke of his systematic researches into the lichens of the *Graphidæ* group, of which he is about to make a general revision; Dr. Fischer, of Berne, described a new fungus (*Hypocrea*); Dr. Nuesch, of Schaffhausen, read a paper on the origin of Bacteria; and Prof. Schuetzler described a curious moss which grows at a depth of 200 feet in the sub-lacustrine moraine of Yvoire. It contains grains of chlorophyll perfectly formed, and is probably a variety of *Thamnium alopecurum*. Prof. Magnus, of Berlin, recounted his observations on the fecundation amongst aquatic plants, and more especially species of the *Naias*. M. Pittier spoke of the modifications being slowly made in the Vaudois flora, certain plants having disappeared wholly, while new ones have taken their places. M. Casimir de Candolle described his investigations into the action of low temperatures in germination.

In the Section of Zoology and Physiology Prof. Auguste Forel communicated a written memoir of the perception of violet by ants. He came to the conclusion that they perceived it with their eyes, and not through the skin. The so-called photodermatic sense does not appear to exist in ants, or at least is of small importance compared with ocular vision. M. Goll read a paper recording his observations on the fauna of Lower Egypt, especially of the fishes of Fayoum. There exists a well-marked distinction between the fauna of the desert and that of the Nile, particularly in colour. Dr. Zschokke gave some details on the development of the *Scolex polymorphus*, a kind of parasitic worm which he studied at the Naples Zoological Station. He thinks that Wagener's classification of the Scolices is not a natural one. Prof. Blance, of Lausanne, continues his studies of the fauna of the Lake of Geneva, and presented a memoir on a new Protozoa which he discovered in a deep part of the lake, and which he names *Gromia brunneri*.

In the Geological Section Dr. Schmidt, of Freiburg in Brisgau, read a paper on the geological and mineralogical nature of the schists of the Grisons, which, it is now demonstrated, belong to the Jurassic. He spoke particularly of the mica which is one of their constituent elements. MM. de Fellenberg and Baltzer described the remains of great vegetable fossils found at Guttanen in the crystalline schists in the mass of Finsteraarhorn. M. Greppin exhibited a beautiful collection of fossils, not yet determined, of the oolite, found in the Jura in the neighbourhood of Basle. Prof. Renevier, of Lausanne, read a report on the excursions made by the Swiss Geological Society in the Vaudoise higher Alps during the five days preceding the meeting. M. Schardt described the geological structure of the Dent du Midi. M. Steinmann gave an account of a journey extending over two years in the Cordilleras of South America, between Bolivia and Patagonia. He sketched rapidly the characteristics of this great chain. The fossil fauna and flora are almost identical with those of European formations. The Upper Trias, Rhætian, Lias, Jurassic, and Cretaceous are all represented.

Of the pleasures as apart from the business of the meeting it is needless to speak. The Genevese authorities and people gave the members a hearty reception, and the whole town was *en fête*. Among the honorary members elected was Dr. J. H. Gladstone.

THE BRITISH MEDICAL ASSOCIATION AT BRIGHTON

THE annual meeting of the British Medical Association is anticipated not only as an occasion for the association and communion of medical men of all classes, but as an opportunity for, so to speak, taking stock of the progress of medical science and practice during the past year. From the choice of a locality near the metropolis, the meeting this year has been very successful, both as to the numbers attending it and the character of the papers read. From the tone of many of the addresses, indeed, it is easily perceived how intimately chemistry, physiology, biology, and even physics are becoming associated with medicine, and how, as a result of this, the special medical departments of pharmacology and therapeutics, pathology and hygiene, are being modified by scientific methods of investigation.

The subject chosen by the President, Dr. Withers Moore, for his address, viz. the higher education of women, was one which, though of interest to all classes of the community, did not lend much scope for the introduction of new matter. The chief argument on the medical aspect of the question brought forward by Dr. Moore was the statement that the extra tax on woman's intellectual faculties produced by this "higher education" leads to bodily degeneration and to unfitness of the individual for a woman's peculiar social duties. This is admittedly so with those who are subjected to over-pressure; still, the questions as to how far these bad effects are general among the class of women who are subjected to severe intellectual training, and how far these bad effects may be counteracted by judicious hygienic surroundings, remain yet to be solved; and the experiments in the higher education of women now being performed in America and England will no doubt yield results which will practically solve the question.

The address in Medicine, which was given this year by Dr. J. S. Billings, of the United States Army, dealt chiefly with medical politics in America, which, like our own country, needs reform in reference to medical education. It is interesting to note, from the remarks of one so well qualified to judge as Dr. Billings, the great progress made in America in the establishment of laboratories devoted to scientific medical investigation; and it may be confidently expected that by this means important contributions will be added to the stores of medical science.

It is in the departments of pathology and pharmacology that the influence of scientific thought and method is most evident. As Dr. Dreschfeld pointed out in his address before the Section of Pathology, there is in the modern study of pathology a great deal more than was comprised twenty or thirty years ago; for, besides the marvellous advances of morbid anatomy due to the improvement in histological methods and knowledge, the stimulus of experimental physiology has initiated important researches on various morbid processes. Indeed it is difficult to draw a hard-and-fast line between experimental physiology and pathology; for, in many instances, the investigation of physiological function proceeds *pari passu* with that of the loss or inhibition of that function. On the anatomical side pathology is seizing the facts discovered by purely scientific investigators, and applying them with good results. Thus, as Dr. Dreschfeld points out, the application of the researches of Flemming, Heuser, Rabl, and others, on the composition of the nucleus, to the study of the cancer-cell, has shown that this is deficient in chromatin and embryonic in character. Again, the selective action of methylene-blue for certain nervous structures when injected into the living body, as described by Ehrlich, indicates a new method of pathological research by which the condition of these structures under the toxic action of substances may be investigated. If, moreover, as Ehrlich thinks, this selective action is due to the conditions of alkalinity and oxidation in the structure, some light may be thrown by future research on the still very obscure reactions of the nucleus and cell, and, more particularly in pathology, on the chemical changes occurring in the nerves in chronic peripheral paralysis due to poisons, such as alcohol and lead. Though a strong advocate of the study of experimental pathology, Dr. Dreschfeld insisted on the necessity of an investigator having a clear idea of the object and, as far as possible, of the methods of the research which he is undertaking. This point, which is of course the basis of all useful experimentation, is very important in experimental pathology, owing to the peculiar conditions under which experiments on animals are performed in this country.

In pathology, which deals more closely with the facts of disease—disordered structure, disordered function—progress has been rapid, but not more so in its scientific aspect than pharmacology and therapeutics. A great deal of attention has of late years been devoted to this subject, as shown by the rapid accumulation of facts concerning old and new remedies. It is on such an occasion as the meeting of the Association that it is well to

stand still for a moment and see the direction in which modern therapeutics is tending. Connected on the one hand with chemistry and physiology, and on the other with pathology and medicine, it is justifiable to expect that the recent advances in these departments of knowledge would have a stimulating effect on the progress of therapeutics.

Dr. Lauder Brunton, in his address (which we print in full) before the Section of Pharmacology, illustrated one aspect of this influence by discussing the connection between chemical constitution and physiological action. It will readily be seen from a study of his remarks how important an effect the line of research which he indicated will have on the progress of rational therapeutics, which is based on a knowledge of the physiological action of a drug. Dr. Brunton's address shows a hopeful sign of advance in the treatment of disease by scientific methods and not by mere empiricism.

One of the most important communications made to the Association, and deserving of mention here, was that by Prof. O. Liebreich, of Berlin, on lanolin as a therapeutic agent. This substance, which is a cholesterin-fat from sheep's wool, is much more rapidly absorbed by the skin than glycerol-fats or vaselin, this property being probably connected with the fact that in nature it is closely associated with, if not formed by, keratin-containing cells, such as those of the skin, hair, feathers, &c. Such a readily absorbable fat, which is unirritating, and will serve as a vehicle for medicaments, has long been a desideratum, and it is probable that lanolin will be a most important agent in the treatment of skin diseases and of local disorders beneath the skin, as in the joints.

Space does not admit of a discussion of the numerous other interesting subjects, chiefly technical, introduced at the meeting of the Association. The interesting questions brought forward by Dr. Taafe in his address on public medicine included the spread of scarlatina by means of milk, a subject the investigation of which has been undertaken by the Local Government Board, and will no doubt yield important results to preventive medicine.

ON THE CONNECTION BETWEEN CHEMICAL CONSTITUTION AND PHYSIOLOGICAL ACTION[1]

THE meeting of the British Medical Association is not for mutual instruction only; it is also for recreation; and, probably, many members of this Association will utilise the opportunity which a meeting at the sea-side, like the present one at Brighton, affords them of indulging in that excellent occupation for an idle man—of watching the waves on the seashore and speculating how far each of them will come. If one have only half an hour to spare, it is difficult to say whether the tide is ebbing or flowing; it is only by watching for a longer time that one can be certain that the water is really moving in one direction or another. Probably a great part of the charm which this occupation possesses is due to the resemblance which one involuntarily traces between the ebb and flow of waters and that of human affairs—individual, national, or racial. The life of a single man is very short in comparison with the history of race; and it is often very difficult to say whether mankind is advancing or retrograding, unless we compare his condition at epochs widely removed from one another.

On doing this, we find a general consensus of opinion, to the effect that civilisation has steadily advanced; and this advancement is usually divided into four stages, characterised by the nature of the tools or weapons employed. In the first, or Palæolithic Age, man employed weapons or tools of flint roughly chipped into shape and unpolished. In the next, or Neolithic Age, the implements consisted of stone, but they were polished. The next age is characterised by the employment of bronze as a material, and the fourth and highest stage by the employment of iron. These stages are not all marked off from one another, for we find them together in the same country or in different countries. Thus, the age in which at present we live is recognised as the Iron Age, on account of the large employment of that metal; but we find that in various countries stone, more or less rudely fashioned, is still used in the manufacture of weapons or tools. For example, when I was in the Colonial Exhibition lately with Mr. Norman Lockyer, he pointed out a kind of threshing implement, such as is now used in Cyprus. It consists of a flat board, in the under side of which are embedded a number of stone celts exactly like those made by prehistoric man, and perhaps used by him for a similar purpose as well as for axes. In the same way that we recognise four stages in the development of the implements used by man in the arts or in warfare, we may, I think, recognise four stages in the development of the implements he has used in the treatment of disease. In the first stage crude drugs were employed, prepared in the roughest manner, such as powdered cinchona or metallic antimony. In the next stage these were converted into more active and more manageable forms, such as extracts or solutions, watery or alcoholic. In the third stage the pure active principles, separated from the crude drugs, were employed, *e.g.* morphine and quinine. In the fourth stage, instead of attempting to extract our medicines from the natural products in which they are contained, we seek to make for ourselves such substances as shall possess the particular action we desire. Now, just as we find stone and iron implements occasionally used together in the same country, so we find that drugs belonging to the different stages mentioned are used at the same time. For example, we may find crude powders, alcoholic extracts, and pure alkaloids all contained in the same pill. Nay more, we may sometimes give to the patient in addition to all these, a medicine made artificially. But, while this condition still exists, we notice that crude drugs are being less and less used, and their place is gradually being taken by pure active principles. We may say, then, that we are passing at present from the Stone Age into the Bronze Age of pharmacology; and may indeed be said to be just entering on the Iron Age. This age may be said to have begun about twenty years ago, when the researches which my predecessor in this office, Dr. Fraser, made with Prof. Crum Brown upon the connection between physiological action and chemical constitution, inaugurated a new era in pharmacology. They found that, by modifying the chemical constitution of strychnine, they could also alter its physiological action, and convert it from a poison which would tetanise the spinal cord into one which would paralyse the motor nerves.

We might perhaps date the beginning of this age from Blake's attempts to show that a connection exists between the form in which various bodies crystallise, and the mode in which they act upon an animal body. Richardson, too, had observed that, amongst various compounds of carbon, certain differences existed in physiological action which might be supposed to correspond to differences in their chemical composition. And at the same time that Crum Brown and Fraser were making their experiments, Schroff in Vienna, and Jolyet and Cahours in France, had independently arrived at somewhat similar conclusions; nevertheless, I think we may fairly say that it was the experiments of Crum Brown and Fraser which fairly started pharmacology in the new direction in which it has since been steadily advancing. It would be impossible for me to enter at all fully into the recent development of this branch of research, but I think it may be both interesting and useful to try to give you a short and popular account of the chief points already made out; and, in doing so, I may perhaps be excused for using, almost to the extent of abusing, similes which are not precisely exact, but which may be useful in giving you a rough idea of a somewhat complicated subject.

We have all heard of the "flesh-pots of Egypt"; but I find that everybody is not acquainted with the "flesh-pots of Shiloh," though "good little Samuel" has probably been frequently held up before us as an example to be followed, and possibly the naughty sons of Eli as an example to be avoided. When these sons of Eli were priests in Shiloh, their custom was, when any man offered a sacrifice, to send their servants with a "flesh-hook" of three teeth, in his hand, which he struck into the pan, or kettle, or cauldron, or pot; and all that the flesh-hooks brought up the priest took for himself.

It is obvious that what the priest's man brought up would depend very greatly on two things, viz. the contents of the pot and the nature of the hook—whether it were large or small, sharp or pointed, single-pronged or many-pronged. It is obvious, too, that a very slight alteration of the points, by the judicious application of a file or whetstone, might considerably influence the savouriness of the priest's dinner. With the small pots that they were likely to have in Shiloh, it would not matter much what the nature of the handle was; but it would matter

[1] An Address delivered at the opening of the Section of Pharmacology and Therapeutics, at the Annual Meeting of the British Medical Association held in Brighton, August 1886. By Thomas Lauder Brunton, M.D., F.R.S., Lecturer on Materia Medica and Therapeutics at St. Bartholomew's Hospital; President of the Section.

very greatly if the priests had to go fishing in the brazen sea of Solomon, for there, with a short handle, they might not be able to reach the tit-bits in the middle, and if the handle were too long, they might go plunging their hooks about the opposite side of the vessel, with the same result as if the handle were too short. Now, in the drugs which we use in medicine, we may find a certain analogy with these flesh-hooks, some part of the drugs being comparable to the hooks, and others to the handle. Perhaps the analogy would be even more correct if we were to regard the hooks as having movable points, which could be taken off and replaced by others of a different form or sharpness. If we take alkaline salts as an example, we may regard the base as the handle, and the halogen as the hook; and by modifying either of these, we may alter the parts of the body affected and the manner in which they are affected. We might, indeed, compare chloride of sodium, in which we have the chlorine attached to sodium, with the low molecular weight of 23, to a hook with so short a shank that it did not reach the big joints lying in the middle of the cauldron; while potassium, with a molecular weight of 40, was just long enough to do this; and rubidium, with a molecular weight of 85, was so long as to go plunging about on the other side. In fact, we find that this is very nearly what occurs in the muscles of the animal body after the administration of the chlorides of sodium, potassium, or rubidium; for, while potassium chloride is a powerful muscular poison, the action of sodium and rubidium chlorides on the muscles is very slight.

We have seen what changes would follow alterations in the shank of our flesh-hook; now let us see the effect of altering the prongs. If we put on a small one like chlorine, it may go dragging about catching everything, but bringing out nothing; a bigger one, like bromine, may lay hold of a lung or a brain; and a bigger one still, like iodine, may lay hold of a big joint. Now, what we find in the body seems to be somewhat similar. The chlorides circulate in the blood without producing any marked alteration beyond that which is due to the substance with which the chlorine is combined. The bromides attack the brain and nerve-centres, and the iodides tend more especially to affect the muscles and the glands.

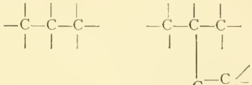
It is evident that another important factor besides the sharpness of the hooks is the number of prongs, and the three-pronged hook seems to be the generally effective one. Now, in pharmacology, there is one substance—nitrogen—which appears sometimes to have three, and sometimes five prongs, or affinities, as chemists term them, and it is a substance having a very general and powerful influence over the body. When combined with hydrogen in the form of ammonia or of ammoniacal salts, it affects nerve-centres, motor nerves, and muscles, tending first to stimulate and then to paralyse them. But, as we would expect, the effect of the ammonia is modified by its combination with iodine, chlorine, and bromine; and we find that, while the ammonium-chloride generally attacks the spinal cord and causes irritation, ammonium-iodide paralyses the motor nerves and muscles.

When nitrogen has oxygen combined with it in place of hydrogen, so as to form nitrous acid, its action is exerted more especially upon the blood and blood-vessels, so that it causes the blood to become chocolate-coloured, and the blood-vessels to dilate. This power of dilating the vessels is sometimes exceedingly useful in the treatment of disease; and we have been enabled to vary the action of our drugs so as to attain, to a great extent, the end we desire, by our knowledge that the action depends upon the nitrous acid, and not on the substance to which the acid may be attached; or, to return to our own comparison, the effect depends on the nature of the hook rather than on the kind of shank to which it is attached. Thus, where rapid dilation is desired, we use nitrite of amyl; but where a slower and more prolonged action is desirable, we employ nitrite of soda or nitro-glycerine.

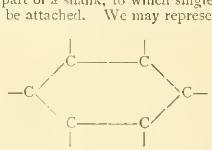
In some useful tools we have the two ends serving different purposes: one end, for example, being a hammer and the other end a claw for extracting nails; and we can easily imagine a flesh-hook constructed on the same principle, one end, let us say, having the prongs widely apart, and the other the prongs close together. With such a hook, it is evident that the viands which were fished up would be different according as one or other end was put into the pot, for the close prongs would bring up delicate little pieces which would simply slip through the wide ones. If we carry our illustration a step further, and suppose this hook to consist of two parts attached to one another by certain prongs, while others were left free, we can see that, if only one prong were left free in each part, but these prongs were of different shapes, the pieces obtained by the man using it would be of a different kind, according as the prong belonged to one end or the other. Now we seem to find something of this sort in the union of nitrogen with carbon. Carbon is a substance with four affinities, while nitrogen appears sometimes to have three and sometimes to have five. When the nitrogen and the carbon are united in such a way that four affinities of each are connected together, leaving one free affinity or prong belonging to pentad nitrogen, thus, $-N \equiv C$, the compound is exceedingly poisonous; whereas, when the free affinity or prong belongs to the carbon and the other three affinities are joined to triad nitrogen, thus, $-C \equiv N$, the compound is comparatively innocuous.

This fact shows us how very important the nature of the free affinity in the compound is in regard to physiological action.

We have just pictured to ourselves an instrument of two parts, joined together by small hooks, and consisting, in fact, of two links. In this instrument the links differ a good deal from each other; but one link—namely, carbon—has a great power of uniting with itself, so as to form long chains, straight or branching, thus—

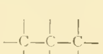


It also forms what we may possibly regard as close chains, so stiff as to act the part of a shank, to which single hooks or long open chains may be attached. We may represent it graphically thus—

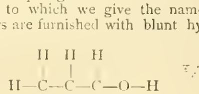


Now, if any of Eli's successors wanted to fish in Solomon's brazen sea with hooks attached to a flexible chain instead of to a stiff shank, the results of his fishing would not only depend on the hooks he used, but on the length of the chain, on the kind of chain, single or branching, and on the position of the links to which the hooks were attached.

Now, in the series of chemical substances to which alcohol belongs we have an illustration of the modifications in physiological action which are produced by the length of the chain, the kind of chain, and the position of the hooks. The links, in the case of alcohol, consist of carbon atoms attached to each other by one affinity, so that each terminal atom, or link, has three affinities, or prongs, and the intermediate links have two each unattached, thus—



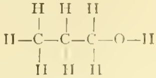
We may regard one prong of one terminal link as furnished with a sharp point, to which we give the name of hydroxyl, while all the others are furnished with blunt hydrogen points, thus—



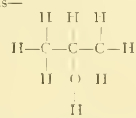
All the alcohols attack the nerve-centres, and paralyse the brain, the spinal cord, and the centres of organic life in the medulla oblongata. In large doses they all produce death, and the longer the chain the more deadly do they become, until the chain is so heavy that it can hardly be used at all, or, in other words, till the alcohol becomes so solid that it will not readily enter the body and produce its toxic action.

If we fix the sharp hydroxyl on one of the intermediate links, instead of the end one, we would naturally expect that it might simply scratch the pieces of meat instead of pulling them out, as it might do if it were attached to the terminal link; and this

is exactly what we find in the case of alcohol. For example, primary propyl alcohol,—

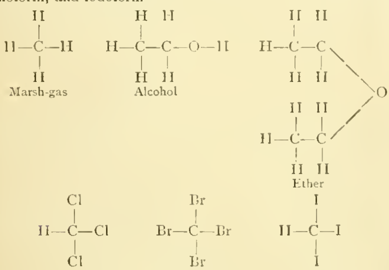


where the hydroxyl is attached to the terminal link, appears to produce steadily increasing paralysis of the nerve-centres; but secondary propyl alcohol, where the hydroxyl is attached to an intermediate link, thus—



scratches up or stimulates the nerve-centres before it paralyses them (Efron Pflüger's *Archiv*, Band xxxvi., 1467).

The whole of the carbon compounds, formed on the principle of an open chain, appear to have an action more or less like that of alcohol, though these are modified by the nature of the substances which "tip," as it were, the free affinities of the carbon links. Thus, marsh-gas, alcohol, ether, chloroform, bromoform, and iodoform—



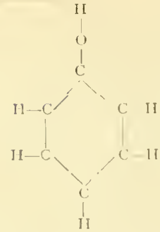
Marsh-gas Alcohol Ether

Chloroform Bromoform Iodoform

all tend to paralyse nerve-centres, and to exert an anæsthetic action; but the chloral in the chloroform tends to make the substance paralyse the heart more quickly than marsh-gas, alcohol, or ether, which contain hydrogen alone, or hydrogen and oxygen; and in iodoform the effect of the carbon is to a great extent swamped by the iodine.

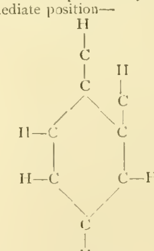
It is to Liebreich's recognition of the fact that similar carbon compounds possess a similar anæsthetic action that we owe the discovery of chloral. The knowledge of the depressing action on the heart of chlorine in such compounds led Schmiedeberg and Cervello to search for a hypnotic substance which should not contain chlorine, with the result that paraldehyde has been added to our therapeutic *armamentarium;* and the stimulant action of ammonia led Schmiedeberg to introduce a new hypnotic, urethane, which, like chloral, will produce sleep, but, instead of weakening, will stimulate the heart, and is thus admissible in cases where chloral might be dangerous.

Let us now turn to the other class of carbon compounds in which the atoms are arranged so as to form a close chain, or, as we may call it, a stiff nucleus or shank, to which either single hooks or open chains may be attached. This group of carbon compounds is termed the aromatic series. The substances belonging to it differ from those of the open chain or fatty groups, inasmuch as they tend to stimulate the nerve-centres, and produce convulsions or spasms before paralysing them. But the most marked property which they possess appears to be their power of reducing temperature, and of destroying low forms of life, so that they act both as antipyretics and as antiseptics. We have seen that in the open chains of the fatty series of carbon compounds, the increased number of links appears to increase the activity of the compound, and a condition which is similar, in some respects at least, is to be found in the aromatic series. For example, in phenol or carbolic acid, as it is usually termed, we have one hydroxyl terminal, just as in ordinary alcohol; the other carbon affinities being saturated with hydrogen—

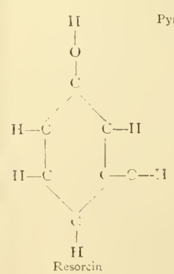


When these hydrogen atoms are replaced by methyl, the antiseptic power of the phenol is increased, and the increase appears to be in proportion to the number of methyl groups which are introduced into the compound. Turning again to our old illustration of the flesh-hooks, we might compare the benzene nucleus to the shank with six points, each of which might be armed either with a sharp hydroxyl hook, or with a blunt hydrogen one, or with a carbon chain. The more the blunt hydrogen hooks were replaced by chains, the more thoroughly would they sweep the pot; and, in fact, we may say that the more chains there are instead of hydrogen, the more thorough is the antiseptic action of the compound.

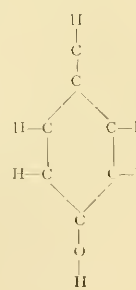
In the case of antiseptics, all that we want is to insure a thorough destruction of the microbes, which give rise to putrefaction or disease; but when we come to deal with antipyretics we have a more complicated problem before us, for we wish to reduce the temperature in man or the higher animals, while at the same time we have to avoid producing any marked action on the nervous system in the way either of spasms or paralysis, and also to avoid depressing the circulation and causing collapse. Now several bodies nearly allied to carbolic acid, and differing from it only in the fact that the benzene nucleus in them has two hydroxyl groups attached to it instead of one, as in carbolic acid, have a strong antiseptic power. These bodies are hydroquinone, resorcin, and pyrocatechin; they all have an antiseptic action, but the strength of their action is very different, resorcin having only one-third of the strength, and pyrocatechin only one-fourth of that of hydroquinone. This difference in strength shows us here, also, how important the position of the hydroxyl groups is; because, in pyrocatechin they are close together, in hydroquinone they are as far apart as they can be, and in resorcin they keep an intermediate position—



Pyrocatechin



Resorcin



Hydroquinone

But these bodies, perhaps from their simple structure, appear to be adapted to attack all parts of the animal organisation, and they are apt to affect the nervous system and circulation. In order to avoid these disadvantages, various attempts have been made to obtain bodies of a similar but more complicated structure, which should have a more specialised action, and would lower the temperature while leaving the nervous system and circulation unaffected. These attempts have been more or less successful, and we owe to them the introduction of three new remedies—kairin, thallin, and antipyrin. The former two, after a brief period of trial, have been found more or less unsatisfactory; but the latter is perhaps, upon the whole, the best antipyretic that we possess, reducing the temperature and, at the same time, having few disadvantages. Salicylate of soda is nearly allied in chemical constitution to resorcin, and as a general antipyretic it is almost equal to antipyrin, and superior to it in cases of rheumatic fever. It is possible that we may still obtain antipyretics more powerful than any we yet possess, and specially adapted to the febrile conditions arising from different causes, for these antipyretics do not appear to be equally successful in different kinds of fever. Antipyrin is best in hectic fever, and salicylate of soda in rheumatic fever, but an antipyretic which will be thoroughly satisfactory in typhoid fever is still a desideratum.

I have said that antipyrin is generally free from any disagreeable action; but this is not always so, for it sometimes may produce collapse. This shows us that in the action of all our drugs we have two factors to consider, namely, the drug itself and the body into which we introduce it. We have just been considering the alterations in physiological action which may be produced by changes in the chemical constitution of our drugs; but there is another factor which is perhaps more difficult to investigate, and still more important in the treatment of disease, namely, the condition of our patients. The failure of our drugs to produce the effects we desire is one of the most trying occurrences in medical practice. Thus, in fever, we sometimes find that drugs will not reduce the pulse as they do in non-febrile conditions, and digitalis in pneumonia sometimes appears to have lost its sedative action on the heart altogether. Some years ago I thought that possibly this might be due to the high temperature producing paralysis of the nervous apparatus which restrains the heart, and supposed that the peripheral ends of the vagus in the heart might be paralysed. I then made some experiments, which showed that I was wrong in this supposition. Several years afterwards my friend Dr. Cash and I made some further experiments, which showed that the failure of digitalis to slow the heart in febrile conditions is really due to paralysis of the regulating nerves of the heart; but the part of them which is paralysed by the heat is their roots in the medulla, and not their endings in the heart.

In other experiments which we made together we found that the muscle of a frog poisoned by barium could be restored to its normal condition by a high temperature, and also by the application of potash salts. It occurred to us that, if we could saturate the body of an animal with potassium, we should be able to render it proof against the poisonous action of barium. On trying this, we succeeded in rendering animals so far resistant to the action of the poison that they were alive and well after animals of similar size, but unprotected, had succumbed to the action of the same dose of poison, although we did not succeed in ultimately saving the animals.

But Dr. Cash has pursued this line of investigation far beyond the limits of our mutual research, and he has obtained results which seem to me to be amongst the most extraordinary and the most promising in pharmacology. Knowing, as he did, that corrosive sublimate was an exceedingly powerful disinfectant, it occurred to him that it might be more harmful to disease-germs than to the bodies of higher animals, and that he might be able, by the introduction of the poison into the body of an animal, to render it insusceptible to zymotic diseases. A similar idea had occurred to Koch, who injected corrosive sublimate into animals after previously inoculating them with anthrax; but his experiments failed, while Cash has proved successful by introducing the corrosive sublimate before inoculating with anthrax, and thus giving the drug the start of the disease. These experiments acquire an additional interest from the fact that M. Pasteur, although uncertain regarding the exact mode in which his process of inoculation for hydrophobia has brought about such satisfactory results, is disposed to think that the agent which prevents the disease is a chemical substance, and not a microbe.

When we look back for twenty years and see how far pharmacology has advanced since Crum Brown and Fraser's experiments directed it into a new path, we may hope that twenty years more may not only have greatly added to our stock of new remedies, but will have enabled us so to ascertain the condition of our patients that, either by the proper modification of a single remedy, by the proper admixture of remedies, or by proper changes in the food or surroundings of each patient, we may insure the action we desire, and we shall not have to feel, as we painfully do at the present, that our patients often die for lack of knowledge, not on our part, but on that of our art.

Nothing is more painful to a medical man than having to answer in the negative the agonised appeal, "Oh, doctor, can you do nothing?" of those who see passing away friends who are dearer to them than their own life. It is because we medical men know the value of human life and the extent of human suffering; because we are called upon to prolong the lives of those whom not only their friends but their country and the world at large can ill spare; because we must, if possible, relieve pain sometimes amounting to extreme torture in the sufferers themselves, and felt hardly less keenly by their friends, that we consider it is not only permissible, but is our imperative duty to gain the knowledge we require to attain our object, even though we sacrifice the lives of animals, and inflict upon them some pain—never wantonly, never carelessly, and almost always slight in comparison with what we often see our patients feel. Moreover, the lower animals suffer from disease as well as men, and we may hope that the advance of pharmacology will give us the means of relieving pain and prolonging life in them as well as in man.

SCIENTIFIC SERIALS

Journal de Physique, June.—P. Garbe, experimental researches on radiation. Examination of the formulæ proposed by Dulong and Petit, by E. Becquerel, by Violle, and by Stefan. The author holds Stefan's law to be true for absolutely black bodies only. The verifications have been made by spectrophotometric measures of glow-lamps fed from accumulators.—G. Wyrouboff, the structure of crystalline bodies endowed with rotatory power. This is a remarkable paper, traversing several conclusions hitherto believed to be proven. The author states that the alleged necessary and constant relation between rotatory power and the existence of facets indicating non-superposable hemihedry is untrue, for of eighteen such substances known, only four have been proved to have such facets, while the nitrates of lead and of baryta which are cubic with facets of this kind have no rotatory power. The author now propounds the view, which he supports by the discovery of striated structures upon the facets in question and by various strong arguments, that the real physical cause of this rotatory power is that such crystals consist of superposed laminæ crossing at different angles, and possessing biaxial refraction. In fact, he holds that these substances are only pseudo-symmetrical, and that the built-up mica plates of Reusch which show rotatory power are actual types of the phenomenon in general. He particularly refers to the optical behaviour of amethyst, and further declares that he has succeeded in proving that the true crystalline form of sulphate of quinine is clinorhombic. He regards as absolutely illusory, in the vast majority of cases, the so-called measurement of the angle of rotation by these substances.—L. Laurent, practical methods for the execution of objectives intended for instruments of precision. This paper describes means for testing during process of manufacture the curvatures, &c., of lenses intended for spectroscopes, goniometers, and such instruments.—Th. and A. Duboscq, saccharimeter for white light. This saccharimeter has a Senarmont polariscope placed between the polariser and analyser. The Senarmont polariscope consists of four wedges of quartz disposed so as to show two fringes with black central band, which in the dark field are situated exactly in line with one another. On introducing any substance that rotates the plane of polarisation, the fringes move right and left. A quartz compensator is added.—J. Voisenat, influence of nature and form of conductors upon the self-induction of an electric current. A summary of the recent papers of Hughes and H. F. Weber.—K. Angstrom, on the diffusion of radiant heat from plane surfaces.—Ch. Soret, researches on the refraction and the dispersion of the crystallised alums.—E. Wartmann, the compensated rheolyser. This instrument consists of a circular modification of Wheatstone's bridge with mercurial conductors.—R. Pictet,

new freezing-machines. Notes on industrial applications of a new liquid, namely a mixture of sulphurous acid and carbonic acid obtained on the commercial scale by the action of sulphuric acid on carbon.—J. Maurer, influence of altitude on diurnal variations of declination.—H. Schneebeli, absolute value of coefficient of friction of air. The results, which agree with Obermeyer, were made by Graham's method.—H. Schneebeli, experimental researches on the impact of elastic bodies.

Bulletins de la Société d'Anthropologie de Paris, tome ix. fasc. 2.—Continuation of M. Topinard's paper on the cephalic index. In his revised system of nomenclature M. Topinard virtually rejects Broca's method in favour of the quintuple division adopted by Prof. Flower, and generally followed by English and American anthropologists. For his old terms of "*sus*" and "*sous*" he further adopts those of "ultra" and "hyper." Thus, for example, while he considers that the true dolichocephalic group is represented by the index of 70–74 inclusive, his ultra- and hyper-dolicho- subdivisions exhibit respectively the indices of 60–64 and 65–69. The medium group standing between the dolicho- and the brachy-cephalic limits he characterises as "mesaticephalic," with an index of 75–79 inclusive; while his brachycephalic divisions range from 80, beginning with the mean representatives of the groups, to 94 as the extreme limit of the ultra-brachycephalic index.—At a later meeting, M. Topinard drew attention to the necessity for using greater exactitude in the definition of the methods to be employed in making anthropometric determinations, those of M. Bertillon as set down in his instructions regarding anthropometric identification being, in his opinion, at once complex and inexact.—On the so-called "Lenape" stone, by M. de Nadaillac, whose opinion of the possible genuineness of the stone is, as he informs us, based only on the testimony of others.—On the occurrence of amber in the prehistoric graves of the Département des Basses Alpes, by M. Bonnemère. These finds were formerly so frequent that the peasants in some districts used amber for lighting their dwellings, and hence it was locally known as "peïra cremarela," or burning stone. This name is still applied to it at Salignac, where many of the villagers are in possession of amber, all of which is more or less red in colour.—At a later meeting, M. Bonnemère described to the Society some curious bronze disks found by M. Ollivier in graves near Salignac, and which appear to have been used to strengthen the outer surface of cuirasses and other forms of protective armour. In a cemetery in Carniola, belonging to the early Iron Age, a helmet has been found composed of similar bronze knobs fastened in rows to an inner skin lining.—Anthropology and philology, by M. Beauregard. The object of the writer is to show the importance of the comparative study of languages to determine the usages, and moral and mental status of various nations. He specially passes in review the languages of Egypt and South Africa, Mexico, Peru, and the Red Indians, indulging in many fanciful deductions regarding assumed ethnic affinities.—On the origin of life, by M. Fauvelle. The author believes that modern science justifies the theory that the simplest forms of green Algæ represent the earliest manifestations of organised beings, in which chlorophyll was the active agent.—On impregnation, and the influence exercised on subsequent offspring by the first conception, by M. Fauvelle.—On the effects of long and short periods of military service in the French army on the health and physical development of the men, by M. Lagneau.—On the origin of the fabrication of glass, by M. Mortillet.—Morphological description of the brain of Gambetta, by MM. Chudzinski and Mathias Duval. This extremely minute report of the *post-mortem* examination, undertaken at the instance of the Society, is illustrated by numerous sectional drawings of the various convolutions, which exhibit a remarkable degree of complexity, and an unusual regularity in the arrangement of the folds.—M. Beauregard laid before the Society various objects obtained from the Gauchos of the Pampas, including the curious "botas de potro," or boots made from the skin of the hind legs of horses, mules, or oxen.—On the exploration of the tumulus of Kergouret at Carnac, in 1885, by M. Gaillard. A few implements and a diorite hatchet are almost the only finds yielded by the recent explorations of these dolmens, which were nearly destroyed, and their contents almost wholly removed at the time of their original discovery about twenty years ago.—Report, by M. Hamy, of the results of the explorations, conducted by M. Charney, in the mountainous region of Popocatapetl, in Mexico. Unusual interest attaches to these researches, which have brought to light the existence of two ancient Mexican cemeteries, in one of which the remains belonged exclusively to young children. Among the numerous fragments of bones were a great mass of broken jars and vases decorated with various emblems of the divinities Chalchiutlicue and Tlaloc, to the latter of whom young children were sacrificed on high places to secure rain.

Bulletin de l'Académie Royale de Belgique, June.—On the origin of the phosphate of lime in the brown chalk phosphatic beds of Ciply, by F. L. Cornet. These beds, which have a mean thickness of 21 metres, and about 18 per cent. of phosphate, are shown to be undoubtedly of animal origin, as attested by the large proportion of nitrous organic substances contained in them. The brown chalk appears to have been deposited in a shallow sea inhabited by a numerous fauna of invertebrates, fishes, and large Saurians. The deposits were slowly formed in tranquil waters during a long geological epoch, as shown by the great thickness of the phosphatic beds, the perfect regularity of the layers, and the state of the fossil shells found in them. These deposits, which occur nowhere else, may have been caused by the periodical destruction of fish, such as at present occurs annually in the Gulf of Aden.—Note on the parallelism between the Carboniferous Limestone of North-West England and Belgium, by L. G. de Koninck. It is pointed out that the synchronism of these various systems is far from being fully established. The Tournai formation would appear to be older than the English fossiliferous mountain limestone, while the Visé rocks may be contemporary of the Yoredale series. On the other hand, the zone of *Productus giganteus* seems to have acquired a far greater development in the north of England than in Belgium.—Remarks on the law regulating the tension of fluids, by P. de Heen. The formula recently announced by the author is here shown to apply not only to stable fluids, but also to those whose physical constitution varies with the temperature.

SOCIETIES AND ACADEMIES

Sydney

Linnean Society of New South Wales, June 30.—Prof. W. J. Stephens, F.G.S., President, in the chair.—The following papers were read:—Note on *Ctenodax wilkinsoni*, by William Macleay, F.L.S. It is here explained that the fish described by Mr. Macleay under the above name has been ascertained by Dr. Ramsay, of the Australian Museum, to be closely allied to *Tetragonurus cuvieri*, of Risso. Some remarks are also made on the habits and affinities of the fish.—Notes on the recent eruptions in the Taupo Zone, New Zealand, by Prof. Stephens, M.A., F.G.S. In this note the author gives particulars of the late volcanic disturbances, and such information as to the geographical and geological features of the district, as may perhaps enable those living at a distance to understand more clearly the accounts of the recent outbreak which have already appeared in the newspapers.—Notes on Australian earthworms, Part I., by J. J. Fletcher, M.A., B.Sc. Up to the present time but three Australian earthworms have been described, *Lumbricus novæ-hollandiæ*, Kinberg, and *Digaster lumbricoides*, Perrier, from N.S.W., and *Megascolides australis*, McCoy, from Victoria. In this paper a fuller account is given of Kinberg's species, and descriptions are given of six new or undescribed worms from the rich volcanic soil of Burrawang and of Mt. Wilson. Of these, two species (*P. coxii* and *P. australis*) are referred to Schmarda's genus *Perichæta;* two others (*N. camdenensis* and *N. grandis*, are included in a new *intraclitellian* genus *Notoscolex;* a fifth (*Didymogaster silvaticus*) also is *intraclitellian*, but differs from *Notoscolex;* and the sixth (*Cryptodrilus*) is *postclitellian*, with eight rows of setæ, but is different from *Digaster*. Three of these, as far as is known at present, occur only at Burrawang, one at Mt. Wilson only, one is common to both localities as well as Sydney, and one occurs at Burrawang, Springwood, and Jervis Bay. Mr. Fletcher has heard of the occurrence of worms, some of them very large, in the Hunter and Manning River districts, and probably these, as well as Illawarra, the Richmond and Clarence districts, and other parts of the colony will yield, when systematically searched, a good harvest of earthworms. He therefore appeals to the members of the Society resident in these or other localities, either for information or for specimens put alive into good methylated spirit, or sent alive packed in a tin box or large bottle, with a little earth and plenty of damp moss. Information as to the existence or otherwise of earthworms in

the plains of the interior would also be very valuable.—Notes on the distribution of *Ceratella fusca*, Gray, from the coast of New South Wales, by John Brazier, C.M.Z.S. A number of instances are given of the occurrence of this Hydrozoon near the Heads of Port Jackson. Mr. Brazier also mentions that a specimen sent from the British Museum to the Australian Museum as *Ceratella fusca*, Gray, is really *Dehitella atrorubens*, Gray, from Algoa Bay.

PARIS

Academy of Sciences, August 9.—M. Émile Blanchard in the chair.—On the problem of Gauss concerning the attraction of an elliptical ring, by M. Halphen. Although a clear demonstration of this well-known problem has lately been made by G. W. Hill (Simon Newcomb's "Astronomical Papers," vol. i. 1882), a fresh solution is here proposed, which has the advantage of not requiring the preliminary resolution of an equation of the third degree.—Observations on the oldest sedimentary formations in North-West France (concluded), by M. Hébert. It is shown that the clay-slates of Saint-Lô, which are pre-Cambrian or Archæan formations, were deposited in horizontal layers in a marine basin, which stretched from Wales southwards to Quimper and Alençon, and which was broken only by a few isolated masses of granite and crystalline schists. This oldest of oceanic waters lasted for a long geological epoch, as attested by the thickness of these deposits, and the transformation of the muddy sediment into hard clay-slates. The present vertical position of these rocks, which were antecedent to all animal life, was evidently due to contraction of the terrestrial crust, by which were determined the foldings, faults, and ruptures, and probably the general upheaval of the whole region.—Reply to M. Hugoniot's note on the pressure that exists in the contracted section of a gaseous vein, by M. Hirn. To M. Hugoniot's objection the author replies that he has shown by experiment that the gas flowing through a cylindrical tube into a reservoir, where it becomes very rarefied, falls gradually from the pressure P_0, which it possesses in the gasometer, to a pressure P_x, which is almost exactly that of the rarefying reservoir.—On the velocity of the flow of fluids, by M. Th. Vautier. Having in a previous communication explained his graphic method, the author here shows the process by which he has successfully applied the revolving mirror to the measurement of the velocity of fluids.—Spectrum of the negative pole of nitrogen: general law of distribution of the rays which appear in the bands of the negative pole, by M. H. Deslandres. In the luminous region, which alone has hitherto been studied, the spectrum of the negative pole is accompanied by faint traces of positive bands. But in the ultra-violet region it is prolonged only by a small number of bands, and becomes, so to say, smothered amid the powerful and numerous positive bands. The rays of the band λ 391 are disposed according to the following simple law: The intervals from one ray to another, calculated in numbers of vibrations, are arranged as nearly as possible in arithmetical progression. This appears to be a general law, not merely an isolated fact, as observed by Piazzi Smyth and Herschel between sixteen rays of the green band of the oxide of carbon.—On the temperatures and critical pressures of some vapours in liquids, by MM. C. Vincent and J. Chappuis. In a previous communication the authors announced their researches on the temperatures and critical pressures of two series of gaseous bodies at the ordinary temperature. Here they give the result of their experiments with liquid bodies at the ordinary temperature—the chloride of propyl, the series of the three amines of ethyl, and the two first normal amines of propyl.—Researches on the variations of solubility of certain chlorides in water in the presence of hydrochloric acid, by M. Guillaume Jeannel. From his experiments with the chloride of potassium the author infers that the variations of solubility of this salt are not subjected to the law recently announced by Engel. He arrives at the general conclusion that the solubility of the chlorides precipitated by hydrochloric acid varies in the presence of the acid, so that the sum of the equivalents of water, salt, and acid forming the solution remains constant at the same temperature, whatever be the chloride and whatever be the proportions of the mixture.—Combinations of ammonia with the metallic permanganates, by M. T. Klobb.—Chemical and thermic study of the phenosulphuric acids: paraphenosulphuric acid, by M. S. Allain-Le Canu. This paper is devoted to a fresh study of the three phenosulphuric acids (oxyphenylsulphonic) $C_{12}H_6S_2O_8$.—On the presence of lecithine in vegetation, by MM. Ed. Heckel and Fr. Schlagdenhauffen. The authors' researches confirm the conclusion already arrived at by Hoppe-Seyler and Krætzschmar that this substance, known to exist in many of the animal tissues, is found also in numerous plants.—Note on fine-flavoured brandy distilled from the grape-cake of white wine, by M. Alph. Rommier.—Fresh researches on the axial nervous current, by M. Maurice Mendelssohn. It is shown that the axial current possesses the same physical and physiological properties that M. E. Du Bois-Reymond has discovered in other nervous currents; also that its direction is in the closest relation with that of the function of the nerve.—On the alterations produced in the constitution of the blood by the action of the sulphuret of carbon on the animal system, by MM. Kiener and R. Engel.—On the resistance of the virus of glanders to the destructive action of atmospheric agencies and of heat, by MM. Cadéac and Malet. It is shown that this virus loses its virulence in humours exposed to the open air after complete desiccation; also that it is destroyed rapidly in warm and dry, slowly in cold and moist weather.—On the disposition of the limestone breccias of the Alpujarras Range, Andalusia, and their resemblance to the carboniferous breccias of Northern France, by MM. Ch. Barrois and A. Offret.—On a method of volumetric analysis for the sulphates, by M. H. Quantin.

BOOKS AND PAMPHLETS RECEIVED

"Life and Labours of John Mercer," by E. A. Parnell (Longmans).—"Arc and Glow Lamps," by J. Maier (Whittaker).—"Fourth Report of the U.S. Entomological Commission," by N. Riley (Washington).—"List of Foreign Correspondents of the Smithsonian Institution," by G. H. Boehmer (Washington).—"List of Institutions in the U.S. Receiving Publications of the Smithsonian Institution" (Washington).—"Bulletin of the U.S. National Museum," No. 30, "Bibliographies of Amer. Naturalists," iii., "Publications relating to Fossil Invertebrates," by J. B. Marcou (Washington).—"Quarterly Journal of the Geological Society," vol. xlii. part 3, No. 167 (Longmans).—"Catalogue of Birds of Suffolk," by Rev. C. Babington (Van Voorst).—"Elements of Plane Geometry," part 2 (Sonnenschein).—"A New Physical Truth," by E. J. Goodwin.—"Progress in Zoology, 1885," by Prof. Gill; "Progress in Chemistry, 1885," by H. C. Bolton; "Progress in Geography, 1885," by J. K. Goodrich: "Progress in Astronomy, 1885," by W. C. Winlock: "Progress in Anthropology, 1885," by Prof. Mason; "Record of North American Invertebrate Palæontology, 1885," by J. B. Marcou; "Progress in Vulcanology and Seismology, 1885," by Prof. Rockwood (Smithsonian Institute, Washington).

CONTENTS

THURSDAY, AUGUST 26, 1886

THE PHYSIOLOGY OF PLANTS

Lectures on the Physiology of Plants. By S. H. Vines, M.A., D.Sc., F.R.S. (Cambridge University Press, 1886.)

IT has already been pointed out in the columns of NATURE that our botanical schools in England are at present leading in great measure a parasitic existence on those of Germany in respect of the text-books which are in common use: translations of the works of Prantl, Sachs, and De Bary are in the hands of most of our students, while the number of manuals from foreign sources will shortly be increased by the publication of others which are in active preparation. However greatly we may admire the works of the above authors, still it is with no small satisfaction that we turn from them to the review of a book which we may well regard as the first-fruits of a renaissance in this country of the physiological branch of the science of botany; and the more so as the production of original text-books may be taken as one important indication of activity in the pursuit of the subject to which they relate. But while receiving this work of Dr. Vines with a hearty welcome, it is to be regretted that, as noticed in the preface, a considerable interval of time has unavoidably elapsed between the printing of the first sheets and the completion of the book: thus one great advantage of original production as opposed to translation, viz. that of being more nearly up to date, is in some measure lost in the present case, and it is to be hoped that the book will, as it well deserves, quickly run on to a second edition in which this defect may be remedied.

Those who have had the advantage of hearing Dr. Vines in the lecture-room would expect from him a clear style, and a skilful arrangement of the matter; and these are two of the most prominent characteristics of the work before us. Its scope is not that of a manual for mere beginners; it is rather constructed to meet the requirements of advanced students, and accordingly the author is freed from the *impedimenta* of external morphology and anatomy, a sufficient knowledge of which he presumes to have been acquired from other sources. But notwithstanding this presumption, the introductory lectures open in a manner easily followed by the uninitiated, and the whole could be read with advantage by any one who has become acquainted with the mere rudiments of vegetable anatomy.

The first three lectures are devoted to the structure and properties of the vegetable cell, including the osmotic and optical characters of its parts; this is preparatory to the study of the absorption of water, of the substances in solution in it, and of gases, which occupies the two following lectures, this subject being followed in fitting sequence by a discussion of the movements of water in plants, and transpiration. Lecture VIII. is devoted to the constituents of the food of plants, together with the more salient points as regards the functions of the several elements, and the sources from which they are obtained; in this, as in other parts of the book, a short historical sketch is given of the development of knowledge and opinion regarding each several function. Then follows the subject of "metabolism" in its widest sense, a broad distinction being drawn between constructive metabolism, or the building up of the organised structures of plants, and destructive metabolism, or the conversion of more complex substances into others of simpler composition. It is in the treatment of the constructive metabolism that the most striking novelty will be found by English readers, in the introduction of a view regarding the formation of non-nitrogenous organic substance, which, though propounded some years ago in Germany, has now we believe, appeared for the first time in an English text-book. While allowing that starch is the first *visible* product of the constructive processes, the question is asked whether the starch which appears in the chlorophyll corpuscles of a green plant under the influence of sunlight is *directly* connected with the decomposition of carbon dioxide which goes on in them? The answer is as follows (p. 145):—". . . according to Schmitz and Strasburger and in harmony with the older statements of Pringsheim, the cell-wall is produced by the actual conversion of a layer of protoplasm, and we shall see hereafter that the same is asserted of the layers of the starch-grains found in seeds, tubers, &c. Translating this into chemical language we find it to mean that molecules of protoplasm may undergo dissociation in such a way as to give rise to molecules of carbohydrate among other products. The conclusion to be drawn is, that the starch which is formed in chlorophyll corpuscles under the influence of light is also the product of such a dissociation of protoplasm." . . . Both here, and more definitely on p. 158, this point is accepted as proved, and is repeatedly referred to in the treatment of constructive metabolism. But, it will be asked, is it at all admissible thus to "translate" microscopical observations into chemical language? When it is remembered that we do not yet know the constitution of the molecule of protoplasm, that the protoplasm of a living cell is confessedly a most complex mixture, and that the observations quoted demand powers approaching the limits of microscopic observation, it would appear that this "translation" is little more than a figure of speech; that the process is *probably* as Dr. Vines describes it many will be found to admit, but it cannot be allowed that the evidence adduced by him is even a near approach to demonstration. This is not the only case of accepting a probability as a proved fact; thus on p. 174 we read:—"Seedlings, it is well known, contain considerable quantities of amides, and the presence of these can only be accounted for by regarding them as having been derived from the reserve proteids of the seed. It is then in the form of amides that nitrogenous organic substance is supplied to the seedling."

English readers will have become familiar with the view of Pfeffer and Draper that it is the yellow rays of the spectrum which are most efficient in the process of assimilation; and it will be a new idea to many that the balance of experimental evidence is rather in favour of the view of Lommel and others, more recently supported by the observations of Engelmann, that those rays which are absorbed by chlorophyll, viz. the red and violet rays, are the chief source of the energy which becomes latent in the process of formation of organic substance in green plants.

It is impossible within the limits of a short notice to take up more than these two points, but they will be sufficient to indicate that the part of the book which treats of metabolism contains much that is new to English readers both in view and in observation. At its close (p. 326) the results acquired are summarised in tabular form, constructed so as to appeal to the eye as a balance sheet, which takes account of income and expenditure of matter and energy, first in green, and then in colourless plants; this brings out clearly the conclusion that there is a nett balance in favour of the plant in either case, of both matter and energy.

The next section of the book (Lectures XV.–XXI.) opens with a description of the fundamental phenomena of growth, which is a clear statement of facts for the most part already familiar. This leads to a discussion, extending over four lectures, of the accompanying phenomena of irritability of growing organs, which result in their varied directive curvatures; two further lectures are devoted to the irritability of mature organs, considered in the light of the observations of Gardiner and others on the continuity of protoplasm; and the book closes with three lectures on reproduction; these include first an account of the chief types of both sexual and vegetative reproduction, and conclude with a discussion of the theories of sexuality of Strasburger, Naegeli, and Weismann.

With regard to the use of terms, two points demand notice: first, as to the words "dorsal" and "ventral," which have so often been the subject of discussion, especially because of the ambiguity arising from their different mode of application to leaves, and to dorsiventral shoots. But is it necessary to use the terms at all as applied to leaves? Will not the terms "anterior" and "posterior" convey the idea just as well, the terms "dorsal" and "ventral" being thus left free for application to dorsiventral shoots? Secondly, Dr Vines has not accepted the term "zygote" proposed by Dr. Strasburger as generally applicable to the fertilised ovum: this term is of use in avoiding the terms "zygospore" and "oospore," which, especially the latter, are often understood in an ambiguous sense.

To say that Dr. Vines's book is a most valuable addition to our own botanical literature is but a narrow meed of praise: it is a work which will take its place as cosmopolitan; no more clear and concise discussion of the difficult chemistry of metabolism in the plant has appeared, while the part which treats of irritability is an able digest of the voluminous, one might almost say inflated, literature on this branch of the science. In estimating the value of the book as a whole, we must bear in mind the circumstances in which physiological botany is at present placed. There is no branch of biological science upon which it is more difficult to write; our position with regard to the phenomena of vegetable life is throughout based rather upon a calculation of probabilities than upon clearly established facts; it is for each individual teacher in the exercise of his duty to draw a line between the discussion of views, and the acceptance for teaching purposes of points still *sub judice* as though they were established truths. Dr. Vines has gone rather further in the acceptance of probabilities than some will be prepared to follow him, and it is perhaps to be regretted that this should be the case in a book intended for the advanced rather than the elementary student. Placing this on one side, the book is one which must command admiration; a glance at the lists of references at the end of each lecture will give a clue to the extent of the literature which has been searched through; in erudition it stands alone among English books on the subject, and will compare favourably with any foreign competitors.

F. O. B.

A PLEA FOR THE RAIN-BAND

A Plea for the Rain-Band, and the Rain-Band Vindicated. By J. Rand Capron, F.R.A.S., and F.R.Met.S. (London: Edward Stanford, 1886.)

A NEAT little spectroscopic book, and furnished, as all such books should be, with a nice index, as well as not a few plates, which may be considered a second, or graphical, index of an instantaneous reference kind. But further it is both an honest, and a modest, production; for while it says nothing more on its title-page than what it fulfils, it has not cared to introduce there a compliment which it might have most legitimately claimed.

How often in literary history have not two words decided whether a book shall be bought and read, or not: these words being "second edition." But here they might have been exchanged for third, if not even fourth, edition, or "issue" at all events, for the date January 1886.

Mr. Rand Capron is evidently of a very practical order, and writes for practical men; and as he writes only of what he fully understands, and has abundantly worked at with his own eyes and hands,—he has the faculty of pleasing and satisfying those whom he addresses. This is testified to most particularly by the successive reprints of his first pamphlet within the short interval of five years; for though he was not the first and earliest rain-band writer, a public had to be created for the subject, and is evidently now rapidly increasing. This too notwithstanding that the feature wherein Mr. Capron's book is very strong, viz. numerical comparison of rain-band indications in the spectroscope, step by step with rain-gauge measures, or ozone papers, or hygrometrical readings of wet, and dry, bulb thermometers, forms by no means a smooth and easy-flowing kind of reading, as mere reading; however instructive it may be, and even necessary to have at hand to confront unreasoning objectors of an older school; endued often with imperfect senses, but all the more positive in their denunciations of a new departure in meteorology, on that very account.

If the poet is born, and is not to be manufactured by the tutors known in these days of cram as "coaches," so is it most assuredly with spectroscopic observers, when the subject to be observed is not the angular place of a sharp line, but the degree of intensity of a nebulous band of shade like the rain-band. Such intensity too to be determined, not by long and repeated observations with some grand photometrical apparatus mounted on a firm altazimuth stand, with tangent screw motions in every direction, but by a moment's look through a mere waistcoat pocket gem of an instrument held lightly between thumb and finger, and leading instantly to a judgment on the case, like a stroke of nothing less than pure genius.

Yet, by the marvellous aid of the prism with a narrow slit in front of it, there appear to be every year more and more persons who can accomplish the feat, and feel extraordinary satisfaction, even exhilaration, in the act of so doing. Wherefore after reading Mr. Capron's earlier pages, laying down what the rain-band, as seen in the spectroscope, really is, how it is to be observed, recorded, and concluded upon, the percentage of its correctness, and the kind of assistance it may afford to other methods of weather prediction in meteorology,—we have had still more pleasure in coming to his Part II., on "The Rain-Band Vindicated." For therein he describes succinctly the contests which have been recently going on in the meteorological world on the subject, and the rise of many new authors, either bringing in most varied experiences to show the truth of the principle, or still better publishing extensions of it. While from one of Professor Sir Henry Roscoe's earliest works on spectroscopy in general, and the telluric additions to the lines of the solar spectrum in particular,—is extracted this paragraph, which deserves to live.

"No one can tell what secrets lie hid in these atmospheric lines, but to us it seems that by their careful and systematic observation, 'the Message from the Stars' which has taught us so much, may be rivalled in practical importance by a 'Message from the Sky.'"

And the harvest to be gathered is still on the increase; for since the appearance of Mr. Capron's last edition, a new observer in unusually exalted circumstances of temperature, sunshine, and moisture (viz. Mr. Maxwell Hall, in Jamaica), almost at once discovered another rain-band, not in the red, but in the green of the spectrum; and as super-excellent for prediction-use in that tropical island, as our D rain-band in the red is to ourselves at home. What wonder, then, that so able a physicist and astronomer writes, and with such hope and joy too of soon having more leisure to devote to science,—writes, we repeat, that although he has not yet settled the exact line of research he will devote himself to,—it must be "something spectroscopic."

Notwithstanding too that, as yet, the rain-band spectroscope has only been employed by day, in noting the dark, or so-called Fraunhofer, lines and bands on the bright continuous spectrum of the sun-illumined clouds or sky,—there seems a new utilisation of it opening up in detecting aurora, when otherwise invisible, by its unique bright citron line in a dark field at night; and thereby affording men another kind of rainfall prediction, even so much as forty-eight hours beforehand.

In conclusion, though not exactly touching on rain-band, we should call attention to Mr. Capron's appendix, descriptive of his well-arranged and successfully carried out observations on atmospheric electricity, as likely to lead eventually to something practical and exceedingly important. For, as M. Gaston Planté has long held in Paris, he has never yet known a storm of wind which was not accompanied by measurable disturbances of electricity; and with indications that the whole quantity of that fluid, lying latent in the earth, is a store of almost unimaginably large quantity, derived from the Creation Age, and only very slowly escaping; while man is still merely looking on, and unable to turn it to any useful account.

C. P. S.

OUR BOOK SHELF

A Manual of Surgery. In Treatises by various Authors. 3 vols. Edited by Frederick Treves, F.R.C.S. (London: Cassell and Co., 1886.)

MESSRS. CASSELL, in issuing these volumes among their manuals for students of medicine, did wisely in invoking the aid of some thirty hospital surgeons, who have in these three handy volumes produced a very practical work of high excellence.

In comparing such a work as the present with a book on surgery written fifteen or even ten years ago, we are at once struck, on the one hand, by the number of new operations which have been introduced, mainly owing to antiseptic surgery; and, on the other, by the much greater definiteness and accuracy with which diseases and lesions are defined and differentiated from one another. As a consequence, the material is so extensive in amount that operative surgery and pathology will occupy additional volumes.

The relations of micro-organisms to septicæmia, pyæmia, and the treatment of wounds, receive full discussion, extending over several chapters. There is a valuable chapter by Mr. Mills, Anæsthetist to St. Bartholomew's Hospital, on the production of anæsthesia and the means of dealing with the difficulties that may occur.

In the discussion of knee-joint disease a much more favourable view of the benefit of rest is taken than would accord with our experience, and it is stated that with the application of splints the great majority of cases will end in complete recovery in six to nine months. This result, however, is surely uncommon, and too often the pulpy mischief progresses until, after months or years of rest, the patient is able to get about again with a limb liable to lay him up after the slightest exertion, or it has ultimately to be amputated. On the other hand, the permanent good results which are obtained by excision of the knee are much under-estimated, and, instead of falling more and more into disuse, the operation will in the future often be the means of saving limbs that are now amputated, especially when the excellent results that can be shown for a long series of cases have been published.

Abdominal surgery receives ample notice, and in no department during the last ten years has greater progress been made; many injuries and diseases which were formerly necessarily fatal are now amenable to operation. Continental surgeons, able to perform trial operations on animals, are far more successful in their operations on the intestines than we are, and every year human lives are offered up as a holocaust to the fanaticism of the anti-vivisectionists. It is to the physiologists that we are indebted for the elaboration of the various steps by which success is now achieved both in these operations and in those on the brain.

The general excellence of the illustrations, which number 200, is worthy of note; and while many are original, not a few have been selected from other books. There is no doubt that each year it becomes more easy to obtain typical illustrations of disease. We would therefore take exception to the illustrations of the teeth of congenital syphilis, of myxœdema, and of single hare-lip, of which more characteristic examples might have been taken.

The handy form of the volumes, as well as the practical nature of the book, will insure its popularity among students.

L'Évolution et la Vie. Par Denys Cochin. (Paris: Masson, 1886.)

This work, which is a *réchauffé* of the ordinary facts of digestion as given in the text-books, and of the relations of micro-organisms to vital processes, and more especially of Pasteur's work on the subject, must have been written chiefly for the author's amusement. It opens with a pro-

test against Herbert Spencer's application of the principles of evolution to the solution of vital, social, and mental problems. The author then proceeds to set up a ghost founded on the statement made some years ago, that "there is no evolution without spontaneous generation." To refute the theory of spontaneous generation will be, he says, to give a direct blow to the theory of evolution. This, he maintains, has been amply done by Pasteur and others, and a number of the most important experiments are here referred to.

The author proceeds to argue that, since evolution has failed to explain the first beginnings of life, there must have been a God who created matter, a living germ, and an intelligent mind, and that the three creations were distinct.

He gives a clear account of many of the vital as distinct from the non-vital processes, and draws especial attention to the fact that solutions of many of the higher organised products polarise light, and that the only organic bodies which have been formed synthetically are the lower organised products which do not polarise light. It is doubtful, however, whether the distinction is one which will hold much longer, as chemical methods are constantly improving.

The author adduces no new facts, but he has the merit of bringing together in a very readable form, statements more or less scattered about in several books and periodicals.

History of the Royal College of Surgeons in Ireland, &c. By Sir Chas. A. Cameron. (Dublin: Fannin and Co., 1886.)

THIS volume, which is published at the expense and by the authority of the College of Surgeons, collects together the charters and histories of the various Irish Medical Schools and Colleges, and supplies biographies of the leading members of the medical profession in Ireland, together with a list of their works.

Many curious ana are given of the old physicians; among others, of Joseph Rogers (1734), one of the first to feed fevers, who gave a patient daily for a month four to six quarts of sack-whey and two quarts of mulled canary, which was certainly vigorous treatment.

The first Society for the regulation of medicine in Ireland dates back to 1446, when Henry VI. established a Guild of Barbers in Dublin; and later on, in 1572, Queen Elizabeth granted a new charter by which women were admissible to the guild; and in those days a barber was equivalent to our surgeon. This Society lingered on until the foundation of the College of Surgeons in 1784.

This book will be of great use as a work of reference with regard to the state of medicine at any period in Ireland, and its compilation must have been a laborious labour of love on the part of the author. The biographies, which are very numerous, form the most interesting part of the work, and include a large number of world-renowned names, the greatest of which are probably Graves and Stokes.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Physiological Selection and the Origin of Species

As I was unable to be present at the Linnean Society when Mr. Romanes read his paper on the above subject, I may take the opportunity furnished by the publication of the abstract in these columns to put forward certain views which I have long held with reference to the points rai-ed by the author. I may remark that I am writing under the disadvantage of distance from notes or books of reference, and that I have not yet seen the complete paper. Moreover, my work of late years has run off biological tracks, and I can but regret that my remarks must, under the present circumstances, be of a more or less general character; but at any rate they may be of use as a contribution to the discussion which Mr. Romanes' carefully considered paper well merits at the hands of biologists.

In the first place, I should like to point out that evolution by what Mr. Romanes calls "independent variation," or the prevention of crossing with parent forms, is very ably discussed by Weismann in one of his earlier works, "Ueber den Einfluss der Isolirung auf der Artbildung" (1872), which essay I can commend to the notice of all interested in the subject. Weismann termed this principle "*Amixie*," and for want of a better word I have rendered this "Amixia" in my edition of the "Studies in the Theory of Descent," in which work the principle is also frequently alluded to.

All evolutionists will agree with Mr. Romanes that natural selection *per se* is incompetent to account for the origin of species. This has long been admitted by naturalists, and Darwin himself in later life frankly acknowledged that in the early editions of the "Origin of Species" he over-estimated the power of this agency. Nevertheless, Darwin to the last considered natural selection as the *chief agency* in the evolution of species, and no one saw more clearly than he did the difficulties which surrounded the formation of incipient species, owing to the obliteration of new characters by intercrossing with the parent form. The sterility of natural species as compared with the fertility of domesticated races is also a difficulty which Darwin fully recognised and did much towards meeting. The results of his investigations in this direction have been to break down the supposed fixity of the rule, although it must be admitted that the broad fact still remains, and we cannot but be grateful to Mr. Romanes for once more emphasising this difficulty with his characteristic clearness. It is chiefly—if not entirely—with the object of meeting this difficulty that "physiological selection" has been conceived, because, as it appears to me, the other difficulties referred to by Mr. Romanes, viz. those connected with the prevention of intercrossing and the inutility of trivial characters, are quite subordinate to this main difficulty, and need not be further considered until the admissibility (or otherwise) of physiological selection has been settled. The questions now to be decided are whether natural selection + sexual selection + correlated variability + amixia + use and disuse, &c., is really a theory of the origin of *species*, or whether these factors have been only made to "pose" as such? Is "physiological selection" competent to account for the origin of species?

If I interpret Mr. Romanes correctly, his theory is equivalent to the admission that amixia may become inter-racial, *i.e.* that it may arise among the individuals of a species without the intervention of physical barriers by the spontaneous origination of a physiological barrier, *i.e.* by variation in the reproductive capacity. That such a form of variation may exist I have long been willing to admit, and I do so now with all the more readiness in face of the arguments so skilfully marshalled by the author of the new theory. But, since Mr. Romanes admits the efficiency of natural selection, the question seems to resolve itself into this: Can physiological selection work independently of natural selection? If not, natural selection must still be regarded as a prime factor, and if physiological selection cannot originate a species independently of the control of natural selection, surely the latter, with its subordinate factors (of which physiological selection *may be* one), is still *the* chief element in the theory of the origin of species.

Let us suppose, for the sake of argument, that among the individuals of a species there arise certain varieties which are fertile *inter se*, but sterile with the parent form. There would thus arise a new race which could not be swamped by intercrossing with the predominant form, and the one species would practically be resolved into two—the parent form being still in the ascendency as regards numbers. But the competition is always most severe between the most closely related forms, and unless the new form (arising by inter-racial amixia) possessed some distinct advantage over the old one, it would as surely be exterminated by the overwhelming majority of the parent type as it would be by intercrossing in the absence of amixia. Physiological selection thus appears to me to be as subordinate to

natural selection as sexual selection, correlated variability, the law of homology, or any other of the Darwinian factors. The expression used by Mr. Romanes for his new factor—the "Segregation of the Fit"—seems to imply fitness for something, presumably for the conditions of life, and if the survival of the "fit" race is determined by natural selection, then I venture to think that natural selection must still be regarded as *the* theory of the origin of species and as something more than a theory of the origin of adaptations.

To the foregoing Mr. Romanes will probably say that physiological selection is a *necessary* adjunct of natural selection, and that no new species can arise without the co-operation of his factor. If this be the case, then, bearing in mind the views which I have expressed with reference to the subordination of physiological to natural selection, it seems to me that the most likely course to pursue is to appeal to the latter for an explanation of the "primary" specific character, viz. sterility. It is true that Darwin and many of his followers have attempted in vain to account for this primary specific distinction by natural selection, but I still venture to think that the solution lies in this direction. Indeed, the elements of the solution appear to me to be furnished by the original theory of Darwin and Wallace, as I will now briefly attempt to show, hoping to elaborate the idea on some future occasion, or, still better, leaving it for development or extermination in the hands of professed biologists.

The struggle for life being the most severe between the most closely allied forms, diversity is in itself an advantage, because the individuals which depart from the parent type may (but not necessarily *must*) "seize on many and widely diversified places in the economy of nature, and so be enabled to increase in numbers." Hence Darwin's principle of "divergence of character," so well restated by Mr. Romanes in his paper. Now, if diversity is an advantage, natural selection can deal with it like any other advantageous character, and *would seize upon any means afforded to secure its perpetuation*, provided always that the divergence was in the direction of some unoccupied "place in the economy of nature." This last condition amounts to nothing more than that the divergence is to be of an advantageous character. Among the most effectual means of securing permanent diversity must be sterility with the parent form: hence, given a variability in the reproductive capacities of different pairs of individuals (which I have already conceded), the question is whether natural selection could not develop out of this more or less imperfect fertility a more or less complete sterility. If it is to the advantage of some particular variety not to resemble its parent form, out of the immense number of divergences which are always taking place (by ordinary variation) those varieties which showed the greatest infertility with the parent form would in the long run survive, for the very reason that their progeny, tending to inherit the characters of their parents, would possess the advantageous characters of the latter, which led to their survival in the first instance, and, among these characters, that diminished fertility with the parent form which lessens the chance of their extermination by intercrossing.

As the foregoing argument is necessarily expressed in general terms, it will be of use to specialise our ideas by an appeal to a hypothetical case. Suppose, for instance, that a dominant species gives rise to the twelve varieties A, B, C, ... L, out of which four, B, D, K, L, possess some slight advantage over the parent form, adapting them to some new conditions in their environment. The four varieties thus stand a chance of surviving, while the eight others would be the "unfit." Now these four varieties in their incipient stage (and in the absence of isolation) would be subject to extermination by intercrossing in the next generation with the parent form. But the chances against these four varieties being *equally fertile* with the parent form must be exceedingly great: let us suppose, therefore, that B and K are less fertile with the parent form than D and L. Under these circumstances the latter would be wiped out by intercrossing, while the former would tend to retain their peculiarities and thus survive. The peculiarities both of B, K, and D, L, were originally advantageous, but those of B, K, are alone allowed to survive. The parent species has, as it were, attempted to give rise to four derived species, and has only succeeded in producing two. It is a case of selecting out of a number of advantageous modifications those particular varieties which are the least fertile with the parent form. From the slight sterility thus produced in the initial stages, natural selection, by acting in the same direction, might evolve the more or less perfect sterility which we now behold, because every departure on the part of the derived form in the direction of fertility with the parent form would be a retrograde step tending to obliterate those advantageous characters which led to the first survival of the new form, and the descendants of such partially fertile departures would constantly be weeded out owing to the dilution of their peculiarities with the less advantageous characters of the parent form. To put the case in another way, it may be said that natural selection is constantly endeavouring to develop the most advantageous modifications of every species, but succeeds the better the less the degree of fertility of the advantageous modification with the parent form, and succeeds only perfectly by producing perfect sterility with the parent form. Fertile advantageous modifications, on the other hand, would be swamped by absorption into the parent form.

I thus venture to think that the theory of natural selection as sketched out by Darwin and Wallace is still a theory of the origin of species. The production of the sterility of species by this agency is, according to the present views, to be referred to the same causes as all the other modifications produced thereby, viz. the natural selection of a "spontaneously" occurring variation in the function of one particular organ. In the case of domesticated races no such selection with reference to the functions of the reproductive system has been effected, but the varieties have only been kept from interbreeding by what amounts to isolation. It is not surprising, therefore, that such artificial forms, which have been selected only with reference to *external characters*, should be fertile *inter se*, while natural species, in which fertility *inter se* has been rigorously suppressed by natural selection through long series of generations, should exhibit a greater or less degree of sterility.[1] In other words, "physiological" appears as one particular phase of natural selection, and as far as we can see there is no reason why there should not be other modes of variation leading to the same result by acting indirectly upon the reproductive system. But all such modes of variation would still be subject to development or suppression by natural selection. R. MELDOLA

Greenock, August 21

THE Duke of Argyll's letter about organic evolution, published in your last week's issue (p. 335), calls for a few remarks, as it is very misleading, and bespeaks some misconceptions on the part of the writer. He has evidently read his own views into the two articles on organic evolution contributed by Spencer to the April and May numbers of the *Nineteenth Century*. In those articles Spencer makes no "admission"; what he says there with respect to natural selection has been held by him for the last twenty-six years. He does not deny that the natural operations denoted by natural selection do constitute an operating cause in the evolution of species. Only, he goes deeper: he, with his characteristic truly philosophic insight, sees in natural selection a *proximate* cause; sees behind it the primordial operations of forces of nature which rendered natural selection possible, and supplied it with a *point-d'appui*. Then he assigns use and disuse as another cause in the origination of species. Now all this is not a "declaration against" what your correspondent pleases to call "Mechanical Philosophy," but is a part and parcel of it. It is rather a "declaration against" all sorts of teleological philosophy. Let him remember also that Spencer's philosophy is the acknowledged philosophy of evolution; and he may rest assured that, even if the theory of natural selection *as a cause* in the genesis of species be proved untrue, that philosophy will still stand opposed to any philosophy that will attempt to bring back "Mind" as one of the *causes* of organic evolution.

Your correspondent is a little too hasty in his rejoicings over Mr. Romanes' paper on "Physiological Selection." He will see from the s cond part of the paper that even Mr. Romanes is unable to deny that in some cases at least natural selection is quite competent to originate species.

Then your correspondent thinks the theory of natural selection "not only essentially faulty and incomplete, but fundamentally erroneous," "in so far as it assumes variations to arise by accident." Now by "accident" or "chance" in this connection, evolutionists (including Darwin) have simply meant the action

[1] From the above it follows that local races or species produced by isolation should be more or less fertile with the parent form. This is a point which might well be tested experimentally.

of some bidden physical causes whose exact mode of operation is not known. We all know, however, that variations are *facts* of nature, and it is not difficult to see that they are the necessary consequences of the varying number, amount, proportion, and manner of action, of the natural forces acting on different portions of living matter. Now, in making variations the starting-point, the theory of natural selection may justly be considered to be "incomplete," even as our knowledge of electricity is incomplete because we do not know the real nature of the thing, as astronomy is incomplete because we do not know for certain how, for instance, the solar system was formed; but in so doing the theory cannot be "essentially faulty" or "fundamentally erroneous." It is illogical, not to say childish, to think a theory to be erroneous because it cannot render a definite explanation of some unquestionable facts of nature on which it is based and with which it starts. To prove the theory of natural selection to be erroneous, it must be shown that it is never competent to originate species. If it ever falls, it will fall quite *ir*respectively of its avowed inability to give definite explanations as to the exact mode of occurrence of variations. S. B. MITRA

19, Keppel Street, Russell Square, London, August 17

Red Sunsets and Volcanic Eruptions

PROF. S. NEWCOMB'S article on the above subject in NATURE of August 12 (p. 340) induces me to send you a brief account of the atmospheric phenomena that I observed in Palermo during and after the recent eruption of Mount Etna.

This volcano is 133 kilometres distant from Palermo, but the transparency of the air here is so great that it is almost always visible from this Observatory.

At dawn on May 21 the smoke from the eruption appeared as a great mass of black vapour, rising from the southern side of the volcano. At 11 a.m. it had formed into rosy balls of vapour, or cumuli. With the theodolite I measured the angular height—2° 21′, which gives 8 kilometres of linear altitude. On May 24 the smoke had the characteristic form of a pine-tree, and a greater height, but at 4 p.m. the upper edge of it was not well defined, and I obtained (approximately) the altitude as 14 kilometres.

Since May 22 these vapours from Etna have spread over the eastern, and more recently over the entire, horizon of Palermo. In the early morning of June 3 the fog was so dense that the sun was invisible, and the towers of the Matrice, 200 metres distant, were only indistinctly visible, which in Palermo is quite extraordinary. From May 29 to June 3 Italy has been invaded from south to north with mist, which was probably also derived from Etna.

Cinders from the volcano have fallen over Eastern and Southern Sicily, and over Calabria, as well as in Palermo, where, in the dust gathered on the terraces of the Observatory on May 27, I detected with the microscope some minute crystalline laminæ of labradorite, which mineral is characteristic of the ejections of Etna.

The sun rising from the sea behind these mists has been purple-red and then reddish-yellow; at a height of about 30° it was neutral gray, but never green or blue. In Nicolosi, too, on the side of the volcano, these colours of the sun have not been observed. The light of the red sun was so faint that it was possible to look at it without inconvenience.

No corona (like "Bishop's ring") was observed around the sun or moon. Spectroscopic observations of the red sun gave only the ordinary atmospheric absorptions, perhaps somewhat intensified.

In the latter part of May and during the month of June the red after-glows appeared almost daily, and were stronger than before or since, but they were not so brilliant and prolonged as in 1831 and 1883–84, and their colour was not properly rosy, but an impure reddish-yellow.

I believe that the red sun was caused by the finest cinders from the volcano, suspended in the air, as the like phenomenon is produced by the dust of the Höherauch in Northern Europe, of the Sirocco in Sicily, and of the Kamsin in Africa. The blue sun (observed after the eruptions of Ferdinandea and of Krakatão) has not appeared, and the after-glows were not strongly brilliant, because the vapours ejected from Etna were not so enormously abundant as those ejected from Ferdinandea and Krakatão, which are marine volcanoes more directly communicating with the water of the sea.

The observation by M. Janssen, mentioned in NATURE of July 29 (p. 299), of a blood-red coloration of light traversing dust, gives a strong confirmation to the preceding explanation of the red sun. A. RICCO

Palermo Observatory, August 17

P.S.—Since July Bishop's ring has not been visible in Palermo.

The Bright Clouds and the Aurora

ON the morning of the 11th inst. I had an opportunity of watching the curious cirrus-like clouds as daylight came on. The display was striking, though not such a bright one as on several former occasions. It first appeared about 2.30 a.m., when there were very faint indications of the clouds; it was some minutes before I noticed that they were the same brilliant kind as has appeared so often this summer. Their apparent upper border being irregular, it was uncertain whether they in any part reached the limit to which the sun could shine upon them, or whether the apparent border was altogether the actual edge of the cloud-sheet; however, it rose higher as the sun approached the horizon, but this might be owing to the circumstance that the motion of the clouds was, as usual, from an easterly direction. At 3.33½ a.m. they were visible as far as α Andromedæ, though they were very faint west of γ. By 3.45½ a.m. they reached down to within 5° of α Aquilæ, and were rather plain there, and by this time the sheet covered most of the sky, though none of it remained visible very low down in the east. It was no longer bright in any part. At 3.55½ they reached down to within 4° of α Aquilæ, and were plainest about there, but growing fainter. I was still uncertain whether the sheet extended beyond the western apparent border, that being simply the limit of sunshine, or whether the sheet ended there; but probably the former was now the case. At 4 a.m. they were scarcely noticeable, and by 4.11 they had disappeared altogether. By this time a faint pink glow had appeared in the east.

The question is, Was the disappearance of the clouds due to their having evaporated, and ceased to exist, or to their light being overpowered by the brightness of the sky? It appeared to me that the latter was the case. It will be well if further observers can confirm this supposition or otherwise; if correct, they cannot be considered clouds at all in the ordinary sense, the sky being beautifully clear and blue after they had ceased to be visible. I could not say at any time that the clouds were not perfectly transparent to the stars. The circumstance that they have never been described as having been seen by day seems confirmatory of the above supposition.

With respect to Prof. Smyth's remark about the spectrum (p. 311) I do not gather whether he considers that the auroral line noticed by him belonged at all to the clouds or entirely to the aurora; but I think that there can be no doubt that the latter was really the case. He does not seem to have detected any aurora at the spot where the clouds were seen, but doubtless it was there, although overpowered by their brightness. As it is so evident that these clouds were illuminated by the sun (this being confirmed by their varied colours depending on their altitude, as described by Prof. Smyth), we cannot expect their spectrum to be otherwise than solar and atmospheric. I looked at them with a miniature spectroscope on the evening of July 12, as well as on the morning of the 11th inst., but on neither occasion was the spectrum bright enough for me to perceive much. I could not see any lines, bright or dark, but the spectrum faded very abruptly in passing from green to orange, which no doubt was owing to the atmospheric bands near D, especially the "low sun band."

On July 27 I saw the aurora mentioned by Prof. Smyth at Gilsland, in Cumberland, and it was a particularly magnificent one there, especially about 10.25 p.m., at which time a part of it was lilac—a very unusual colour. The bright clouds were also visible that night, but chiefly before the aurora appeared and after it vanished; there appears no reason to suppose there is any connection between the two phenomena.

As regards the dark space beneath the auroral arch, has the theory mentioned by Prof. Smyth ever been proved, that there is any true darkness there, and that it is not merely the effect of contrast with the aurora? My impression is that it must be at least mainly the effect of contrast, though perhaps not entirely, and the idea is confirmed by a similar darkness sometimes appearing by contrast with the brilliant clouds, when no

aurora is present; the stars shine quite bright in this dark sky above them.

Prof. Smyth considers that the night after the aurora of the 27th the twilight extended over the region "aurora-blackened" the evening before. Would not this be owing to the brightness of the aurora preventing the twilight from being seen so high then simply by contrast? The fact that the dark sky was luminous in the spectroscope seems to bear out this.

I do not understand Prof. Smyth's suggestion why these clouds should never be seen in winter, for any night in the year there is a time when the sun is at the same distance below the horizon as it is when the bright clouds are well seen.

Sunderland, August 18 T. W. BACKHOUSE

Cloud Effect

A VERY unusual cloud effect was noticed here on the 18th inst. at 7.45 a.m. The whole sky, especially to the east or south-east, was at that time covered with a widespread field of mackerel cloud. This field was cut from north to south with a strongly defined cleft or narrow line showing the blue sky beneath. It was like a crack in the cloudy tissue, and formed a perfect arch, whose greatest altitude was not many degrees above the sun's apparent place. It lasted nearly half an hour. There was little wind at the time, only a slow motion from the north, but a change took place shortly after, when it veered to the south-west. E. BROWN

Further Barton, Cirencester, August 20

The Crag Deposits on the North Downs

To students of Tertiary geology, the interest of Mr. Clement Reid's verification of Prof. Prestwich's judgment of many years ago as to the Pliocene age of certain outlying deposits at Lenham is so great that I must crave permission for space for a line or two with reference to other similarly situated deposits on the North Downs, which have been described as belonging to an horizon "somewhere between the Chalk and the moon." The deposits to which I refer were described by Prof. Prestwich in the *Q.J.G.S.*, vol. xiv., and of his paper Mr. Whitaker made free use in preparing the account of these outliers in vol. iv. of the "Memoirs of the Geological Survey" (pp. 336 42). The idea has been for some time growing up in my own mind, with reference to these unfossiliferous outliers, that some of them will have to be recognised as remnants of the once more widely extended Upper Bagshot Sands. This conclusion is at present based mainly on three facts: (1) the literal application of Prof. Prestwich's description of their lithological character to portions of those beds; (2) the occurrence of "similar beds on the Chalk Downs on the opposite side of the Channel, between Calais and Boulogne"; (3) the superposition of "analogous strata" on the top of Cassell Hill in French Flanders upon the *Calcaire grossier* series, the equivalent of our Middle Bagshot (so-called Bracklesham) Beds. I hope to deal with this more at length during the next session of the Geological Society, and only draw attention now to the suggestion which I threw out several years ago (*Proceedings* of the Geological Association, vol. viii. p. 170) for reasons assigned, that the oldest plateau-gravels of the London Basin are probably of Pliocene age. This may possibly have escaped Mr. C. Reid's notice. A. IRVING

Wellington College, Berks, August 17

Actinotrocha on the British Coasts

IN answer to Mr. Cunningham's letter on the distribution of Tornaria and Actinotrocha, I may state that I took Actinotrocha in the tow-net at the mouth of this bay on July 31. I believe I have found it more than once before on the west coast during the last few years, but, not having my note-books with me, I cannot say definitely where and when. If I am not mistaken, *Phoronis* was found by Dr. Strethill Wright in the Firth of Forth, and is therefore known as a British animal.

Loch Ranza, Arran, August 21 W. A. HERDMAN

GEORGE BUSK, F.R.S.

A SINGLE-MINDED, true-hearted man, a warm friend, and an able and accomplished naturalist, has just passed away from the midst of his family, his friends, and his fellow-workers.

George Busk was the second son of Mr. Robert Busk, of St. Petersburg. He was born in 1807, and at an early age gave promise of those tastes and of that aptitude for research which, developing with his years, gained for him the high position which he was destined to hold among the scientific workers of his time.

After completing his medical education he was appointed surgeon to the seamen's hospital-ship *Dreadnought*, a post which he continued to hold for about twenty-five years. It is these twenty-five years which constitute the strictly professional period of his life, and which gained for him a place among the most distinguished members of his profession as an able, clear-sighted, and enlightened surgeon.

In 1856 he resigned his appointment to the *Dreadnought*, and at the same time decided on retiring from professional practice and on devoting himself to scientific work.

Having now leisure for the cultivation of those studies which were always dear to him, he threw himself warmly into biological work. An excellent and cautious observer, it was chiefly to researches on the structure of the lower members of the organic world that he now devoted himself, and scarcely a month passed without the periodical literature of biology receiving from his labours the record of some new and interesting fact.

About this time he became one of the editors of the *Microscopical Journal*, and the numerous communications which appeared from his pen in the pages of that periodical contributed largely to its popularity and success.

There were few departments of biological science which Busk did not enrich by his researches, and we now find following one another in rapid succession a long series of papers containing the results of his studies among the lower groups of the animal and vegetable kingdoms. He was a skilful microscopist, an acute and conscientious interpreter of the optical expressions of organic form presented by the microscope to the observer, and his contributions to the transactions of our leading scientific societies and to various natural history journals have advanced our knowledge of some of the simple unicellular plants, of the Infusoria, the Hydroida, the lower Vermes, and above all of the Polyzoa, to an extent which those who have worked in the same fields can fully appreciate.

In 1856 appeared his article "Polyzoa" in the English Cyclopædia. In this admirable article we have an exhaustive account of the structure of the Polyzoa, while it contains the first satisfactory attempt at a scientific arrangement of the group, and proposes for the first time the employment of certain systematic characters which are now universally accepted as offering the only legitimate bases of a philosophical classification.

Soon after this he undertook the labour of drawing up an illustrated descriptive catalogue of the Polyzoa contained in the collection of the British Museum, and brought to bear on the descriptions and systematic arrangement of the species those principles whose soundness he had already established. There was thus placed in the hands of the student a work of great value, with which no investigator of the group can afford to dispense.

On the return of H.M.S. *Rattlesnake* from its explorations in the Australian seas under Capt. Owen Stanley, the collections of Polyzoa and Hydroids made during the voyage were placed in Mr. Busk's hands for examination and description. His report on the new species thus obtained is published in the narrative of the voyage, and forms an important addition to our knowledge of these animals.

Among the facts of anatomical interest which have been successfully worked out by Busk in the organisation of the Polyzoa, his demonstration of the structure of the *avicularia* and *vibracula* deserves special mention. He has given by far the best account which had been hitherto

published of the structure and functions of these remarkable and enigmatical bodies, while he insists on their value in affording characters for classification. His very instructive and expressive figures form part of the illustrations of Polyzoal morphology contained in the morphological atlas of Victor Carus.

It was about this time that Busk undertook, for the Palæontographical Society, a monograph of the fossil Polyzoa of the Crag,—a task for which his knowledge of the recent species had eminently fitted him. But his geological work was by no means confined to researches among these lower forms of life. In 1864 he made a journey to Gibraltar along with Dr. Falconer, for the purpose of investigating the ancient fauna which had been preserved in the caves of that region. The results of the joint labours of the two explorers were embodied in a report read at the Norwich Meeting of the British Association in 1868, and more fully in a complete monograph on the subject subsequently published. Among other palæontological contributions may be mentioned his observations on certain points in the dentition of fossil bears, as affording good diagnostic characters, and on the relations of *Ursus priscus* to *Ursus ferox*; also his descriptions of three extinct species of elephant, the remains of which were collected by Capt. Sprat in the ossiferous caverns of Zebbung in the Island of Malta; his report on the animal remains in the Brixham Cave; and a report on the animal remains found by Col. Lane-Fox in the High and Low Terrace-gravels at Acton and Turnham Green. All these communications bear evidence of his skill in recognising palæontological characters and in detecting their relations with those of living forms, while his study of fossil mammals, and his comparison of these with existing species, suggested to him an ingenious method of graphically representing the dimensions and proportions of mammalian teeth.

It was somewhat later than this that his attention was largely given to ethnology, and the Anthropological Society not only owes to his pen many valuable memoirs, but bears evidence of judicious management and administrative capacity in his labours as its President and as a member of its Council. Along with Dr. Carpenter and Dr. Falconer he formed one of a Commission which visited France in order to take part in the Conference which was held there for the purpose of inquiring into the circumstances attending the asserted discovery of a human jaw in the Gravel at Moulin Quignon, near Abbeville. Among his other anthropological work will be found many interesting comparisons of crania belonging to various nations. These investigations were carried on chiefly by means of a systematic method of measurement, which he advocated as affording a uniform basis of comparison, by which anthropological studies might be facilitated and the data of comparison rendered more definite and precise.

At a time when the German language was much less understood in this country than it is at present, Busk performed an important service by giving to the English student an excellent translation from the German of Steenstrup's famous treatise on the alternation of generations, and, in collaboration with Huxley, a translation of Kolliker's valuable manual of human histology.

The last piece of work which devolved on him was the preparation of a Report on the Polyzoa collected during the voyage of the *Challenger*. The first part of this important work was completed in 1884, and has been already reviewed in NATURE. It forms an admirable exposition of the additions made to our knowledge of these animals by the great exploratory voyage; and amply realises all that had been expected from one who had made the Polyzoa the subject of so much careful and philosophic study.

The second and concluding part of the Report he left behind him in a condition nearly ready for the press, and under the judicious supervision of the proofs by his eldest daughter—through whose loving care during his last months of suffering he was enabled to carry on his work to completion—is now quite ready for publication.

The many-sidedness of Mr. Busk's mind was one of the most striking features of his clear and comprehensive intellect, and naturally obtained for him distinctions and honours in many and various departments of science. He was early elected a Fellow of the Royal Society, of which he was afterwards nominated one of the Vice-Presidents, and on the Council of which he served on several occasions. He was more than once President of the Microscopical and Anthropological Societies, was Zoological Secretary of the Linnean Society, and would have been made its President were it not that, notwithstanding the warmly expressed solicitations of the Council of that body, he felt that the labour of the Presidential chair was greater than he believed himself justified in undertaking.

In recognition of the eminence he had attained as a surgeon during the professional period of his life, and of the interest he had always continued to take in the welfare of his profession, he was elected in 1871 to the Presidency of the Royal College of Surgeons. He was one of the Trustees of the Hunterian Museum of the College, and continued for three years to hold in connection with that Museum the Hunterian Professorship of Comparative Anatomy. He was a Member of the Senate of the University of London, for many years Treasurer of the Royal Institution of Great Britain, and had more recently been nominated one of the Governors of Charterhouse School.

For his researches in zoology, physiology, and comparative anatomy the Royal Society in 1871 awarded to him the Royal Medal, while for his palæontological researches he afterwards received from the Geological Society the Lyell and Wollaston Medals.

On the passing of the Cruelty to Animals Act, intended to regulate the performance of experiments on living animals, he was appointed by Government inspector of the various medical schools and physiological laboratories registered under that Act in England and Scotland; and the judgment and skill with which he performed the difficult duties of the office bear ample testimony to the wisdom of his appointment. Abhorring the infliction of unnecessary pain, he saw that for the advancement of knowledge which might tend to the alleviation of human suffering such experiments were not only permissible but called for, while at the same time he set himself strenuously against the infliction of pain which might be avoided, and against the institution of experiments which did not hold out obvious promise of the results which alone would justify them.

He was a genuine lover of Nature, deriving unalloyed pleasure from all that was beautiful in the external world; and the writer of this notice can well remember the enthusiasm with which he would recall the vegetation of the lower reaches of the Thames—amid which his early work on board the *Dreadnought* lay—with its rich growth of Sagittaria, and Butomus, and Sedges, and picturesque water-weeds, long since swept away before the spread of manufactures and the encroachments of civilisation.

Generous and liberal to his fellow-workers, with his rich store of material always at their disposition, his loss will be long and deeply felt by the many who profited by his friendship. Free from all selfish and personal ambition, and pursuing his investigations for the sake alone of the truths which might result from them, he cared little about asserting his claims to discovery, and would rest satisfied with the belief that, whoever may be the discoverer, human knowledge would be the gainer.

And yet, though he had no ambitious longing for reputation, Busk was no cynic. He could appreciate the esteem of those whose esteem was worth having, and few

men had a larger number of genuine admirers, or gathered around them a wider circle of sincere and attached friends. And not alone to the fields in which he himself worked did he extend his interest and sympathies. Amid the labourers in very different departments of thought he found some of his most cherished friends—frequent and always welcome guests at his hospitable home. For these, and for all who had enjoyed the privilege of his friendship, the sorrow at his loss will be softened by the ennobling memory of his life.

GEO. J. ALLMAN

BRITISH ASSOCIATION, SECTION B: DISCUSSION ON THE NATURE OF SOLUTION

IT may perhaps be convenient to those chemists who have announced their intention of joining in the proposed debate in Section B, at the approaching meeting of the British Association, that, having accepted the invitation of the President to open the discussion, I should indicate briefly the general nature of the subjects upon which I shall offer some remarks, and the order in which I shall probably take them.

After an historical sketch of the theories which have been framed with the object of explaining the constitution of saline and other solutions, the phenomena of solution will be dealt with somewhat as follows :—

Thermal and volume changes occurring in the act of solution and their mutual relations. How far and under what circumstances are thermal and volume changes to be considered as indicating chemical change?

The molecular volumes of salts in solution. The specific heat and vapour pressures of salt solutions. The relation of solubility to molecular volume, to fusibility, and to the composition of the liquid.

Action of solids and especially of porous bodies on solutions. Phenomena of supersaturation.

What is chemical combination, and is there any criterion by which it may be distinguished from adhesion or mechanical combination?

In consequence of the very wide-reaching character of the subject, it will not be possible to take up the question of solution except as relating chiefly to solids, and especially salts, in water. For the same reason I cannot fully discuss the phenomena of absorption-spectra nor generally the action of solutions upon light, but I hope some of those chemists who have worked on this part of the subject will be present, and will give us the benefit of their experience.

There will of course be a great number of questions incidentally touched upon in my opening, which may well form the basis of remarks from other speakers, such as—

How is saturation to be explained, *i.e.* why is there generally a limit to solubility?

Is there any general connection between solubility and atomic weight in a series of compounds in which only one constituent varies?

What becomes of water of crystallisation when a salt containing water is dissolved in water?

WILLIAM A. TILDEN

The Mason College, Birmingham

THE RECENT VOLCANIC ERUPTIONS IN NEW ZEALAND

WE have been favoured by Dr. Hector, F.R.S., Director of the Geological Survey of New Zealand, with a copy of a Preliminary Report drawn up by him for the New Zealand Government regarding the volcanic eruptions of last June in the North Island. It is gratifying to find that the hope expressed in NATURE (p. 322) has been so promptly fulfilled, and that the investigation of the remarkable phenomena has been undertaken by so competent an observer as Dr. Hector. The following is his Report, but it is merely a preliminary outline, and will no doubt be followed by much ampler details.

"*Colonial Museum of New Zealand, Wellington, June* 23, 1886

"According to instructions from Government, I proceeded to Tauranga on the evening of Thursday, the 10th instant, in the colonial gunboat *Hinemoa*, and arrived there on Saturday afternoon. At Tauranga I engaged the services of Mr. Spencer, a skilful landscape photographer, and on Sunday our party, seven in number, drove to Rotorua by the Oripi Road, the ordinary route by Te Puke being blocked On Monday I proceeded to Wairoa with Captain Mair, who joined the boat expedition which had been organised to search the Native settlements on Tarawera Lake. On the same day I sent my assistant, Mr. Park, to the south of the disturbed area by way of Kaiteriria; and on Tuesday, following the same route, I examined the vicinity of Rotomahana. Mr. Spencer, with his camera, accompanied me everywhere, so that a series of well-selected views of the eruption and its effects was obtained. On Wednesday we started for Taupo, feeling anxious to complete the general view of the whole line of volcanic activity from Ruapehu to White Island, as alarming rumours were in circulation as to the extent of country that had been affected. By this route we also obtained a distant but interesting view of the newly-raised cones of Tarawera from the eastward. The incidents of the eruption have been so fully described by the Press that it is unnecessary for me to refer to them in this preliminary report, the chief object of my rapid inspection having been to ascertain the exact locality, nature, and extent of the outbreak, and its probable consequences to the district. A complete geological examination of the district has therefore been deferred until a more favourable season for field-work, and until the volcanic activity has sufficiently subsided to admit of accurate observation.

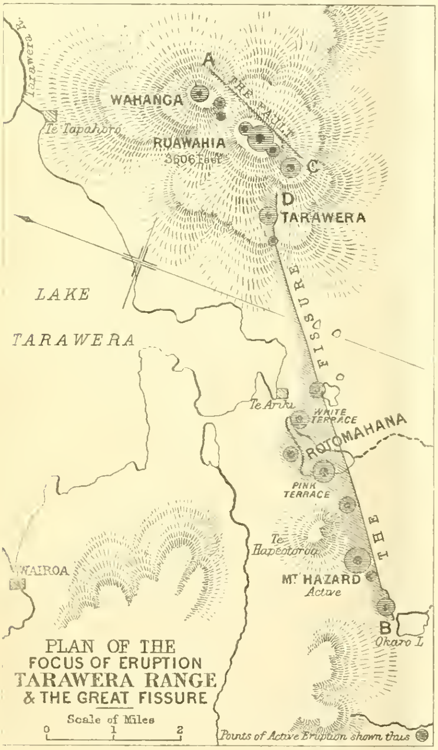
"The focus of the disturbance was ascertained to be in a line extending from seven to ten miles in a north-east to south-west direction from the north end of the Tarawera Range to Okaro Lake (see plan.) The northern part of this line is occupied by the Tarawera Range. This range has three summits, the northernmost being Wahanga; the central, Ruawahia, 3606 feet alt.; and the southernmost, Tarawera Mountain proper. The southern part of the line previous to the outburst was a depression occupied by Rotomahana Lake, surrounded by low undulating country composed of pumice-sands and overspreading deposits of siliceous sinter, most of which were connected with active geysers, amongst which the most famous were those at the Pink and White Terraces.

"From the most reliable evidence it appears that the outbreak commenced at ten minutes past two on the morning of the 10th, by an eruption from the top of Wahanga, attended by a loud roaring noise, and slight earth-shocks. In a few minutes this was followed by a similar but more violent outburst from the top of Ruawahia—the middle peak of the range, and after a short interval this phase of the eruption culminated in a terrific explosion from the south end of Tarawera Range, north-east of Lake Rotomahana. For nearly two hours this was the only phase of the eruption, and was accompanied by the ejection of vast quantities of steam, pumice-dust, and hot stones, forming huge towering clouds, illuminated by lightning flashes.

"It was at this time also that a great crack or fissure (A C on plan) was formed along the east face of the Tarawera Range. I only had a distant view of this fissure from the eastward, but Mr. Percy Smith, the Assistant Surveyor-General, who had a near view from the sides, reports that the whole east end of the mountain

has been blown away, and that the *débris* covers the country to a distance of many miles. The white terrace of pumice-sand that I saw was singularly flat-topped, and seemed to slope abruptly from the mountain like a huge embankment 500 feet high. Besides these heavy sands that lodged close to the fissure in the mountain side, the lighter dust was spread out in the form of stratified clouds, which were distinctly seen, at this period of the eruption, from Rotorua, Tauranga, and Taupo.

"The cloud thus formed discharged its contents for the greater part in a direction to the eastward of the mountain, reaching as far as Te Teko and Fort Galatea, and to the westward as far as Wairoa. The earth-shocks, however,



PLAN OF THE FOCUS OF ERUPTION TARAWERA RANGE & THE GREAT FISSURE

Stanford's Geog¹ Estab^t

during this period of the eruption do not appear to have been of extreme violence, or to have created much alarm beyond that part of the district lying in the immediate vicinity of the volcanic eruption; but shortly before four a.m. a violent outburst of a totally different nature was experienced, accompanied with loud reports that reverberated through the atmosphere to enormous distances. The first notice of this outbreak was an earth-shock that appears to have been much more widely felt than those previous, and chiefly in areas where hot springs occur. This development was attendant on the outburst of an immense volume of steam—carrying pumice-dust and fragments of rocks to an enormous altitude—which proceeded from the site of Rotomahana Lake, causing the formation of a dense cloud in the higher atmosphere, that spread in definite directions, its advancing edge being marked by electrical discharges of the most awe-striking character. At first the wind was from the south-east, and the inhabitants of Rotorua appear to have been terrified by the approach of this hideous cloud, when suddenly the wind sprang up from the south-west and arrested its progress in that direction, turning it off towards the north-east, at the same time condensing the vapour of the cloud to such an extent that the suspended solid matter dropped on the surface of the earth in the form of mud, smothering the country, and leading to the disastrous results experienced at Wairoa. By six a.m. the period of active eruption appears to have closed, and since then the display of energy in a modified form has also rapidly declined.

"The following are the chief points which require notice in this report:—

"I. *Focus.*—Tarawera Range, about 3600 feet above sea-level, is an isolated and very conspicuous object in the scenery of the Lake District. It slopes from the east side of Tarawera Lake—the level of which is about 1000 feet above the sea—and previous to the eruption rose very abruptly, with mural precipices and columnar rocks, especially on its western and southern escarpments. It was no doubt judging from this feature that Dr. Von Hochstetter was led to class Tarawera Mountain with the Horohoro Range, as being part of his older or submarine-formed volcanic series, and a remnant of the great plateau (Von Hochstetter, "Reise der Novara," i. 106), the surface of which denotes the original level of the country prior to the production of its present broken surface by the excavation of valleys, by the up-bursting of volcanic mountains, and the consequent subsidence or breaking-in of large cavities that are now occupied by lakes. He nevertheless maps Mount Tarawera as belonging to his recent volcanic group, and also alludes to it in other parts of his work as being largly composed of obsidian. I have never ascended the Tarawera Range, but have examined its slopes and found them to be composed of lavas of a high acidic or rhyolite type, in the form of flows intersected by dykes, and containing, amongst other rocks, large quantities of compact and vesicular obsidian. From this I conclude that the mountain really is one of recent volcanic origin, belonging to Von Hochstetter's new volcanic series, and that its abrupt outlines have resulted from fractures and subsidences of its flanks. According to this view it is natural to assume that the still-imperfectly-cooled mass of lava in the heart of this volcanic mountain has given rise to the long-continued (historically speaking) solfatara action at high temperatures that created the attractive wonders of the Rotomahana. It has been stated that no Native tradition exists of Tarawera having been the site of previous activity, but the range culminates in three distinct peaks, the meaning of the Maori names of which—according to Mr. Locke, M.H.R., and other authorities—clearly contradicts this assumption. This consideration has interest, as a sudden development of volcanic activity in a new locality, or in an ancient and greatly-denuded formation like the trachyte breccia that forms the Horohoro, would have been more serious and significant than the mere temporary revival of the expiring energies of a recent focus of volcanic force.

"II. *The Vents.*—As viewed across Rotorua Lake, on the 13th, from the point where the Tauranga Road emerges from the bush, Tarawera Range appeared to have quite lost its former characteristic outline. The deep gap dividing Wahanga, the northern peak, from Ruawahia, the central one, was almost obliterated, and the abrupt, precipitous sides of the mountain were everywhere softened by great slope deposits of material ejected from the volcanic vents, consisting of stones and dust of a grey colour. Along the edge of the range seven distinct

points were seen to give off steam from flattened conical heaps of dark-coloured *débris*, and at intervals these vents threw off large volumes of steam and vapour, darkened to a reddish hue by solid matters, which were discharged to a height estimated at from 200 feet to 500 feet. Four days later, when viewed from the eastward, the same range showed a similar appearance, allowing for the change in direction; but the cone on the summit of Ruawahia had evidently accumulated with greater rapidity than the others, and had acquired lateral cones, giving its outline a similar appearance to that of Rangitoto, near Auckland.

"During two clear nights I watched the eruption from these vents, and could distinguish them against the sky with a powerful binocular telescope; but I never observed any illumination of the ascending steam clouds, as if from the surface of an incandescent mass within the vent, nor was there any sign of any outpouring of lava, either from these vents or from cracks or fissures in the sides of the mountain, during the time of my visit. In addition to the above-mentioned conical vents on the summit of the range, along its eastern side the line of fissure already alluded to was distinctly visible, emitting wreaths of steam. This line of fissure lay in an oblique direction, so that it appeared to gain in elevation along the sides of the mountain from north towards south, but not sufficiently so as to indicate for it a direction that would make it continuous with the great fissure south of Tarawera, but rather in the direction of line A C on plan. It is below this fissure-line on the eastern flank of the range that bulky terrace-like accumulations of pumice-sand have been formed, and if this eruption should ever reach the stage of producing lava, which from other circumstances I think hardly likely, it is from this fissure that I should expect the lava to exude.

"III. *The Great Fissure.*—This is the most remarkable and characteristic feature of the late eruption, and the chief origin of the disastrous results which attended it (B D on plan). A good view, but much obscured by steam, was obtained from the hill called Te Hape-o-Toroa—alt. 2300 feet—by Mr. Park on the 14th, and by myself on the following day. This fissure seems to commence as a narrow rift at the northern end from the great rent which has been formed in the south end of Tarawera Mountain. This rent is a most wonderful feature. It is not a slip from the mountain side, but appears as if a portion of the mountain measuring 2000 feet by 500 feet, and 300 feet deep, had been blown out, leaving a ragged, rocky chasm, from which steam was being discharged in rapidly-succeeding puffs. The eastern side of this chasm was brightly tinted, as if by the efflorescent deposit of a mineral substance, probably ferrochlorides. Sulphur has been mentioned as a deposit from this recent outburst by some who have witnessed it; but this is hardly a possible result of such rapid volcanic developments.

"The view I obtained of the extent of this chasm south was much obscured by numerous volumes of steam blowing off from the newly-formed fumaroles that occupied the site of Rotomahana. From the eastern slope of Te Hape-o-Toroa we looked right into the fissure, and, as far as I could see, it appeared to have a nearly straight boundary of undisturbed ground on its eastern side, extending from the Tarawera chasm to within a few chains of Lake Okaro, thus intersecting the Rotomakariri or the cold lake, the Rotomahana Lake, and the valley extending from thence southward. The west side of the fissure, on the other hand, is very irregular in outline, and is continually being altered by the falling-in of its precipitous walls, as the hills are undermined by the action of powerful geysers, seven in number, which at irregular intervals throw up great volumes of boiling water, with stones and mud, to a height of 600 feet to 800 feet from the bottom.

"It is only by occasional glimpses during the breaks of the steam that any idea can be formed of the nature of the bottom of this huge fissure; but it seemed as if it was entirely occupied by large circular areas of mud, seething and boiling in such a fashion as to convey the impression of its being in a very liquid state. These mud-pools are separated from one another by comparatively solid ground, and in some cases, especially towards the eastern side of the fissure, what appear to be small pools of water with sedgy margins could even be distinguished; but the difficulty of estimating distances and depths through the steam-clouds rendered the observations made very uncertain.

"The largest of these mud geysers appeared to be that rising from the position formerly occupied by the Pink Terrace, but the most interesting is one a mile further south, which, unlike the others, does not spring from the bottom, but from the comparatively high ground on the west side of the fissure, and, owing to the obliquity with which the fragments are thrown out, is gradually building up a conical mound, which already has attained an altitude of several hundred feet (Mount Haszard, on plan). At the southern extremity the fissure is bounded by a bold semicircular extremity, from the base of which powerful steam jets are escaping; but there was no evidence that it was prolonged by a crack or fissure, or fault, or other displacement of ground, nor was there any evidence that the fissure had been produced by any inequality of the movement of the ground bounding it, but rather that it was caused simply by the removal of material which formerly occupied its space. Its direction, as far as could be ascertained, is N. 50° E., which is the general line of direction that would connect all the more active geysers between Tongariro and White Island.

"IV. *Matter ejected during the Eruption.*—The quantity of matter which was ejected during the different phases of the eruption was very large. In the first place, stone fragments were scattered from the earlier eruptions of Tarawera over an area of country extending to the eastward as far as Te Teko, and even, some say, to Fort Galatea; while in the opposite direction they are not reported to have fallen at any place farther west than Wairoa, a distance of six miles. None of the fragments which I collected are other than portions of rocks of the district, nor do they present in the slightest degree the character of volcanic bombs or lapilli formed from lava or rock material in a state of fusion. Yet there can be no doubt, if we can accept the evidence of the eye-witnesses, that these rock-fragments must have, in some cases, reached the ground in a partially incandescent state. Next followed the great ejection of pumice-sand, which forms enormous deposits in two localities: the one is on the eastern slope of Tarawera Mountain, already described, the nature and origin of which I had no opportunity of ascertaining; the other deposit of this nature is chiefly on the western side of Rotomahana fissure, and was no doubt ejected at the commencement of the second phase of the eruption. Over a district of twenty-four square miles south of Tarawera Lake, and on an almost equal area to the north and east of the lake, the whole surface of the country has been covered with this pumice-stone so thickly as to obliterate in a great measure the natural features, partly filling the gullies and enveloping all the hills as if with a deep mantle of snow, so that not a trace of vegetation can be seen, from the highest peaks, such as Te Hape-o-Toroa, which is 2300 feet above the sea, down to the level of the lake. The thickness of this deposit could not be ascertained at the time of my visit, as no slips had occurred in it and no sections were to be seen. It consisted of fine-grained and gritty pumice-sand, slightly crusted on the surface by the action of the rain, which also caused it to assume a slightly greyish tinge; but underneath it was a pure white, and at a depth of 12 inches to 18 inches from the surface had still a high temperature on the sixth day after the eruption.

"Lying on the surface of this deposit, especially on the slopes directed towards the fissure, fragments of considerable size of various kinds of rocks were scattered about, and among these were masses evidently derived from the sinter of the terraces, and, from the manner in which these fragments appeared to occur in quantities where the finer dust had been blown from the surface, it is probable that the lower layer of the deposit will prove to be composed of coarser material than the upper. The boundary-line of this dazzling white deposit is very distinctly marked. It can be well seen where it passes over Kakaramea Mountain, dividing it, as it were, into two portions, one white and the other green. While traversing it we experienced a great downpour of rain, which formed the powdery material of the surface into little pellets; but it did not appear to be very absorbent, or to show any tendency to work up into an adhesive material. This is very different from what may be termed "the grey deposit" which is next to be mentioned, and which covers the country, from about two miles south of Wairoa, in a northerly direction towards the Bay of Plenty, as far as the Te Puke Settlement. This is the mud-forming deposit, and wherever it appears to have descended in a thoroughly pasty condition it coated the vegetation so heavily as to break limbs off lofty trees and to crush the smaller scrub flat simply by its weight. The sand, as already stated, appears to have fallen hot, so hot, indeed, as to set fire to the trees, the stumps of which were seen burning in many places; but there is nothing to lead us to suppose that this grey mud when it fell was even warm.

"It has been suggested by some that this moist deposit was mud thrown out from the bottom of Rotomahana Lake; but it is difficult to conceive how, in that case, it should have overleapt a strip of country four or five miles wide, where there is nothing but dry sand, before it reached Wairoa; and I think that a more likely source for its origin is to be found in the sudden condensation of the front edge of the great vapour-and-dust cloud when it suddenly met the violent cold south-west gale which averted it from Rotorua and directed it towards the sea-coast, where it spread over the sky and caused the darkness that was experienced at Tauranga and all over the country to the eastward. The great volume of this dust-cloud was directed towards the East Cape, dropping over the country in that direction a comparatively heavy deposit of brownish-black dust, so coarse as almost to be sand; while on its northern edge, as far east as Tauranga, the dust is of a light grey colour, and excessively fine in grain. A collection of all these different deposits has been obtained, and will be reported on as soon as the chemical analysis is complete. The impact of the moist deposit when it fell must have been very great, from the effects which it produced at Wairoa, where it appears to have attained its maximum thickness of about 12 inches in open level places free from any influence that would cause it to drift; on the flat spur above the bridge at the outlet of Rotokakahi its depth was found to be 9 inches, and in the Tikitapu Bush 4 inches; and from that point it gradually decreased towards the north. The action of rain upon this mud rapidly converts it into a semi-fluid condition, in which state it slides off the hill-slopes and fills the low grounds and watercourses; and where it has been thickly deposited it will thus be a constant source of danger for some time to come, but where only an inch or so in thickness it will, I believe, rapidly disappear, and, excepting that it may for a time deteriorate the pasture and destroy the existing vegetation, it will in the long run be an advantageous addition to the light pumice soils upon which it has been deposited, owing to its slightly absorbent properties. As for the light deposit of dust, which fell in a dry state, there is very little doubt that it will be all washed off into the soil with the first heavy rains that come. The distance to which this fine dust was carried was very great, exceeding at least 120 miles from the focus, in a direction between north and east; and the time it remained suspended in the atmosphere was at least eighty-four hours, as we passed through it in the *Hinemoa* when crossing the Bay of Plenty on the Saturday afternoon, as a peculiar yellowish fog, charged with pungent acid vapour and dust; and on the following afternoon we recognised the same fog-cloud still suspended in the atmosphere towards the north-east.

"V. *The Evolution of Steam.*—The enormous volume of steam rising from the site of Rotomahana Lake gives rise to a pillar of cloud that is visible in all directions over the country, having a diameter of about an eighth of a mile, and rising to a height of not less than 12,000 feet. Its effect is most impressive, especially in the morning and evening, when it is lighted up with gorgeous tints by the slanting rays of the sun when it is below the horizon, and all the surrounding landscape is in twilight. Although this steam-cloud receives rapid additions in its lower part from successive explosions, these do not generate any rapid movement through the mass of the cloud, so that, if viewed from a distance, it appears to be almost solid and immovable, except the changes that are gradually effected upon its lower portion by the movements of the atmosphere.

"VI. *The Propagation of the Earthquake Tremors.*—Earthquakes are the usual results of the violent concussions attendant upon violent outburst, and they afford the only clue which we can possibly have as to the depth below the surface of the earth at which the volcanic energy has been exerted. Thus, if the earthquakes are felt with only slightly-decreased violence to great distances from the focus of disturbance, it would indicate that the disturbance is a deep-seated one. On the other hand, if the earthquakes, although extremely violent close to the focus, are only felt at a moderate distance, the conclusion to be drawn is that the forces at work are only superficial. All reports agree that at the Wairoa, about four miles distant, which is the nearest point to the eruption from which any persons have survived, the shocks of earthquake during the first phase were violent and continuous; whereas at Rotorua, twelve miles distant, they were comparatively slight. The great earthquake at the commencement of the second phase appears to have been felt with considerable violence at Rotorua, and distinctly arrested attention for a distance of at least from sixty to seventy miles, but does not appear to have done any damage.

"During our visit the earthquake shocks in the vicinity of Rotomahana were still frequent and violent, but at Rotorua they were only experienced as gentle undulations; and I ascertained that they proceeded from the effects of the explosion from the Rotomahana fissure, and that the eruptions from the summit of Tarawera, which were clearly visible from Rotorua, did not produce the slightest apparent tremor at that distance. A few insignificant earthquake-rents were seen crossing the flats south of Kaitiriria, but only where there was a drop or unsupported bank.

"VII. *The Sounds.*—The sounds produced during the eruption must have been, from all accounts, appalling to those within a moderate distance. The crackling thunder produced by the electrical discharges, the terrific roaring of the high-pressure steam escaping through the volcanic vents, were combined with terrifying effects. Much has been said about noises heard at Auckland, Wanganui, and other places. From the times mentioned, these appear to have been due to the reverberating reports accompanying the Tarawera outbreaks. Some of these noises may have been propagated through the atmosphere, and reflected to the earth from the under surface of the stratiform cloud-sheets that were widely spread in various directions over the colony on that morning. Others, again, may have been propagated through the earth.

But I have been informed that at the whaling settlement of Tawaite, on the east entrance of Tory Channel, from six p.m. up to about eight p.m. on the evening of the 9th (the night preceding the eruption), loud booming reports were heard as through the earth. As these reports were previous to any symptom of the loud disturbances at Tarawera, this suggests that they may have resulted from a slight movement along the great fault-lines that traverse the North and South Islands in a north-easterly direction; and, in this case, the immediate cause of the Tarawera outburst may be found in a local fracture resulting from such movement.

"VIII. *Premonitory Symptoms.*—The only premonitory symptoms of the coming outburst which have been described were an oscillation in the level of Tarawera and Rotorua Lakes, and the occurrence of earthquakes for some months past in that district, where, as a rule, earthquakes are rarely felt. But neither of these are very characteristic incidents, nor would it be safe on future occasions to base any expectation of an eruption on such phenomena alone. The increased activity of the geysers and hot springs during the past season has also been advanced as having been a symptom of an approaching outbreak; but those who were most familiar with the district will agree that their variation was no greater than is usual under the influence of rapid changes of wind and atmospheric pressure. The reports of sympathetic outbreaks in other places along the line of volcanic energy from White Island to Ruapehu appear to be quite unfounded. The outburst has shown conclusively that the springs at Rotorua and Rotomahana are quite independent of each other, and of those at other places, thus confirming the observations made by Von Hochstetter long ago, that all the various points at which thermal springs occur are situated round the margins of lakes formed by subsidence of circular areas, and are not connected by an underground system of gravitational drainage.

"IX. *Conclusion.*—From the foregoing sketch of the character of the eruption I think there can be little question that it is a purely hydro-thermal phenomenon, but on a gigantic scale; that it is quite local and not of deep-seated origin, and that all danger is past for the present, so far as one can venture to form an opinion on such a subject. The extra activity of the *puias* which has been observed is no doubt owing to the heavy rains that, on the 9th, set in after the longest period of drought which has been experienced in that district for many years, and probably the frequent earthquakes which have of late agitated the ground have contributed to this activity by stirring up the sources of the water-supply, and facilitating the access of drainage-waters to the sources of the heat. But beyond what may be accounted for in this manner I believe there is no increased disturbance at Rotorua, Wairakei, Taupo, and other places. The quiescent condition of Tongariro and Ngaurahoe was plainly shown by the manner in which we observed it to be enveloped in snow. As a rule, on the scoria cone of Ngaurahoe, snow rarely lies, excepting in a few of the gullies, but melts almost as rapidly as it falls. On the morning of the 17th, however, the cone of Ngaurahoe was covered with a great mantle of snow; while the *puias* on Tongariro showed less than their usual amount of steam escaping. The only fresh activity which may be reasonably expected is that which I anticipate when sufficient rain has fallen to cause the overflow of Okaro Lake into the south end of the great fissure, as its former drainage outlet to the Rotomahana Lake appears to me to be completely filled up. If this should occur, and a fresh explosion takes place in consequence, it will be comparatively moderate in its effects, as, unlike Rotomahana, the soft, incoherent pumice deposits between the fissure and Okaro Lake are not sealed down by an enormous weight of siliceous sinter.

"For some time to come great variations must be expected in the activity of the newly-formed *puias* according to the manner in which changes occur in the atmospheric pressure; but, unless it can be shown that any local change in the barometer is experienced which is not shared by the surrounding district, the barometer affords no indication as to whether an eruption is or is not imminent. One of the most unfortunate results of the eruption, in addition to the disastrous loss of life and the destruction of the country, is the disturbance of the sense of security which has grown up amongst those residing at the Hot Springs; and I believe that many persons are so thoroughly shaken by the horrors experienced on the morning of the 10th that they will not recover their equanimity until they have been for some time resident away from the sounds, smells, and shocks that characterise the district. "JAMES HECTOR"

IN QUEST OF THE ORIGIN OF AN EPIDEMIC

IN our issue of the 8th ult. (vol. xxxiv. p. 213) we dwelt on certain general aspects of the reports lately laid before the President of the Local Government Board by the Medical Officer of the Department on milk-scarlatina, but these documents deserve more detailed consideration, for they show us our modern organisation for combating death and disease, by prevention, at its best. They show us, too, the men to whom the task of guarding public health is primarily committed at their best—patient, watchful, wary, tenacious of the thread of their investigation, eliminating this or that doubtful element, until finally they have tracked their quarry to its lair. In reading Mr. Power's report, we have been constantly reminded of that famous description of the contest between the man and the gun in Hugo's "Toilers of the Sea." Here the fight was man against disease, and the former has succeeded in his task. We shall endeavour in this article to show how Mr. Power, of the Local Government Board, succeeded in tracing, step by step, an epidemic of scarlatina to its source.

On December 18, 1885, Mr. Winter Blyth, the Medical Officer of Health of St. Marylebone, reported to the Board an extensive outbreak of scarlatina in his district. This he believed to be associated with the distribution of milk from a certain retailer in South Marylebone, who obtained his supplies from two farms, but the occurrence of the scarlatina appeared to be coincident with the milk-distribution from a certain farm at Hendon. Mr. Blyth had himself visited this farm, and, with the assistance of Dr. Cameron, the Hendon Medical Officer of Health, had carefully examined it, but was quite unable to discover in its sanitary circumstances or in the health of those employed about it any sort of clue to the cause of the infection of the milk. Accordingly he went with his story to the Local Government Board. It will be seen that Mr. Blyth had done his work exceedingly well: in one of the most crowded districts of London he had succeeded in tracing the scarlatina to a farm at Hendon; that is, he had made out a strong *primâ facie* reason for suspecting this farm; he had put a clue into Mr. Power's hands which he had not been able to follow any further himself. The first question for Mr. Power to answer was whether the Hendon farm was at fault or not. When this was answered it would be time enough to pursue the inquiry more minutely: it would be loss of time to try to dig out the fox unless it was first ascertained that he was in that particular earth. With this object, then, Mr. Power traced the milk from the Hendon farm to other milk-retailers in St. John's Wood, St. Pancras, Hampstead, and Hendon itself. From each of these, except St. John's Wood, the same story came. Until the end of November or beginning of December the district had for some months been exceptionally free from scarlatina, but about

this date the disease had suddenly and notably increased, a large proportion of the recorded cases having occurred amongst the customers of milk-retailers dealing in the particular Hendon milk. These facts strengthened the case against the Hendon farm, but did not by any means establish it, inasmuch as the retailers in question obtained their supplies from other farms as well, and although in two cases these were situated in widely different counties, yet the case against Hendon was still in the condition of not proven, more especially as the St. John's Wood customers of that farm were certainly wholly free from scarlatina. Simultaneously with this investigation, another was being pursued at the incriminated dairy itself. But nothing was revealed here to show how the disease could be propagated from it as a centre. There was no scarlatina, nor any illness at all like scarlatina, amongst the persons employed about the farm, or their families and neighbours, at any such time or in any such way as to influence the farm or its produce. This, then, was the state of affairs on December 23, or less than a week after Mr. Blyth's report: there was a strong presumption against the Hendon farm, but outside human agencies had to be set aside as not having been operative. A thorough inspection of the farm itself was at once undertaken. Now it happened that the farmer in question, as well as one of the dealers who purchased from him, was particularly careful in all sanitary matters respecting his dairy. Every precaution had been taken by both to secure the farm and milk against any known sanitary fault or misadventure, and thus the inquiry advanced another stage. If the Hendon farm had caused the scarlatina, it did not do so in any commonly accepted way, such as through unwholesome conditions of water or drainage, or careless handling of milk or milk-utensils, by persons carrying scarlatina infection. This threw Mr. Power back on the theory of something in the cows themselves which caused the scarlatina to be distributed with their milk, and this formed his working hypothesis thenceforth. To discover this "something," and to understand its nature, it was necessary to ascertain in detail every parallel between the doings at the dairy farm and the observed scarlatina.

Here, then, we enter on the second and by far the most difficult stage of the investigation. The various districts supplied from Hendon were taken one by one; the quantities of milk obtained from Hendon by the dealers there, and by the same dealers from other sources, were ascertained; the dates of the notable incidence of the disease among the customers, and the degree of incidence at one period and another, were carefully observed, and compared, with the following results:—(1) The disease commenced at one and the same time in the four districts supplied from Hendon, viz. South Marylebone, Hampstead, St. Pancras, and Hendon. (2) In South Marylebone the disease increased day by day with increasing force up to the date of the inquiry. (3) In Hampstead and St. Pancras there was a cessation of ten days after the first attack, and then a larger number of persons were taken ill, the attacks continuing up to the date of the inquiry. (4) In St. John's Wood there was no scarlatina whatever down to the date of the inquiry, although the dealer there got five-sixths of his milk from Hendon. Were there any conditions in the farm operations parallel to these special phenomena? And first, was there any new condition pertaining to the cows coincident with the milk producing scarlatina at the end of November in four districts, continuously in South Marylebone, and after a break in the other three, while this condition was absent in the case of the cows supplying the St. John's Wood dealer? A tedious inquiry into such circumstances as the food, calving, health, arrival and departure of cows proved barren of result; nothing could be heard of for some time that was new or changed. But at last it appeared that on November 15 three newly-calved cows, purchased in Derbyshire, had come on the farm, and four from Oxfordshire on December 4. The practice of the farm was to isolate or quarantine new arrivals for examination for a week or ten days, and then to admit them into the stalls with the others. The cows on the farm at this period numbered 90 or 100, distributed in unequal numbers in three sheds, called the large, middle, and small sheds. The supply of the milk from the large shed went to South Marylebone only; that from the middle shed partly to South Marylebone, partly to Hampstead and St. Pancras; and that of the small shed to the two latter places and to St. John's Wood. So far we have this coincidence between the doings at the farm and the incidence of the disease—that the latter broke out after the time that the milk of the Derbyshire cows was added to the general stock, in three districts supplied from the farm; and that St. John's Wood, which did not receive any milk from the new arrivals, was free from scarlatina.

We have now reached what may be called the third stage of the case. In the first, what Mr. Power calls a "notable," and what lawyers perhaps would call a "violent," presumption had been made out against the Hendon dairy; in the second, a weaker presumption had been established against the Derbyshire cows which had been added on November 15, and whose milk began to be distributed to the three affected districts, and not to St. John's Wood, a few days later. But then, the facts of a continuous and increasing attack in South Marylebone, and the intermission of about ten days in St. Pancras and Hampstead, had to be accounted for, if the case was to be made out conclusively against the incriminated dairy. To deal with these, Mr. Power reversed the process hitherto pursued, which was that of pure induction from observed facts. He now employed the *a priori* process, and argued thus:—Taking the fact of uninterrupted progress of the disease in South Marylebone, and of the lull of ten days in the other two, if the dairy at Hendon be the cause of the outbreak, and if, as is most probable, the different results produced by the milk from the same cows was due to a difference in the relation of the cows themselves within the business of the farm, then we should find at the latter—(1) a change in the manner of distributing the milk of the Derbyshire cows, and this probably consisting in placing them, or one of them, in the "large shed," from which South Marylebone was supplied; (2) about the second week in December (the date of the recrudescence of the disease in St. Pancras and Hampstead), some of the Derbyshire or of the Oxfordshire cows, or some other cows which had been in close relation with them, were probably transferred to the "middle shed," from which these two districts were, it will be remembered, supplied; (3) as St. John's Wood, which was supplied from the "small shed," was free from scarlatina, it should be found that none of the new cows, or any other cow in close relation with them, had been placed there. Now, were any arrangements at the farm found corresponding with any or all of these *a priori* conclusions or probabilities? What was found on investigation was this: (1) The Derbyshire cows had been transferred towards the end of November into the "large shed" (the source of the South Marylebone supply), and remained here at the date of the inquiry; (2) the four Oxfordshire cows were transferred about December 11, two into the "large shed," and two into the "middle shed" (St. Pancras and Hampstead supply); (3) at no time had either the Derbyshire or Oxfordshire cows been transferred to the "small shed" (St. John's Wood). Here, then, both by positive and negative evidence, the presence of scarlatina in certain London districts was associated, first, with a particular dairy, and secondly, by a series of parallel events, with certain cows within that dairy. Mr. Power, having reached this point, felt justified in assuming, until anything to the contrary should appear, the presence

of something in these cows competent to produce scarlatina in persons consuming their milk, and the inquiry was narrowed to determining what this was. All comparison with former experiences was for the present left out of consideration, the investigation proceeding strictly on the circumstantial evidence obtained and obtainable. A consideration of all that had gone before, and the absence of any alternative, led to the provisional adoption at this point of a theory of disease in the cows, and the probability was that this was an infectious disease, communicable from cow to cow, a disease, moreover, the existence of which was compatible with the animal affected feeding well, and milking abundantly.

The discovery of vesicles and ulcers on the teats and udders of cows in the large shed soon followed; the first to show the disease was one of the Derbyshire cows, the second one from Oxfordshire. After this the matter passed into Dr. Klein's hands; but with his report we have nothing to do here. A painful incident soon gave Mr. Power ample corroboration of the result which he had reached. The Marylebone dealer returned on the farmer's hands, on December 15, all his milk from the larger shed, and this was destroyed by pouring it into a pit dug on his land. The news of the destruction of milk spread among some of the poor people of Hendon, and some of them succeeded by the favour of friends amongst the cowmen in obtaining some of it on December 16. By the 20th scarlatina made its appearance amongst half-a-dozen of the families thus supplied. Conversely in South Marylebone about Christmas, when these Hendon families were falling ill, the disease ceased almost suddenly, and there were no fresh attacks, except such as were referable to infection from previous sufferers.

A thorough examination of all the cows showed that the disease had spread to every one of the three sheds, and the farmer was accordingly advised to seek out every cow then or afterwards affected with sore teats or udder, or any other ailment, to isolate her and keep all her milk out of the business, and prevent cowmen employed about the sound cows from attending the infected ones. These precautions were taken from January 1, and were barely in time to prevent an alarming increase of scarlatina in all the districts served from Hendon, including St. John's Wood, where the appearance of scarlatina corresponded to a nicety with the appearance of the cow-disease in the animals in the small shed. The milk from the Hendon farm was ultimately given up by all the dealers concerned, with the result that scarlatina has disappeared from amongst the customers of the dealers here referred to in Marylebone, St. Pancras, Hampstead, and St. John's Wood. The work of demonstrating the nature of the cow-disease, and its connection with human scarlatina was not Mr. Power's, and from him the matter passed on to Dr. Klein. The former had succeeded in gathering up and connecting the scattered links of a chain of presumptive evidence against certain cows so strong as to be unassailable; and he had done this by the exercise of patience, sagacity, and acuteness which would have done credit to a great criminal lawyer weaving the web of circumstantial evidence around an unusually cunning forger or murderer.

THE ORIGIN OF VARIETIES

THE publication in the three last numbers of NATURE, by Mr. Romanes, of very important papers,[1] induces me to send the following lines as a contribution to the discussion upon them that is sure to ensue. He ascribes the origin of varieties to peculiarities in the reproductive system of certain individuals, which render them more or less sterile to other members of the common stock, while they remain fertile among themselves. I also have a theory which, while it differs much from that of Mr. Romanes, runs on curiously parallel lines to it, and was prompted by the same keen sense of an inadequacy in the theory of Natural Selection to account for the origin of varieties. I should not have published my views until they had been far more matured than they are had not the present occasion arisen.

It has long seemed to me that the primary characteristic of a variety resides in the fact that the individuals who compose it do not, as a rule, *care to mate* with those who are outside their pale, but form through their own sexual inclinations a caste by themselves. Consequently that each incipient variety is probably rounded off from among the parent stock by means of *peculiarities of sexual instinct*, which prompt what anthropologists call endogamy (or marriage within the tribe or caste), and which check exogamy (or marriage outside of it). If a variety should arise in the way supposed by Mr. Romanes, merely because its members were more or less infertile with others sprung from the same stock, we should find numerous cases in which members of the variety consorted with outsiders. These unions might be sterile, but they would occur all the same, supposing of course the period of mating to have remained unchanged. Again, we should find many hybrids in the wild state, between varieties that were capable of producing them when mated artificially. But we hardly ever observe pairings between animals of different varieties when living at large in the same or contiguous districts, and we hardly ever meet with hybrids that testify to the existence of unobserved pairings. Therefore it seems to me that the hypothesis of Mr. Romanes would in these cases fail, while that which I have submitted would stand.

The same line of argument applies to plants, if we substitute the selective appetites of the insects which carry the pollen, for the selective sexual instincts of animals. Both of these, it will be remembered, are mainly associated with the senses of smell and sight. If insects visited promiscuously the flowers of a variety and those of the parent stock, then—supposing the organs of reproduction and the period of flowering to be alike in both, and that hybrids between them could be produced by artificial cross-fertilisation—we should expect to find hybrids in abundance whenever members of the variety and those of the original stock occupied the same or closely contiguous districts. It is hard to account for our not doing so, except on the supposition that insects feel a repugnance to visiting the plants interchangeably.

No theme is more trite than that of the sexual instinct. It forms the main topic of each of the many hundred (I believe about 800) novels annually published in England alone, and of most of the still more numerous poems, yet one of its main peculiarities has never, so far as I know, been clearly set forth. It is the relation that exists between different degrees of unlikeness and different degrees of sexual attractiveness. A male is little attracted by a female who closely resembles him. The attraction is rapidly increased as the difference in any given respect between the male and female increases, but only up to a certain point. When this is passed, the attraction again wanes, until the zero of indifference is reached. When the diversity is still greater, the attractiveness becomes negative and passes into repugnance, such as most fair-complexioned men appear to feel towards negresses, and *vice versâ*. I have endeavoured to measure the amount of difference that gives rise to the maximum of attractiveness between men and women, both as regards eye-colour and stature, chiefly using the data contained in my collection of "Family Records," and have succeeded in doing so roughly and provisionally. To determine it thoroughly, and to lay down a curve of attractiveness in which the abscissæ shall be proportional to the amounts of difference, and the ordinates to the strength of attraction, would require fresh and special data that have

[1] I write from abroad, and have not yet seen the original memoir published by the Linnean Society.

yet to be collected and discussed, and about which I will not now speak. Suffice it to say that such inquiries as I have made confirm, so far as they go, the reasonable expectation that some more or less regular curve will be found to exist in respect to any given quality or group of qualities. Each individual would possess his own characteristic curve, but the average of the tastes of many individuals would, as all statistical experience justifies us in believing, afford fairly constant data. These would enable us to argue out the hypothesis I have submitted, with mathematical precision; at all events, with much more closeness of reasoning than is now possible. But this much may even now be averred: (1) That the existence of a law of sexual selection such as I have described, is probable; (2) if it exists, it would have a powerful influence in rounding off any incipient variety that differed notably in any one particular or in any group of particulars from the parent stock; (3) it would be favourable to the vigour of the variety, after it was once fairly started, by checking too close interbreeding.

It must be borne in mind that differences overlooked by ourselves, who are singularly deficient in the sense of smell, and who are hardly able to distinguish without scrutiny even the sexes of some animals, may seem very considerable to the animals themselves. Also that the only differences that we are able to recognise between two varieties may connote a host of unseen differences, whose aggregate would amply suffice to erect a barrier of sexual indifference or even repugnance between their members.

FRANCIS GALTON

August 23

NOTES

THE Local Committee of the Birmingham meeting of the British Association has issued a descriptive programme of the excursions which have been arranged for Saturday, September 4, and Thursday, September 9. The programme covers 120 pages, and has been compiled by several specialists with the greatest care. There are twenty-seven excursions in all, besides a geological excursion to the Lower Palæozoic district of Shropshire. This excursion will last six days, from September 9 to September 15. Prof. Lapworth will take the leadership.

THE French Association for the Advancement of Science has concluded its annual meeting at Nancy, after having resolved that the 1888 session will be held in Oran, Algeria; Col. Laussedat has been elected President for that meeting. The 1887 session will be held in Toulouse, as decided at the last meeting.

AT the Buffalo meeting of the American Association it was proposed to devote especial attention to the study and discussion of the interesting phenomena of the Niagara Falls and the gorge below. On Friday, August 20, one or more preliminary papers of an expository and suggestive nature were to be given, intended to prepare the way for a short field-study of the Falls and the gorge, which occupied Saturday. Monday forenoon would be devoted to the discussion of the gorge and the problems to which it gives rise. A new survey of the Falls has been arranged for, so that a considerable addition to the data for the computation of the rate of recession will be at command, and it is expected that new observations in other important lines bearing upon the chronology of the gorge will be presented, and will throw fresh light upon the history of the formation and recession of the Falls, and upon the utility or untrustworthiness of the gorge as a geological measure of time.

WE learn that the Lick Trustees—after a most thorough discussion of the various plans and specifications submitted for the mounting of the 36-inch refractor of the Lick Observatory and for the steel dome to cover the same, and with a special consideration of the element of time, which circumstances now make one of vital interest to the work—have let the contract for the former to Warner and Swasey of Cleveland, Ohio, for 42,000 dols., and the contract for the latter to the Union Iron-Works of San Francisco for 56,850 dols. The Trustees acknowledge the very prompt and courteous manner in which Mr. Grubb has responded to their invitation, and the very great disadvantage to which he has been put by the remote situation of his works from California, &c. The President of the Trustees has stated that he believes that Mr. Grubb's idea of an elevating floor in principle offers the best solution yet submitted of the very difficult problem of a convenient chair for the observer with so large a telescope. The method of elevating the floor will have to be adapted to the peculiar circumstances of the site of the Lick Observatory, and the means to be commanded there with its very limited water-supply. This subject is now being carefully studied, and so far the only apparent obstacle to the adoption of Mr. Grubb's plan is the question of cost.

WE have to record the death, at Tomsk, of Alexander Krapotkin, on August 6, at the age of forty-five years. M. Krapotkin had done some good work for science in Russia. He had translated into Russian Mr. Herbert Spencer's "Principles of Biology," and Clerk-Maxwell's "Theory of Heat," and for several years contributed to Russian periodicals reviews of the progress of physical astronomy, much valued by Russian astronomers. In 1874 M. Krapotkin was exiled to Minusinsk in East Siberia, and there he helped Dr. Martianoff to organise a local museum; and for several years carried on meteorological observations, which were printed by the Kazan Society of Naturalists. His most important work, however, was a critical investigation of all our present knowledge of the stellar systems and constitution of stellar groups. Every known source in every European language was ransacked for data, though the difficulties he encountered in his peculiar position prevented him from bringing his work down to a later date than 1879. He hoped to complete the work, and publish it, after his expected liberation in September. His untimely death has put an end to this hope.

WE have received the third number of the *Journal* of a Society recently founded in Bombay, called the Natural History Society of Bombay, which, though it is young, appears to have abundant vitality. There are already several learned societies in India and Ceylon, all of which appear to be very successful; but the field is so vast and varied, and the number of men, servants of the Crown and others, capable of doing good work is so great, that it is impossible to have too many of these associations, and accordingly we welcome the new Society, and are glad to notice the energy it displays. In the number of the *Journal* before us, Capt. Becher describes the life (mainly the bird-life) of a Sind lake, Manchar, near the Indus; "A member of the Society" similarly compiles some notes on animal life in the rivers of British Deccan and Kandesh. Mr. Sterndale, one of the editors, has a paper, with illustrations, on abnormalities in the horns of ruminants, in which he expresses the opinion that there is neither persistence nor transmission in the abnormalities of antlered deer, but that they must be persistent in the case of hollow-horned ruminants, and that in the latter case the adage is true: "As the twig is bent, so is the tree inclined." Mr. Aitken, the second editor, publishes a list of the Bombay butterflies in the Society's collection, with notes. The collection appears to be far from complete in any direction. Dr. Kirtikar describes a new species of Alga (*Conferva thermalis Birdwoodii*), discovered among the hot-water Algæ in the hot springs of Vajrabai. There are, in conclusion, various zoological and botanical notes, and a list of presentations to the Society, which we notice in order to mention that they appear to be of great number and variety. One present is a collection of 105

birds. The new Society evidently has many friends and supporters.

AMONGST the great number of publications which are received from time to time from the Smithsonian Institution, two which have lately been issued help better perhaps than anything else to show the magnitude of the work of the Institution as a disseminator of scientific knowledge—a work, moreover, the sphere of which is limited only by the civilised world. The first of these is the list of institutions in the United States receiving the Smithsonian publications. The latter "are so distributed as to be accessible to the greatest number of readers," and the rules for distribution are accordingly of a very elastic kind, giving abundant discretion to the authorities. The publications are divided into three classes: (1) the reports; (2) the miscellaneous collections; (2) the contributions to knowledge. Of these, one, two, or all classes are distributed according to the demands of the neighbourhood to which they are sent, and all that is required in return is that they be "duly acknowledged, be carefully preserved, be accessible to any person who may wish to consult them, and be returned to the Smithsonian Institution in case the establishment at any time ceases to exist." The list of institutions in the United States receiving the publications under these conditions fills a pamphlet of about seventy pages, and numbers nearly 2000. They include various classes of schools and colleges, literary and scientific institutes, learned societies, public libraries, hospitals, &c., in wonderful variety. In looking through the list it is impossible not to recollect the trouble with which a few of our own public institutions succeeded last year in getting some of the Parliamentary papers published by the Government.

THE second publication to which we have alluded is the Smithsonian list of foreign correspondents, in other words, of institutions outside the United States to which the Institute's publications are sent. These reach the enormous number of 7969, every country on the globe with any pretence to civilisation being represented. It thus appears that an ordinary Smithsonian Report has a free circulation of about 10,000, and is spread all over the globe, from Pekin to Valparaiso, from Iceland to New Zealand. The exchange department of the Smithsonian is certainly not the least marvellous part of a marvellous institution.

ACCORDING to the latest consular report from Newchwang, in Manchuria, Seoul, the capital of Corea, is now in telegraphic communication with Pekin, and so with the outer world. The line runs through Moukden. Six years ago no European was allowed to visit Corea, and those who ventured to disregard Corean seclusion generally paid for their temerity with their lives: to-day a merchant in London might telegraph direct to the capital of the Hermit Kingdom.

A SHOCK of earthquake was experienced on Friday evening at Kilsyth, a mining town situated in Stirlingshire, about thirteen miles from Glasgow. About 9 o'clock a sharp rumbling noise of a few moments' duration was heard over the greater part of the town. Much vibration was noticed in many houses at the same time. People ran immediately into the streets, not knowing what had occurred, and many rushed off to the Craig Ends and Haugh pits, situated to the east and west of the town, thinking that one or the other must have been the scene of a great disaster. The shock was most distinctly felt in the north-east portion of the burgh. The weather at the time was close and dull.

ANOTHER slight shock of earthquake was felt at Malta at 8.30 a.m. on August 19. The captain of a steamer which arrived there on the 18th, officially reports that at about 9 o'clock on the evening of the 17th inst. he observed something like a blaze of fire coming out of the water. It was about 30 feet wide and rose to 100 feet above the water, and disappeared at once. The position of the steamer at the time was about 200 miles eastward of Malta. The blaze was observed at the head of the ship, and those on board were certain that it was not lightning.

A CURIOUS result of the volcanic eruption in New Zealand (according to the *Colonies and India*) is alleged to have been found in the sudden breaking up of the drought in Australia. It is said that the great Java earthquake of 1883 was the immediate forerunner of a long spell of dry weather in Queensland in that year, and that a welcome fall of rain in the same colony followed immediately upon the eruption of Mount Tarawera.

MR. F. W. PUTNAM's last report of the explorations which he is conducting with Dr. Metz in Ohio for the Peabody Museum, deals with what is called the Marriott Mound, No. 1, forming part of the Turner group in the Little Miami Valley. The report describes with great minuteness the various objects found in this mound, of which numerous illustrations are given. The find was a rich one. The mound, though it had been ploughed over, was 2 feet high and 60 feet in diameter at the time of the examination. In the centre was found a mass of burnt clay in the shape of a basin, 2 feet in diameter, containing ashes, charcoal, burnt bones, pottery beads, and various shells used as ornaments. About 600 fragments of pottery, from 2000 to 3000 broken and split pieces of bones of animals (chiefly the deer and bear), shells of river clams, several objects in bone and stone, and some human remains, were amongst the objects found in the mound. Of the latter the principal were a perforated skull, various bones belonging to a different skeleton, a third skeleton, partly covered by a large hammered copper plate, and a fourth, which was apparently that of a woman, with numerous personal ornaments near it. Mr. Putnam's report is confined to a bare description of all these and other objects found, and of their precise situations in the mound with regard to the basin in the centre and to each other.

AT the annual meeting of the Royal Society of Queensland, held at Brisbane on July 2, the President, Mr. L. A. Bernays, delivered an address in which he gave a brief *résumé* of the work of the Society during the past year, its meetings, and the publication of the papers read on these occasions, the endeavour to assist in the exploration of New Guinea by organising a fund for the benefit of the Forbes Expedition; the efforts to encourage special scientific pursuits amongst the members by the admission of Sections into its constitution. Finally, having dwelt on the importance of the conduct by the State of systematic instruction with the immediate object of fostering numerous industries which the marvellous range of soil and climate of the colony is capable of calling into existence, occasion was found to dwell on the value of technical and industrial botany, and the importance of its recognition in the plan and management of the colonial botanical gardens, and in the selection generally of objects publicly displayed for educational purposes. With regard to the Forbes Exploration Fund referred to by the President of the Queensland Royal Society, in December last Mr. H. Tryon suggested to the Council of the Society that such a fund should be opened in Queensland. This was done, and at the time it was closed 94*l*. was raised. Subsequently a further appeal was received on behalf of Mr. Forbes, whose operations had been suspended. This led to the fund being reopened, with the result of additional subscriptions being received, raising the total to 145*l*. 10*s*. For a Society which is quite young, and the total income of which is but little over 100*l*. per annum, this is a considerable donation to Mr. Forbes's work, even though Queensland has a special interest in New Guinea.

THE additions to the Zoological Society's Gardens during the past week include an Egyptian Gazelle (*Gazella dorcas*) from Egypt, presented by Capt. Robbins; two Red-under-winged Doves (*Leptopila rufaxilla*) from Guiana, presented by Mr. S. Wells; a Barn Owl (*Strix flammea*), British, presented by Sir Henry Tyler; two Great Eagle Owls (*Bubo maximus*), bred in Shropshire, presented by Viscount Hill; three Yellow-headed Conures (*Conurus jendaya*) from South-East Brazil, presented by Mr. C. Rudge; a Raven (*Corvus corax*), British, presented by Mrs. Tatham; a Martinique Gallinule (*Porphyrio martinicus*) from South America, presented by Mr. J. M. Booker; two Common Boas (*Boa constrictor*) from South America, presented by Mr. T. H. Church; a Common Viper (*Vipera berus*), British, presented by Mr. R. B. Spalding; four Ruscon's Newts (*Molge rusconi*) from Sardinia, presented by Prof. H. H. Giglioli, C.M.Z.S.; two Black-eared Marmosets (*Hapale penicillata*), a Feline Dourocouli (*Nyctipithecus vociferans*), two Yarrell's Curassows (*Crax carunculata*), two Magpie Tanagers (*Cissopis leveriana*), two Ariel Toucans (*Ramphastos ariel*), two Laughing Gulls (*Larus atricilla*), a White-faced Tree-Duck (*Dendrocygna viduata*) from South-East Brazil, purchased; three Aldrovandis Skinks (*Plestiodon auratus*) from North-West Africa, two Common Slow-worms (*Anguis fragilis*), British, received in exchange; six Ribbon Snakes (*Tropidonotus saurita*), born in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *AUGUST* 29—*SEPTEMBER* 4

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on August 29

Sun rises, 5h. 9m.; souths, 12h. 0m. 46·9s.; sets, 18h. 52m.; decl. on meridian, 9° 18′ N.: Sidereal Time at Sunset, 17h. 24m.

Moon (New) rises, 4h. 51m.; souths, 11h. 58m.; sets, 18h. 52m.; decl. on meridian, 9° 21′ N.

Planet	Rises	Souths	Sets	Decl. on meridian
	h. m.	h. m.	h. m.	° ′
Mercury ...	3 37	10 54	18 11	14 4 N.
Venus	2 48	10 27	18 6	17 51 N.
Mars	10 45	15 38	20 31	13 40 S.
Jupiter... ...	8 5	13 59	19 53	2 0 S.
Saturn... ...	0 46	8 51	16 57	21 47 N.

Occultation of Star by the Moon (visible at Greenwich)

Sept.	Star	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image
			h. m.	h. m.	° °
3	γ Libræ	$4\frac{1}{2}$	21 19	22 13	143 273

August 29.—Total eclipse of Sun: not visible in Europe. The central line crosses the West Indies, the Atlantic, and Southern Africa. The members of the British Expedition are prepared to observe the eclipse at Grenada, one of the Windward Isles, where the eclipse will occur soon after sunrise, having a duration of totality of about 4 minutes. In mid-Atlantic the duration will be 6 minutes. In Africa the eclipse occurs near to sunset, with a duration of totality of about 4 minutes.

Sept.	h.	
2	11	Mercury at greatest elongation from the Sun, 18° west.

Variable Stars

Star	R.A.	Decl.		
	h. m.	° ′		h. m.
U Cephei	0 52·2	81 16 N.	Sept. 1,	20 27 *m*
U Ophiuchi... ...	17 10·8	1 20 N.	,, 2,	1 22 *m*
				21 30 *m*
W Sagittarii ...	17 57·8	29 35 S.	,, 2,	0 0 *m*
T Serpentis... ...	18 23·3	6 13 N.	,, 4,	*M*
η Aquilæ	19 46·7	0 43 N.	Aug. 29,	21 0 *M*
R Vulpeculæ ...	20 59·3	23 22 N.	Sept. 3,	*m*

M signifies maximum; *m* minimum.

Meteor Showers

Amongst the radiants that have been observed at this season are the following:—Near γ Pegasi, R.A. 6°, Decl. 11° N.; near ψ Cygni, R.A. 306°, Decl. 54° N.; near λ Cygni, R.A. 311°, Decl. 35° N.; near ε Cephei, R.A. 335°, Decl. 52° N.; and near β Piscium, R.A. 345°, Decl. 0°. Fireballs are of frequent occurrence during this week.

Stars with Remarkable Spectra

Name of Star	R.A. 1886·0	Decl. 1886·0	Type of spectrum
	h. m. s.	° ′	
71 Pegasi	23 27 46	21 52·3 N.	III.
19 Piscium	23 40 34	2 51·3 N.	IV.
φ Pegasi	23 46 41	18 29·2 N.	III.
D.M. − 0° 4585 ...	23 48 55	0 31·6 S.	III.
30 Piscium	23 56 7	6 38·9 S.	III.
47 Piscium	0 22 6	17 15·6 N.	III.
57 Piscium	0 40 34	14 51·2 N.	III.
7 Schjellerup	1 9 49	25 9·9 N.	IV.
R Piscium	1 24 45	2 17·6 N.	III.

GEOGRAPHICAL NOTES

IN a lecture delivered at Cooktown (published in the *Daily Observer* of Brisbane), Mr. H. O. Forbes described his work in New Guinea during the six months he remained there. He set up his winter camp at Sogere, three days' march from the coast, though only 25 miles in a straight line, on the slope of a steep mountain. His work here was varied and important. The meteorological station which was erected was placed under the charge of Mr. Hennessy, and the observations were continued down to the end of his stay. These consisted of records of the mercurial barometer, maximum and minimum, dry- and wet-bulb thermometers, and rainfall, and were recorded without interruption six times in every twenty-four hours. The mass of observations thus accumulated will take a considerable time to tabulate, especially those referring to the atmospheric humidity. Then there was the collecting of zoological and botanical specimens. A large portion of the herbarium consists of giant trees of the forest. It contains about one thousand specimens, one set having been sent to Baron von Müller to Melbourne. A great part of Mr. Forbes's own time was devoted to the survey and delineation of the geographical features of the country. He obtained angles from about fifty different stations and established a base of several miles in length, on which he had hoped to found the triangulation of the country between Sogere and Owen Stanley, and the north-east coast. He also paid a visit to the latter place, and there, as elsewhere, with a little management, found the natives extremely friendly and well-disposed. When Mr. Forbes found his funds failing, he determined, with Mr. Chalmers, on making a dash for Mount Owen Stanley, but the natives who were to have aided him fled in the night, apparently on account of the terrors inspired by the journey. He only got as far as Kaukari, a village two days' journey beyond Sogere. He says that no words can give a true idea of the break-neck, shattered, disrupted condition of the country between Sogere and the central ridges. Beyond the natural obstacles, however (and they appear to be very great), there appears no reason why British New Guinea should not be thoroughly explored, provided the natives are treated with conciliation and tact.

THE Hon. Duncan Gillies, Premier of Victoria, has received a deputation, consisting of members of various learned societies, who urged the expediency of Antarctic exploration. The deputation represented that whale-fishing would make the enterprise remunerative, but at the same time asked the Victorian Government to give encouragement to the project. The Premier, in reply, said that the Government would be willing to grant a subsidy to aid scientific discovery, and that he would ask the other colonies to do the same. In the meantime he would instruct the Agent-General in London to inquire whether steam-whalers would be disposed to embark in the enterprise, and what subsidy would be required.

THE annual meeting of the Association of Swiss Geographical Societies took place at Geneva, at the same time as that of the Society of Natural Sciences. Prof. Chaix was President. Geographical Societies exist now in Geneva, Berne, St. Gall, Aarau, and Neuchatel, and others are about to be established in Zurich, Basle, and Lausanne. Those in existence count altogether more

than a thousand members. The paper which attracted most attention was one by Prof. Forel, on Lake Leman. He gave an historic sketch of the examination of the bed of the lake from Delabèche in 1819 down to the present day, from which it appeared that the knowledge of the central portion is very incomplete, while the rest of the lake is now well known. It is clear that there are two parts in the lake of wholly different character—one small and shallow, the other large, deep, and Alpine in its character. These two are separated by the Yvoise bank or bar, which is really a glacial moraine, as shown by the flints dredged up. These fragments of rock, found sometimes at a depth of 61 metres, are covered with moss of a beautiful green—a fact which appears to demand a reconsideration of the theory that light will not penetrate to more than 25 metres. A discovery in connection with the lake which M. Forel regards as a most interesting one in physical geography is that of a sub-lacustrine ravine through which the Rhone flows. Prof. Forel's long and laborious study of the lake entitled him, the President said, to the title "Prophet of Leman." Dr. Dufresne described the orohydrography of Brazil, and M. Brun recounted his adventures on the Gran Chaco. The Association discussed at some length various questions connected with the teaching of geography, especially the compilation by the allied societies of a manual of geography, and the establishment of geographical museums.

THE current number of the *Verhandlungen* of the Berlin Geographical Society (Bd. xiii. No. 6) contains two papers on the Congo region: one by Dr. Büttner on his journey from San Salvador to the Quango, and thence to Stanley Pool; the other by Lieut. Kund, who, with Lieut. Tappenbeck, was sent out by the German African Society in 1884. Their task was to explore the southern tributaries of the Congo, and to study their navigable qualities between Koango and Kassai. The length of the journey was, in all, 800 German miles, of which 340 was by water, and 460 by land. They succeeded in finding, between Koango and Kassai, three navigable rivers, the Wambu, Saie, and Kiulu; and they regard Lukenje, with its people, as practically a new discovery in the Congo basin. Dr. Joest writes on Minahassa, a peninsula in the north-east Celebes.

THE *Zeitschrift* (Bd. xxi. Heft 3) contains less matter of specially geographical interest than usual. A short paper, with an excellent map, discusses the improvements, which appear to have been great, made in recent years in roads and other means of communication in Asiatic Turkey. The greater part of the number is occupied with an exhaustive examination, by Herr Jung, of the census of India for 1881. The only real geographical paper is a summary of the report presented to the Brazilian Government on the surveys made for the purpose of the frontier between that empire and Venezuela.

THE last number of the *Izvestia* of the Russian Geographical Society (1886, ii.) is of great interest. It contains a beautiful map of the upper course of the Amu-daria, on the scale of 20 miles to an inch, including the space between the 36th and 41st degrees of latitude, and the 66th and 76th degrees of longitude. The whole of the Pamir appears on this map according to the recent surveys and barometric levellings of the Pamir Expedition, while a number of other surveys, including those of M. Kosyakoff (who accompanied Dr. Regel), the astronomical determinations of MM. Scharnhorst, Bansdorf, Schwartz, Skassi, Putyata, and Mr. Forsyth, as also the sketch map "of M. S. in and around Badakshan," have been taken into account. The same issue contains a very interesting paper by M. Grum-Grzymailo on the Pamir region; a paper, by M. Makaroff, on the double currents in straits, and especially in the Bosphorus (being a summing up of papers on this subject published in the *Memoirs* of the St. Petersburg Academy of Science); a most interesting account of the earthquakes at Tokmak in 1885; and, finally, the minutes of the proceedings of the Society brought up to a recent date, that is, embodying the sittings of the Society and its Sections as far as April last.

THE last issue (Nos. 5 and 6, 1885) of the *Journal* of the North China branch of the Royal Asiatic Society has a paper by Mr. Phillips on the seaports of India and Ceylon, described by Chinese voyagers of the fifteenth century, with an account of Chinese navigation. It is illustrated by a very curious old chart said to have been used by Chinese sailors who visited these distant places. In the present paper the route from Sumatra by the Nicobars to Ceylon is described; at a future time the writer will continue the maps to Arabia and Persia. The method of navigation by star charts, one of which is given, is very interesting. The whole paper shows that the Chinese visited these seas long before European navigators found their way there.

THE INSTITUTION OF MECHANICAL ENGINEEERS

THE summer meeting of this Institution for the reading and discussion of papers was held on the mornings of the 17th and 18th inst., at the Theatre of the Institution of Civil Engineers. On the afternoons of these days, and on the 19th and 20th, various works in and about London were visited. The Institution was entertained three years ago by the Belgian Engineers at Liége, and on this occasion Belgian Engineers have enjoyed the hospitality of the London members of the Institution.

The proceedings commenced with a few introductory remarks and a welcome by the President, Mr. Jeremiah Head, after which he read an address, taking as the text of his discourse the "Depression of Trade," to which Dr. Percy referred at the meeting of the Iron and Steel Institute in Glasgow last autumn, attributing it to over-production.

Mr. Head drew attention to the circumstance that mechanical engineers had done their utmost to make possible what had actually occurred, illustrating his remarks by recalling to the minds of the members some of their recent visits to works in various parts of England, where "the advantages of adhesion to a few types, and to but a few sizes of each type, of working to gauges throughout, of the piece-work system, of making for stock as regards all details, and taking from stock when erecting so as to avoid delays, impressed themselves strongly upon the members, who realised what rapid strides had been made in the direction of increased production at diminished cost." He instanced a steam-navvy, which was capable of doing the work of 80 to 120 human navvies, thus turning them into the ranks of the unemployed, and the flooding of our markets with American and Swiss watches, which, according to the evidence of a Liverpool watchmaker, was killing the British industry. The probable causes of these unfortunate circumstances were "diffusion instead of concentration, and adherence to old habits instead of quick appreciation of new and better ones."

Passing from the subject of the aid rendered by mechanical improvements towards over-production, the speaker referred to various commodities we send abroad as affecting our trade. "Some of these commodities may, in their production and sale, beneficially affect us now, and may also bring other benefits in the future; others may be profitable for the time being, but may tend to destroy future trade."

The address was listened to with interest and attention by the members, the meeting being one of the largest that has taken place in the metropolis for some time.

Two papers only were read on this occasion, the one by Mr. Borodin, of Kieff, and the other by Mr. Sandiford, of Lahore, both being on the working of compound locomotives, Mr. Borodin's paper also having reference to steam-jacketing. Mr. Borodin employed Mr. G. A. Hirn's system of investigation, with some modifications necessary to adapt it to locomotives working without condensation. Tests were first made in the locomotive testing-shop, where there was no dynamometer, and as only 90 per cent. could be utilised, high grades of expansion and comparatively low pressures had to be employed. The arrangements made were very complete; pressure-gauges and counters were observed, and indicator-diagrams taken at frequent intervals, the readings of which were tabulated. The results of each one and of all the tests, without exception, indicated a decreased consumption of moist steam when the jackets were working, the effect of the jackets including a decrease in the quantity of steam condensed during admission, a decrease in re-evaporation of water during expansion, and an increase of mean pressure in the cylinders. When variable rates of expansion were employed it was found that the consumption of steam per effective horse-power was larger at the higher rates of expansion, from which the conclusion may be drawn that when cylinders are too large they prevent economy in the consumption of steam.

The second set of trials was made with experimental trains, on ordinary and compound locomotives respectively, with jackets working and not working, but unfortunately as regards these experiments "the great want of success in the attempts to measure the quantity of water condensed in the jackets, as well

as the impossibility of selecting a representative diagram of the mean work of the steam that should be sufficiently accurate for showing the mean power developed in the cylinders during the whole of the test, rendered it hopeless to estimate the effect which the jackets had upon the state of the steam in the cylinders." The comparisons were on this account only available with tests made when the jackets were not at work. The following were the final conclusions arrived at:—For the same consumption of water and fuel per hour, the work done by the compound engine is greater than that done by the ordinary engine; for the same boiler-pressure and the same speed, the increase of work done by the compound engine compared with ordinary engines diminishes in proportion as the total consumption of fuel and water increases.

Mr. Sandiford's experiments were made on two locomotives which had been sent into the shops for heavy repairs, both of them requiring new cylinders. One was arranged with two high- and two low-pressure cylinders, and the other with one high- and one low-pressure cylinder. The compounding of both locomotives was attended with economy in consumption of fuel, and they were decidedly more powerful than the original engines had been, whilst from the drivers' point of view they were not more complicated.

The views held by the members taking part in the discussion agreed generally with those of the authors of the papers as regards the benefits both of jacketing and of compounding locomotives.

After the close of the discussion, the members visited various works in and around London, amongst those most favoured being the Royal Mint, Lambeth Pottery, the Royal Small-Arms Factory (Enfield), the Royal Arsenal (Woolwich), Beckton Gas-Works, the Royal Victoria and Albert Docks, Tilbury Docks, and the Crossness Sewage-Works of the Metropolitan Board of Works.

PROGRESS OF CHEMISTRY AND MINERALOGY

M. FRIEDEL, President of the French Association which met recently at Nancy, gave an address on the progress of chemistry and mineralogy. After briefly referring to the ravages made by death amongst the founders of the Society, and to the prospect of its amalgamation with the Scientific Association of France, M. Friedel proceeded to remark that the progress of chemistry during a period of thirty years had been set forth with masterly clearness and attractive eloquence by the late M. Wurtz at the gathering held at Lille in 1874, and two years later at the Clermont Conference. The theoretical conquests resulting in the discovery of the brilliant coal-tar dyes, the reproduction of alizarin and the other colouring substances of madder, of vanilline (the odoriferous principle of vanilla), of indigo, of the tartaric and citric acids, &c., continue their progressive and pacific career.

The study of countless artificial compounds brings us daily nearer to the natural compounds that have not yet been reproduced, and the most important alkaloids, such as quinine and morphine, seem already almost within the scope of synthetical chemistry. The work that has been undertaken in their regard resembles that of architects engaged in raising stone by stone the plan of some edifice at once of intricate design and difficult access.

This plan once securely established, the reconstruction of the building itself will no longer lie beyond the power of those regular synthetic methods which are daily acquiring greater expansion. It will soon be a mere question of patience and intelligent work, and the time is approaching when quinine and morphine will be produced as readily as alizarin now is. Nay more, there is reason to hope that besides the natural alkaloids others will be obtained endowed with valuable therapeutic properties. While endeavouring to reproduce atropine, whose synthesis he afterwards succeeded in making, M. Ladenburg has obtained homatropine, which produces physiological effects sufficiently distinct to claim, side by side with its homologue, a place amongst the agents employed by oculists. Other less successful essays have also shown in the derivatives of quinoline that patients have perhaps been somewhat prematurely treated with agencies producing a vigorous and very special action on the organism.

If synthetic chemistry has a bright future, we shall also doubtless see the development of another branch of chemistry which has hitherto been comparatively neglected, after having been held in honour at the beginning of the century, and found in Braconnot, of Nancy, an able and devoted cultivator. M. Friedel referred to the research of direct principles, that is to say, of the chemical compounds which exist in animals and plants, and which may be extracted from them. Similar alternatives are often presented in the history of science, which proceeds with irregular leaps, as results from the very nature of things.

The separation of the direct or immediate principles had first to place at the disposal of chemists abundant materials of varied composition, in order to fix their attention on the complexity of organic substances. Then came the time to seek the laws determining their constitution, and now that these laws are sufficiently understood to establish the structure and functions of many of them, the more complete study of their transformations, the more accurate definition of some already determined, the certain discovery of many others still unknown, must attract further attention to the work of those who, like Braconnot, have made a special study of the natural products.

Mineral chemistry has at last succeeded, in the hands of a young and skilful naturalist, in obtaining the isolation of fluorine, which had been in vain attempted by so many other students.

This important result is again due to the process used by Davy for isolating potassium—the decomposing action of the pile. The essay had already been made, but under conditions in which this exceptionally active element reacted on the electrodes or on the vessels. M. Moissan's merit consisted in perceiving that the decomposition should be made at a low temperature, and in the happy choice of the substance to be subjected to the process of electrolysis—hydrofluoric acid made conductive by the addition of fluoride of potassium. In the gaseous current disengaged at the positive pole, crystallised silicium and boron burn at the ordinary temperature, iodide and chloride of potassium are decomposed, mercury and other metals transformed to fluorides, organic compounds carbonised or inflamed, while water absorbs the gas, yielding in its place ozonised oxygen. Thus is produced a large number of reactions, whose study promises a most interesting sequel to this brilliant discovery.

Physico-chemical research continues on its part to furnish means of investigation enabling us to penetrate more deeply into the very life of the chemical molecule, that is, those inner movements whose existence must now be admitted.

Spectroscopy, which has just yielded to M. Lecoq de Boisbaudran two new metals, reveals, by the comparison of the rays, a connection, which is assuredly far from accidental, between the various elements of the same family.

Thermo-chemistry, after having, in the hands of M. Berthelot and M. Thomsen, given the reason of most reactions, now approaches the study of isomeric bodies. M. Bouty's researches on the conductivity of the solutions of salts, and those of M. Raoult on the lowering of the freezing-point of the various solutions, seem to supply fresh means for determining the molecular weight of compounds.

But our attention must now be directed to mineralogy, a far less popular science than chemistry.

After being held in considerable esteem at the close of the last and beginning of the present century, when Werner's labours enabled mineralogists to describe and methodically classify the rich materials accumulated in collections, mineralogy lost its votaries according as it became more scientific. The immortal labours of Haüy, of Berzelius, and the chemical school, seem to have scared the amateurs, who probably saw in mineralogical collections little more than so many picturesque specimens distinguished by their diversified colours and fantastic forms.

Mineralogy presents the special character that it profits by the progress of chemistry and physics, for which it has itself often enough supplied the starting-point. Aiming especially at the description of crystallised minerals, it applies to this description methods which are afterwards profitably transferred to the domain of artificial products.

Thus it has given birth to crystallography, which establishes the laws determining the formation of crystals—those marvellous products of the mineral world in which Haüy recognises the regular aggregations of infinitely minute particles.

This regularity of structure, indicated at once by their outward form, has been confirmed by the study of their many physical properties, especially that of their action on light. From this study has been derived one of the safest and most fruitful processes by which the inward architecture of crystals has

been revealed. This consists in examining their action on polarised light, that is, on light which, by reflection or refraction under suitable conditions, has acquired special properties, and become incapable of being reflected or refracted like ordinary light, except under certain well-defined conditions.

To use a somewhat crude comparison, the luminous ray, after traversing certain media, assumes the appearance of an iron rod that has been passed through a rectangular drawing-frame. If on leaving the frame it meets an opening of like form and size, it will pass through without difficulty; but if the opening be placed crosswise, it can no longer pass.

There is this difference between the rod and the ray—that in all the intermediate positions a portion of the latter will pass through, the quantity increasing according as a more parallel disposition is assumed. Hence, if we take two apparatus corresponding to the frame and the opening, one of which supplies the polarised ray and the other intercepts it at right angles, the result will be complete darkness on the field of the instrument. But if we now place between both a crystalline plate of some substance which does not crystallise in the cubic form, we shall generally see the dark field illumined and often assuming the most lovely colours—an effect due to an action discovered by Arago and explained by Fresnel. With a homogeneous crystal, and when the light falls in parallel pencils on the plate, a uniform tint is diffused over the whole field of the instrument. If the crystal be not homogeneous, but formed of diverse parts jointed or regularly grouped together, but in positions not parallel, we shall get different tints for the different parts. By turning the crystal round, certain coloured strands will be extinguished, as we say, that is, will cease to transmit the light, while others will remain luminous. Hence we have here an extremely delicate and accurate means of studying the structure of crystals in their most intimate details. Haüy had already remarked that all crystals are doubly refracting, except those belonging to the cubic system. Brewster soon after thoroughly established the relation that exists between the optical properties and crystalline symmetry, stating, amongst other points, that cubic crystals alone have no action on polarised light. Nevertheless, observation had shown that certain substances affecting the cubic form had such action, and illumined the obscured field of the polarising apparatus. Biot had even suggested a term to designate, if not to explain, this exception, calling it "lamellar polarisation."

To the researches of M. Mallard we are indebted for the true account of this anomaly, which in fact he has explained away. He shows that the cubic crystals acting on the polarised light are not really cubic, but formed by the regular grouping of parts belonging to other crystalline systems. Boracite, for instance—chloroborate of magnesium usually taking the form of rhombic dodecahedra, that is, a solid of twelve equal rhombs belonging to the cubic system—is formed by the union of twelve straight pyramids with rhombic bases, whose summits unite in the centre of the crystal, and whose bases are the rhombic facets.

M. Mallard's beautiful experiments with parallel rays have been confirmed by those of M. Emile Bertrand with convergent rays, showing in isolated portions of the garnet and of boracite all the properties belonging to regular crystals of orthorhombic substances.

There can be no doubt as to the correctness of the explanation given by M. Mallard of the optical anomalies of crystals which had been regarded as cubic, but which have once more served to illustrate the trite remark, "Trust not appearances."

The optical investigation of crystals, due mainly to the late M. de Senarmont, has become a familiar process which no mineralogist can henceforth afford to neglect.

These same methods, employed with much greater magnification than in Amici and Nörremberg's primitive appliances, also render the greatest services to the geologist in the study of rocks. They enable him to determine with an otherwise unattainable accuracy the minutest elements of these formations, in which minerals are intermingled in diverse proportions. After Sorby, the pioneer in this line of investigation, Zirckel and Rosenbusch in Germany, Fouqué and Michel Lévy in France, have turned to the best account the new method, which has thrown much light on the origin and mode of formation of certain rocks, by showing what substances were first solidified and what parts resisted longest the cooling process.

All these determinations are aided by the study of the optical sign of crystals—that is, the relative velocity with which the two polarised rays are propagated in certain directions—the observation of the position of the axes wherever possible, that of dichroism, and even the approximate measurement of the indices of refraction.

This last has been much facilitated by an instrument recently devised by M. Emile Bertrand. With a transparent or opaque plate of some crystallised substance, and by means of not more than four readings made in two positions of the crystal, we obtain, by the determination of the angle of total reflection, the two or three indices, and consequently the wave-surface of the crystal for all bodies not having too high an index of refraction. And these operations, hitherto impracticable except with prisms or plates of great size, may now be made on extremely small crystals, such as those of rocks.

But however paramount the importance of optical properties, others also claim attention in crystallo-physics. Although of less practical interest in the determination of crystals, they may still open up many new avenues of inquiry to the physicist.

The curious property possessed by some hemihedral minerals of becoming charged with electricity with contrary signs at the two extremities of certain axes when heated or chilled has long been known. MM. J. and P. Curie have now shown that compression on the same crystals acts like the cooling, depression or traction like the heating process. In both cases the phenomenon appears due to the greater proximity or distance of the molecules. It is remarkable that the phemonenon may be reversed, so that hemihedral crystals with inclined facets properly charged with electricity, positive at one and negative at the other extremity, will contract or expand as the case may be.

As regards synthetic mineralogy, it is now known, thanks mainly to the researches of Berthier, Becquerel, Senarmont, H. Sainte-Claire Deville, and Daubrée, that minerals may be reproduced in our laboratories, and that we already possess a valuable means of study, enabling us to understand the conditions in which the natural minerals and their compounds may have been produced. We are thus advancing towards a chemical knowledge of certain species, whose formula analysis alone has failed to establish, and it may even soon be possible to produce useful substances under the very form from which they derive their properties.

The observation of the crystalline products accidentally formed in the metallurgic furnaces first led to this line of study, the firstfruits of which Mittscherlich and Berthier obtained by fusion.

By melting certain silicates, certain rocks or substances with the same chemical composition, and then exposing this vitreous mass to a temperature somewhat lower than that of fusion, MM. Fouqué and Michel Lévy have succeeded in reproducing the identical minerals found in lavas, basalts, and other eruptive rocks. Such are the anorthite and labrodorite feldspars, amphigene, pyroxene, peridot, magnetic iron, &c.

The case is otherwise with the granites, the problem of whose origin is far more difficult to solve. Nevertheless, of their three constituents two have already been artificially obtained.

Quartz had long ago been reproduced by Senarmont by heating gelatinous silica with a solution of hydrochloric acid to about 300° C. But Hautefeuille was the first to obtain fine crystals of orthoclase and albite feldspars by heating silica with alumina and the necessary alkalies in presence of a solvent such as a fused alkaline vanadate or tungstate.

But the conditions of this beautiful experiment do not appear to have been realised in nature. The nearest approach to them was probably the series of essays made by our President jointly with M. Edmond Sarrasin, by heating a solution of alkaline silicate with a precipitated silicate of alumina to nearly 500° C. in a strong steel tube lined on the inside with platinum. According to the alkalies and proportions employed, the result is albite or orthoclase mixed or not with quartz, the crystals resembling those occurring in nature and presenting the same peculiarities of form and grouping. The well-ascertained presence of drops of water in the granitic quartz seems to show that these granites must have been formed in the presence of aqueous solutions. Thus the natural conditions have already been approached, but will not be entirely realised until the hitherto recalcitrant mica has been obtained.

The first essays at reproducing the zeolite group of minerals have been made by De Schulten, who, by heating the silicate of soda in tubes of aluminous glass, has procured small icositetrahedra of analcime, such as occur in the lavas of the Cyclops Islands.

As regards precious stones, the solution of the problem from the scientific, if not the economic, standpoint, was long ago

given for spinel and corundum by Gaudin, Ebelmen, H. Sainte-Claire Deville, and Caron. More recently, MM. Frémy and Feil have prepared the ruby in large crystalline masses unsuitable for cutting, although possessing all the properties of the natural mineral.

Fresh essays seem to have led to more practical results, as for some time past rubies of fair dimensions are met with in the trade, which, although rather less brilliant and transparent, possess the hardness, density, and optical properties of this valuable gem. Several features of their inner structure show conclusively that they were obtained by fusion; in any case it is well known that, unlike silica, which remains vitreous, alumina crystallises by fusion.

The diamond alone appears to have hitherto resisted all attempts at reproduction. Although success in this direction has been frequently announced, the statement has always proved erroneous. The problem is rendered more difficult from the fact that the diamond has nowhere been found in its original lode. This holds good as well for the Brazilian itacolumites and quartzites, and for the serpentine breccias of South Africa, as for the diamantiferous sands. Nevertheless, in the diamond are occasionally embedded some foreign substances, which, while depreciating its commercial value, are very interesting as showing that it must have been formed at a relatively low temperature.

But enough has been said, M. Friedel concluded, to enlist your interest in mineralogy, a science whose progress has been rapid, whose methods are being constantly renewed, and which in every respect deserves the attention of inquiring minds.

SCIENTIFIC SERIALS

American Journal of Science, August.—On hitherto unrecognised wave-lengths, by S. P. Langley. The object of the laborious and delicate operations here fully described has been, not so much to settle the theoretical questions involved in determining the relation between dispersion and wave-length, as to enable future observers to determine the visible or invisible wave-lengths of any heat, whether from a celestial or terrestrial source, observed in any prism. A knowledge will thus be gained of the intimate constitution of radiant bodies, which an acquaintance with the vibratory period of their molecules can usually alone afford. These researches into the whole unexplored region of infra-red energy both from celestial and terrestrial sources have led to the certain determination of wave-lengths greater than 0·005 mm. Radiations have also been recognised whose wave-length exceeds 0·03 mm., so that, while the wave-length known to Newton has been directly measured to nearly eight times, there is probable indication of wave-lengths far greater. The gulf between the shortest vibration of sound and the longest known vibration of the ether has thus in some measure been already bridged over.—On the chemical composition of herderite and beryl, with note on the precipitation of aluminium and separation of beryllium and aluminium, by S. L. Penfield and D. N. Harper. The composition of herderite is shown to be an isomorphous mixture of $CaBeFPO_4$ with $CaBe(OH)PO_4$, which may be written $CaBe(FOH)PO_4$, or a salt of phosphoric acid, two of whose hydrogen atoms have been replaced by a bivalent element, and the third also by a bivalent element whose other free affinity has been satisfied by a fluorine atom or hydroxyl. Chemically it is closely related to wagnerite, triplite, and triploidite, these minerals offering the best illustration of the isomorphism of F and OH. In crystallisation, herderite is orthorhombic, with a prismatic angle of nearly 120°. Regarding water as an essential constituent of beryl, the authors add $\frac{1}{2}H_2O$ to its usually accepted formula, writing it $H_2Be_6Al_4Si_{12}O_{37}$. Its theoretical composition, according to this formula, becomes SiO_2 65·81; Al_2O_3 18·83; BeO 13·71; H_2O 1·65. Specific gravity, 2·706.—Communications from the U.S. Geological Survey, Division of the Rocky Mountains, by Whitman Cross and L. G. Eakins. The present paper deals with ptilolite, a new mineral occurring in cavities of a more or less vesicular augite-andesite, which is found in fragments in the Tertiary conglomerate beds of Green and Table Mountains, Jefferson County, Colorado. It is described as a white substance in extremely delicate tufts and spongy masses composed of short hair-like needles loosely grouped together; hence its proposed name of ptilolite, from the Greek πτίλον = *down*, in reference to the light downy nature of its aggregates. Its empirical formula is $RO,Al_2O_3,10SiO_2 + 5H_2O$, R representing Ca, K_2, and Na_2; it is thus an alumino-silicate of which no previously described hydrate contains so high a percentage of silica. In this respect it may compare with the rare mineral milarite.—Notes on the peridotite of Elliot County, Kentucky, by J. S. Diller. This formation, described as a dark greenish rock with specific gravity 2·781, appears to be undoubtedly of eruptive origin, traversing many thousand feet of palæozoic strata to reach the surface. Its mineralogical composition shows 40 per cent. of olivine, 30·7 serpentine, 14 dolomite, 8 pyrope, 2·2 ilmenite, 2 magnetite, with smaller quantities of biotite, enstatite, octahedrite, and apatite.—Temperature observations at the Lake Superior Copper-Mines, by H. A. Wheeler. The unusually low thermal gradients recorded in these mines—about 1° F. to 100 feet as compared with the normal of 1° F. to 50 or 55 feet in vertical descent, is attributed to the proximity of the cold waters of Lake Superior. The nearer the mines are to this great cooling influence, the lower the thermal gradient will be found to be.—An application of the copper reduction test to the quantitative determination of arsenic, by Henry Carmichael. Using a standard square of copper as an indicator, the author has been led to adopt the method here described, which, for the estimation of small quantities of arsenic in the human system or elsewhere, he believes to be quicker, simpler, more delicate, and, in the hands of toxicologists, less exposed to fallacy, than any other. A copper square 1 millimetre on a side detects ·0000025 grm. arsenious oxide, a quantity 400 times less than that necessary for turning the beam of the ordinary chemical balance.—On the crystallisation of gold, by Edward S. Dana. The paper deals more especially with the delicate crystalline threads and arborescent forms from the White Bull Mine, Oregon, and the specimens of finely crystallised gold from the Californian mines. The crystals are illustrated and fully described.—Classification of the Cambrian system of North America, by Charles D. Walcott. The formations here treated are those characterised by the predominance of the types of Barrande's "First Fauna," and such additional strata, not characterised by the presence of fossils, as are stratigraphically and structurally connected with the Cambrian strata identified by organic remains. These formations, showing a total thickness of over 18,000 feet, with a known fauna of 92 genera and 393 species, are regarded, not as a subdivision of the Silurian, but rather as a well-defined geological system underlying the Lower Silurian (Ordovician) on the North American continent.—Note on the spectrum of Comet *c*, 1886, by O. T. Sherman. When observed with the equatorial of Yale Observatory in May and June, this comet presented no less than seven loci of light where three only are usually seen. These showed approximate wave-lengths 618·4, 600·6, 567·6, 553·7, 517·1, 468·3, and 433·2, besides strongly suspected loci at 545·4, 535·0, 412·9, and 378·6. These are compared with the low temperature spectrum of carbohydrogen, and it is suggested that a chart should be prepared for the carbon compounds at successive heat-levels, after the manner of that drawn up by Lockyer for the photographed spectra of some carbon compounds (*Proc. Roy. Soc.*, xxx. p. 463).

Annalen der Physik und Chemie, Bd. xxviii., No. 7, July 1886.—Th. Schröder, experimental investigation of the influence of temperature upon elastic reaction. The experiments were made with three wires, respectively of silver, iron, and german silver. The elastic reaction was greatest with the first, least with the last of these, and the change in the elastic reaction produced by change of temperature followed the same order.—E. Warburg, remarks on the pressure of saturated vapour. Discusses relation between vapour-pressure and curvature of liquid surface.—W. Fischer, on the pressure of saturated vapours above liquid and above solid substances. The substances chosen were ice and water. The difference, for ice, between the two differential coefficients of pressure with respect to temperature for saturated steam over ice and saturated steam over water is 0·0465, at the melting-point, where the two curves meet. For benzol the two curves do not meet at its melting-point.—A. Schrauf, on dispersion and axial density in prismatic crystals; and on the properties of trimetric crystals. The latter shows the existence of a relation between coefficients of expansion, axial density, and the parameters of the crystal.—A. Toepler, some lecture experiments on waves. A small gas flame is used to show the propagation of a wave of compression in a long tube filled with air, and provided at one end with an india-rubber pear. Several interesting experiments can be shown.—E. Cohn

and L. Arons, conductivity and dielectric constants. An investigation as to whether the dielectric constant of a conductor is infinite, as often stated in text-books. A condenser was arranged to be filled with mixtures of anilin and benzol, xylol, mixtures of anilin and xylol in various proportions, &c., liquids being selected to avoid as far as possible residual charges. The capacities of this condenser were compared with that of an air-condenser, and arrangements were also made to measure the resistances on the bridge. The resistances of the three mixtures of anilin and xylol were 224,900, 1,383,000, and 18,780,000 Siemens's units, and their dielectric constants 1·590, 1·443, and 1·336. The authors conclude from these and other experiments that there is no necessary relation between the two constants; and, further, that the wide differences observed by Hopkinson between the square root of the dielectric constant and the index of refraction in certain vegetable oils cannot be explained by the conductivity.—E. Hoppe, on the theory of unipolar induction: experimental verification of Edlund's theory of the origin in terrestrial magnetism of auroral phenomena.—H. Jahn, on the equivalence of chemical energy and current energy. A discussion of Helmholtz's theory of the secondary heat of a voltaic element.—H. Jahn, on galvanic polarisation. A study of changes of polarisation of certain liquids with changes of temperature, together with deductions verifying Helmholtz's equations.—G. Adler, on the energy of magnetically polarised bodies.—E. Ketteler, addendum on the total reflection of crystals.—F. Koláček, on the gold-leaf spectroscope. An attempt to calculate a calibration of the electroscope from its electrostatic capacity.

THE number of the *Nuovo Giornale Botanico Italiano* for April 1886 contains a number of short articles on various points relating to the flora of different parts of Italy, both phanerogamic and cryptogamic.—Sig. A. Mori describes and figures a singular instance of the production of a pitcher-like structure on the upper surface of a leaf of *Gunnera scabra*.—Dr. O. Mattirolo has examined the "mycorhiza" of the roots of the sweet chestnut, and finds it to furnish a new example of polymorphism among the Hypocreaceæ. It consists largely of two species of fungus, which he names *Melanospora stysanophora* and *M. Gibelliana*. The former is identical with *Stysanus Steminotis* and also with an *Acladium*, and presents an instance of "apandry," or the production of ascospores independently of the previous formation of a male organ. *M. Gibelliana* produces chlamydospores, and also the peculiar structures known as "spore-bulbils," which appear to replace the true ascophorous perithecia.

The most interesting article in the number for July is by Sig. A. Piccone, on the plants growing wild in Liguria which he terms "zoophilous" or "ornithophilous," *i.e.* those which are absolutely dependent for the germination of their seeds on the fruit being swallowed by birds.

SOCIETIES AND ACADEMIES

LONDON

Entomological Society, August 4.—Prof. J. O. Westwood, M.A., F.L.S., in the chair.—The following gentlemen were elected Fellows:—Lord Dormer, Messrs. J. H. A. Jenner, James Edwards, Morris Young, F. V. Theobald, E. A. Atmore, and William Saunders, President of the Entomological Society of Ontario.—Mr. Theodore Wood exhibited and made remarks on the following Coleoptera: an abnormal specimen of *Apion pallipes*; a series of *Langelandia anophthalmi*, from St. Peter's, Kent, taken in decaying seed-potatoes; a series of *Adelops wollastoni*, and *Anommatus 12-striatus*, also from decaying seed-potatoes; and a series of *Barypeithes pellucidus*, from the sea-shore near Margate. Mr. Wood also exhibited, on behalf of Dr. Ellis, of Liverpool, a specimen of *Apion annulipes*.—Prof. Westwood exhibited five specimens of a species of *Culex*, supposed to be either *C. cantans* or *C. lateralis*, sent to him by Mr. Douglas, who had received them from the Kent Water-Works. It was stated that they were very numerous in July last, and that persons bitten by them had suffered from "terrible swellings." Prof. Westwood also exhibited some galls found inside an acorn at Cannes in January last.—Mr. Billups exhibited a male and female of *Cleptes nitidula*, taken *in copulâ* in July last, at Benfleet, Essex, on the flowers of *Heracleum sphondylium*. He stated that it was probably the rarest of the twenty-two known species of British *Chrysididæ*, though it had been recorded from the New Forest and from Suffolk.—The Rev. W. W. Fowler announced that a series of specimens of *Homalium rugulipenne* had been received from Dr. Ellis, of Liverpool, for distribution amongst Members of the Society.—Mr. White exhibited a group of three specimens of *Lucanus cervus*, consisting of a female and two males. The female was *in copulâ* with one of the males, which, while so engaged, was attacked by the second male.—Mr. E. A. Fitch read a paper, communicated by Mr. G. Bowdler Buckton, on the occurrence in Britain of some undescribed *Aphides*.—Prof. Westwood read a paper on a tube-making homopterous insect from Ceylon.—Mr. Theodore Wood read a paper on *Bruchus*-infested beans. A discussion ensued, in which Prof. Westwood, the Rev. W. W. Fowler, and Messrs. Weir, Fitch, and Trimen took part.

PARIS

Academy of Sciences, August 16.—M. Fizeau in the chair.—The Secretary announced the loss sustained by the Academy in the death of M. Laguerre, Member of the Section for Geometry, who died on August 13 at Bar-le-Duc. At the obsequies, which took place on August 16, the Academy was represented by M. Halphen.—Remarks on the recent volcanic disturbances in the Northern Island, New Zealand, by M. Emile Blanchard. The author pointed out that this sudden display of igneous activity was a remarkable confirmation of the views already advocated by him in 1882 and 1884 on the subsidence of an austral continent during the modern geological age of the earth. He regarded the New Zealand Archipelago and more or less adjacent islands as a remnant of this continent, or at least of an extensive region, which had existed in a comparatively recent epoch, and he had already, in 1884, anticipated fresh convulsions, such as the tremendous catastrophe of which New Zealand had been the scene after a lengthened period of quiescence. Tarawera and other volcanoes supposed to be extinct have suddenly broken out into fresh activity; lava-streams have overspread vast spaces, and a romantic tract of country, the delight of the early explorers, has been wasted or swallowed up. Although the exact change that has taken place in the aspect of the land cannot be fully known for some time to come, the event already appears as an illustration of still more violent outbursts, which occurred in more or less remote ages. Thus it has been shown in this instance that the inductions drawn from a recently-created science already bear the character of certainty.—On the differential equation of a curve of any order, by Prof. Sylvester. It is shown that a direct and universal solution may be had of the following problem: To find the differential equation of a curve of the order n, where the function of the equation (with unity for constant term), whether U or $(x, y, 1)^n$ is represented under the symbolic form u^n, where $u = a + bx + y$. It is added that the formulas given by M. Halphen in his "Recherches des points d'une courbe algébrique plane," &c., lead to the same results as those here arrived at.—On the employment of intermittent light for the measurement of rapid movements, by M. Gustave Hermite. Indicators of velocity at present in use always absorb a portion of the force of the machine to which they are applied. The author proposes to avoid this inconvenience by the arrangement here described, which, by an ingenious application of intermittent light, enables the observer to measure not only the number of revolutions of any machine, but the velocity of any rapid movement whatsoever, without exercising any mechanical action on the apparatus under examination.—On the mono-substituted haloid derivatives of acetonitril, by M. Louis Henry. The researches undertaken by the author on the functional solidarity and the volatility of the carbon compounds have led him to complete the series of these derivatives. Here he describes mono-ioduretted acetonitril, ICH_2—CN, and monobromuretted acetonitril, $BrCH_2$—CN, reserving for a future communication the comparative study of the mono-substituted haloid derivatives of acetonitril and acetate of methyl.—On the composition of the mineral waters of Bagnères-de-Luchon, Haute-Garonne, by M. Ed. Willm. It is shown that carbonic acid, far from being a negligible quantity in these and similar waters, as was supposed by the late M. Filhol, mostly occurs in a proportion more than sufficient to give a quantity of bicarbonates corresponding to the alkaline property of the water, independently of that which is due to the sulphuret. A complete analysis yielded sulphuric and carbonic acid, chlorine,

silica, ferric oxide, aluminium, sodium, potassium, calcium, magnesium, and traces of iodine, lithium, copper, ammonia, manganese, phosphoric and boric acid, but no arsenic.—Priestley's experiment repeated with aquatic animals and plants, by M. N. Gréhant. The experiment here referred to consists in placing under an air-tight vessel small mammals, such as mice, until the atmosphere becomes vitiated by the absorption of oxygen and liberation of carbonic acid; then, if a sprig of mint be introduced and the vessel exposed to the sun, after a certain time a mouse again introduced will breathe and live freely, the carbonic acid having been decomposed by the chlorophyll under the influence of the light and replaced by oxygen. An analogous experiment is here described with fish, and the leaves of an aquatic plant (*Potamogeton lucens*) introduced into receptacles filled with water.—Atmospheric phenomena observed at Palermo during the recent eruption of Etna, by M. A. Riccò. These light-effects are compared with those following the eruptions of Krakatão and Ferdinandea, their less brilliant character being attributed to the smaller quantity of vapours discharged by Etna.—The telluric currents, their nature, and the part played by them in the production of meteorological phenomena, by M. J. J. Landerer. In this paper, which is supplementary to the communication made to the Academy on October 17, 1881, the author gives the further results of the studies which he has now prosecuted for several years at Tortosa on the telluric currents and their various relations to terrestrial magnetism, the trade-winds, the solar spots, and the like.

BERLIN

Chemical Society, June 21.—A. W. Hofmann, Vice-President, in the chair.—Prof. Scheibler described in a long and very interesting paper his new methods of obtaining a product rich in phosphorus from the crude slag produced in Thomas's process. Whilst formerly the slag was extracted with dilute hydrochloric acid, and a precipitate rich in phosphorus obtained by adding lime to the solution, the present price of hydrochloric acid rendered it desirable to simplify the process. It was at first attempted to do this by a fractional solidification of the fused slag, the portion first solidifying containing little phosphoric acid, whilst the liquid portion separated from it furnishes an excellent material for manure. An essentially better method consists in adding the lime to the iron, not all at once, but first of all about two-thirds of the necessary quantity; the slag produced is then removed, the remainder of the lime added, and the process completed. The first lot of slag obtained in this way contains about 31 per cent. phosphoric anhydride, and 58 per cent. lime, whilst the second lot contains but little phosphorus, though it is rich in iron, of which it contains 24 per cent., the first slag having only 1·8 per cent. The second slag is returned to the furnaces used in the production of crude iron. The advantages of the method are a shortening of the blowing operation, the possibility of increasing the charge, a more complete removal of the phosphorus, less loss of iron, and considerable saving of lime. Further advantages are that the first portion of the slag forms a valuable manure, whilst the second portion is used again in the furnaces. The paper concludes with a discussion of the importance of this more complete separation of the phosphorus from iron ores for agricultural purposes.—W. Will exhibited an aromatic ketone obtained from the root of *Pæonia Moutan*, and which has been more closely examined by Prof. Nagai, of Japan. Its composition is

$$C_6H_3\begin{cases}COCH_3 & (1)\\ OH & (2).\\ OCH_3 & (4)\end{cases}$$

—There were two papers by J. Traube: (1) on the size of maximum-drops of the ordinary alcohols and fatty acids, and their aqueous solutions; and (2) on the dependence of the size of drops on external influences.—K. Polstorff has found that East Indian bolarrhena contains conessine, and he considers that this alkaloid is identical with Haines's wrightine.—K. Heumann and E. Mentha have studied the behaviour of monochlorazo- and hydrazobenzene to acids; the latter yields chlorazobenzene, parachloraniline, and aniline.—Piutti has obtained a new asparagin from vetch sprigs; its aqueous solutions are dextrorotatory, and its compounds have the same rotatory power as the corresponding compounds of ordinary asparagin, but the rotation is always in the opposite direction.—T. Salzer described a new method of obtaining pentathionic acid by oxidising a solution of sodium thiosulphate with iodine in the presence of potassium arsenite.—M. Lange described a new synthesis of mixed azo-dyes from aromatic diamines.—G. Ciamician and P. Silber have studied the action of pyrroline on alloxan, and have described the properties and derivatives of pyrrolalloxan, $C_8H_7N_3O_4$, which results from the reaction.—J. Schmid showed that the colouring-matter of fisetwood (*Rhus cotinus*, L.), is not identical with quercitine, and that it is present in the plant as a glucoside (*fustin*); he described the preparation and the ethyl- and acetyl-derivatives of fisetin.

STOCKHOLM

Geological Society, April 1.—Prof. W. C. Brögger gave an account of the volcanic rocks extending between Langesund, in the Christiania Fjord, and Lake Mjösen, in Central Norway, founded on earlier and his own researches, the latter extending over many years. He had come to the conclusion that the whole basin was due to an erosion of the earth's crust, which had forced up the volcanic matter. The oldest of these, the augite porphyry, had been discharged in the form of lava streams over the Devonian surface of the earth. The more recent ones had not reached the surface, but had hardened at lower depths, and had become disclosed at a later date.—Herr A. E. Törnebohm described the remarkable coal-bearing rock which was discovered by Igelström some twenty years ago in the crystalline slates of the fundamental rock at the Nulla Mountain, in the province of Varmland. His microscopical researches went to show that the coal had been introduced into the rock whilst the formation of feldspar was still in progress.

BOOKS AND PAMPHLETS RECEIVED

"Proceedings of the American Association, 34th Meeting" (Salem).—"Quarterly Journal of Microscopical Science." August (Churchill).—"Proceedings of the Linnean Society of New South Wales," 2nd series, vol. i. part 1 (Cunningham, Sydney).—"Journal of the Royal Microscopical Society," August (Williams and Norgate).—"British Petrography," part 7, by J. J. H. Teall (Watson, Birmingham).—"Political Science Quarterly," vol. i. parts 1 and 2 (Ginn and Co., Boston).—"Avifauna Italica," by E. H. Giglioli (Firenze).—"Journal of the Asiatic Society of Bengal," vol. lv. parts 268 and 269.—"Annual Report of the Department of Revenue, Settlement, and Agriculture, 1884-85" (Madras).—"Schriften der physikalisch-ökonomischen Gesellschaft zu Königsberg i. Pr."—"Pond Life: Insects," by E. A. Butler (Sonnenschein).—"Papers in Inorganic Chemistry," by G. E. R. Ellis (Rivingtons).—"Report on the Meteorology of India in 1884" (Calcutta).—"Indian Meteorological Memoirs," vol. iii. part 1, vol. iv. part 1 (Calcutta).—"Bergens Museums Aarsberetning, 1885" (Bergen).

CONTENTS

THURSDAY, SEPTEMBER 2, 1886

CHEMICAL PHYSICS

Elements of Chemical Physics. By Josiah P. Cooke, Jr., Erving Professor of Chemistry and Mineralogy in Harvard University. Fourth Edition. (London: Macmillan and Co., 1886.)

THE preface to this work bears the date Feb. 1, 1860; no explanation is given of the issue in 1886 of a *fourth* edition in the form before us. According to the dictionary, an edition is "the whole number of copies of a work published at once." We are forced to assume that such a definition would be accepted as sufficient by Prof. Cooke; but in the case of educational works dealing with a branch of science which is daily enriched by new discoveries, the reading public are in the habit of supposing that a *new* edition is not a mere reprint of the former edition: it is rightly expected that it shall take notice of, at all events, the more important of recent discoveries, and failure to recognise this elementary truth is unpardonable.

It is a grave injustice to put into the hands of the innocent student a book dated 1886 which was first issued in 1860, and without informing him that he is to learn nothing of the classical discoveries in chemical physics made during the past quarter of a century: on advancing to do battle in the ranks of science he would be about in the position of our old wooden walls in face of modern ironclads and torpedo-boats.

The work is divided into five chapters, the first (9 pp.) being introductory. Chapter II. (107 pp.) deals with the general properties of matter; Chapter III. (309 pp.) is on the three states of matter; heat is the subject of Chapter IV. (235 pp.); and Chapter V. (24 pp.) is on weighing and measuring. The volume is intended—according to the 1860 preface—to furnish a full development of the principles involved in the processes of weighing and measuring small quantities of matter; subsequent volumes were to treat on light in its relations to crystallography; on electricity in its relations to chemistry; and on stoichiometry and the principles of chemical classification; but we are not aware of their issue. Unlike most text-books, it is a most readable work, containing much interesting matter, and is admirably printed; hence it is particularly to be regretted that it presents so imperfect and antiquated an account of the subjects treated of. In justification of this statement we may point out that the latest reference to experiments on the condensation of gases is to those of Natterer. Andrews's great discovery of the critical point is nowhere referred to. Regnault's determinations of the specific heat of carbon in its three forms are quoted, but not a word is said of Weber's important investigation on this subject; in the section in which the methods of determining specific heat are described no reference is made to Bunsen's beautiful method; and the word dissociation nowhere occurs, the classical discoveries of Deville and his school being unmentioned. One inaccuracy requires special mention. We are told (p. 428) that, "According to the modern theory of chemistry, equal volumes of all substances in the state of gas contain precisely the same number of molecules, *or, what amounts to the same thing, the molecules of all bodies in the state of gas occupy exactly equal volumes.*" The latter part of the sentence, which we have italicised, is obviously unmitigated nonsense, yet it is a statement which perpetually haunts us. A well-known standard text-book of chemistry from which probably a very large proportion of our youth gain inspiration in fact tells us almost in the same words that "the relation existing between the volumes of gases when they combine together has been found to be a very simple one, inasmuch as the densities of all elements known in the gaseous state are identical with their atomic weights; *or, what is the same thing, the atoms in the gaseous state all occupy the same space* (Gay Lussac, Avogadro)." Could anything be more misleading and inaccurate, since mercury, phosphorus, sulphur and iodine are all known "in the gaseous state"? It cannot be too clearly stated that the *chemist* only concerns himself with the *relative weights* of atoms and molecules, and that Avogadro's law only has reference to the *relative numbers* of molecules in equal volumes of gases, their size being altogether left out of account: no notice being taken, in fact, of the space which the stuff itself occupies. Our notions on the subject of the size of molecules and atoms are of the very vaguest at present, and even Sir William Thomson has not ventured, we believe, to consider the differences in size of molecules of different kinds: for the most part they certainly cannot be of the same size. The popular method of teaching the volumetric relations between gases is probably the cause of the error now referred to being steadily perpetuated. The ordinary student more often than not will insist—very naturally—in regarding the term volume as representing a specific quantity: being taught, moreover, to consider the symbol of a compound gas as equivalent to "two volumes," he extends the idea of volume to the elementary symbols and naturally enough concludes that if I_2, for example, represent *two* volumes, I must represent *one* volume; hence it is difficult to make him realise that a change such as is represented by the equation $I_2 = I + I$ involves a doubling of the volume. Again, the common practice of speaking of compound gases as *containing* certain volumes of the constituent gases is both misleading and inaccurate: for example, it is found that the gaseous density of phosphorus is such that its molecular composition is expressed by the formula P_4: this represents "two volumes," and so the symbol P is said to represent "half a volume," and we are then gravely told that "two volumes" of phosphuretted hydrogen *consists* of or *contains* "three volumes" of hydrogen and "half a volume" of phosphorus. Some teachers engrave the error still more deeply upon the student's mind by performing before him a kind of Jack-in-the-box trick, picking out from a box on which the symbol of the compound is painted a series of boxes which in number and size are supposed to represent the volume of the elements contained in the compound. All that we really know is that a given bulk of a particular gas will decompose into, or can be formed from, certain bulks of the constituent elements: that phosphuretted hydrogen, to take the example cited, on decomposition yields one and a half times its bulk of hydrogen and half its bulk of phosphorus gas. The

only method by which we could endeavour to form any estimate of the relative volume occupied in a compound by the constituent atoms would be by taking the molecular volume of the compound and the atomic volumes of the constituents into account; but this is never done. It would be a great advantage if we were to cease using the term *volume* when speaking of gases, and were to treat all questions concerning changes in volume from a dynamical point of view: Avogadro's law tells us, in fact, that, in every change in which gases are concerned, the variation in the space required to contain the gases—pressure and temperature remaining constant—will be directly as the variation in the number of gaseous molecules. If the equation be written which expresses the change, care being taken to employ the symbols which represent the molecular composition of the gaseous substances concerned in it, it is merely necessary to add up the number of molecules of the gaseous substances appearing on either side: the two sums will give the ratio of the volumes before and after change. By our present practice thoroughly false conceptions are engendered: a fundamental principle of our science is most unscientifically taught: and yet when we come to examine we marvel at the stupidity displayed by the examinee when dealing with simple questions of volume change!

It will be in place here to inquire What is *chemical physics?* To judge from the books on the subject, we are probably justified in defining it as a milk-and-watery kind of physics specially prepared to suit the weak digestion and small appetite of the embryonic chemist; as a spineless kind of physics—physics without the solid backbone of mathematics. It is high time that it were recognised that he who wishes to be a chemist, and not the mechanical automaton who too frequently in this country goes by the name, must study physics seriously and properly: a mere smattering is of very little use. It is now incontestable that the sciences of chemistry and physics are inseparable: indeed the whole system of modern chemical theory reposes on a purely physical basis—that of Avogadro's law; and of late years the opinion has undoubtedly gained ground that the study of the physical attributes of pure elements and compounds must be carried on systematically and at the same time that their strictly chemical attributes are investigated, if we are ever to penetrate the veil of mystery which at present enshrouds the simplest chemical phenomena. Much work has already been done, and it is greatly to the credit of chemists that it has nearly all been executed by chemists who have been at the pains to study physical methods. Physicists have contributed comparatively little to the advancement of chemical physics, and it is undeniable that not a few investigations of importance to chemistry executed by physicists with the precision in measurement which characterises modern physical inquiry, are to a large extent labour spent in vain, as no guarantee has been given of the purity of the materials employed. Parenthetically it may be added that the physics as well as the chemistry of *pure* substances are fields of inquiry all but untrodden: the study of *pure* materials is *the* work of the future; and it is safe to say that when the results are recorded they will differ in many and important particulars from those now accepted. It is perhaps not undesirable to point out that a true conception of the meaning of the word *pure* does not always appear to be present in the mind of the chemist or physicist, otherwise the expression "chemically pure" would not so frequently occur. What is meant thereby, we presume, is that the substance is so nearly pure that the amount of impurity present does not perceptibly affect the results of quantitative analysis; but the amount of impurity present in a substance in such a case may exercise a most important influence on its chemical behaviour, and may also materially modify some of its physical properties. If, instead of distinguishing degrees of impurity, we continue to make this somewhat improper use of the word pure, it will be desirable also to speak of substances as being "spectroscopically pure," "electrically pure," &c., to indicate the nature of the test which they will pass.

To conclude, a book worthy to be called a treatise on chemical physics has yet to be written. It should contain a sufficiently detailed account of exact methods of determining the various physical attributes and the trustworthy results thus far recorded, together with a discussion of the bearing of these results on the problems of chemistry. It could hardly be the work of a single individual, but would doubtless require the co-operation of chemist and physicist. No mere compilation will suffice: to be of value it must be subject to the control of a chemist who is master of his science. Such a work would of necessity be of enormous service to students; and it would exercise an important influence on the progress of our science. HENRY E. ARMSTRONG

OUR BOOK SHELF

Madagaskar, und die Inseln Seychellen, Aldabra, Komoren, und Maskarenen. By Prof. Dr. R. Hartmann. (Leipzig and Prague, 1886).

THIS little work forms the fifty-seventh volume of "Das Wissen der Gegenwart," a scientific series which has already done so much for the spread of useful and accurate information amongst the Germanic populations. The author, himself personally acquainted with some of the localities here described, gives as clear and comprehensive an account of the various insular groups in the Indian Ocean as was possible within the available space of 150 pages. Of this space over two-thirds are devoted to Madagascar, whose physical constitution, natural history, ethnology, and political relations are treated with great ability. The best authorities, such as Grandidier, Shaw, Richardson, Sibree, and Hildebrandt, have been carefully consulted, and room has even been found for the discussion of such controversial questions as the existence of Sclater's vanished Lemuria, the origin of the Malagasy people, the affinities of their language, the presence in the island of the Vazimba and other aboriginal non-Malayan and Negrito tribes. Dr. Hartmann is inclined to accept the statements made by Commerson and Modave regarding the woolly-haired and dwarfish Kimo people of the southern districts, and suggests possible affinities either with the South African Bushmen or the Andamanese and Aeta Negritos of the Philippine Islands. The Malagasy he regards as essentially a mixed race, Polynesian, Malay, and African (especially Galla and Somali) elements being found diversely intermingled amongst the Hovas, Sakalavas, Betsimisaracas, and other native communities. The Comoros, Seychelles, and Mascarenhas (Mauritius, Réunion, and Rodriguez)

archipelagoes are treated with equal thoroughness, and the work is provided with a map of the Indian Ocean, an index, and numerous well-executed woodcuts.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Physiological Selection and the Origin of Species

IN the *Journal* of the Linnean Society (Zoology, No. 115, 1886, p. 350, footnote) Mr. Romanes says: "I cannot find that any previous writer has alluded to the principle which it is the object of the present paper to enunciate, and which is explained in the succeeding paragraphs."

But in the fourth edition of the "Origin of Species" (1866), p. 311,[1] the following passage occurs, in which the main idea of "physiological selection" is clearly alluded to.

"It may be admitted, on the principle above explained, that it would profit an incipient species if it were rendered in some slight degree sterile when crossed with its parent-form or with some other variety; for thus fewer bastardised and deteriorated offspring would be produced to commingle their blood with the newly-forming variety."

The author then goes on to show that, as he believed, this kind of sterility could not be increased by natural selection—a discussion with which I am not now concerned. I have other evidence to show that my father was familiar with the principle of physiological selection, and, moreover, that he did not regard it with any great favour.

In Mr. Belt's "Naturalist in Nicaragua" (1874), a suggestion is made, identical with that of Mr. Romanes in the Linnean *Journal*. Mr. Belt says (p. 207):—"The varieties that arise can seldom be separated from the parent form and from other varieties until they vary also in the elements of reproduction. . . . As long as varieties interbreed together and with the parent form, it does not seem possible that a new species could be formed by natural selection, excepting in cases of geographical isolation. All the individuals might vary in some one direction, but they could not split up into distinct species whilst they occupied the same area and interbred without difficulty. Before a variety can become permanent, it must either be separated from the others or have acquired some disinclination or inability to interbreed with them. As long as they interbreed together, the possible divergence is kept within narrow limits, but whenever a variety is produced the individuals of which have a partiality for interbreeding, and some amount of sterility when crossed with another form, the tie that bound it to the central stock is loosened, and the foundation is laid for the formation of a new species. Further divergence would be unchecked, or only slightly checked, and the elements of reproduction having begun to vary, would probably continue to diverge from the parent form, for Darwin has shown that any organ in which a species has begun to vary is liable to further change in the same direction. Thus one of the best tests of the specific difference of two allied forms living together is their sterility when crossed, and nearly allied species separated by geographical barriers are more likely to interbreed than those inhabiting the same area."

In my copy of Belt's book the words "No, No," are pencilled in my father's handwriting on the margin, opposite the sentence "All the individuals might vary in some one direction, but they could not split up into distinct species whilst they occupied the same area and interbred without difficulty."

Cambridge, August 27 FRANCIS DARWIN

NEITHER Mr. Galton nor Mr. Meldola have had time or opportunity to consult my original paper before writing their comments on the NATURE abstract. I will, therefore, consider those of their remarks which have been anticipated in the paper.

Mr. Galton writes:—"It has long seemed to me that the primary characteristic of a variety resides in the fact that the individuals who compose it do not, as a rule, *care to mate* with those who are outside their pale, but form through their own sexual inclinations a caste by themselves." Now, I have fully recognised this principle as one among several others which is accessory to, although independent of, physiological selection: see L.S. paper, p. 377, where also reference is given to the "Origin of Species," showing that this factor was likewise recognised by Mr. Darwin as one of importance in the prevention of intercrossing. But, inasmuch as this factor—which may be called psychological selection—can only apply to the case of the Vertebrata,[1] I am disposed to think that it is of much less general importance than the other factors which I have mentioned as accessory to physiological selection, and which, taken altogether, furnish a complete theoretical explanation of the fact that sterility between natural species is not invariably absolute, but occurs in all degrees. For, "in all these cases where the principles of physiological selection have been in any degree accidentally assisted by other conditions, a correspondingly less degree of variation in the reproductive system would have been needed to differentiate the species" (p. 377).

Thus far, therefore, Mr. Galton is really in full agreement with me. But he goes on to say:—"If a variety should arise in the way supposed by Mr. Romanes, merely because its members were more or less infertile with others sprung from the same stock, we should find numerous cases in which members of the variety consorted with outsiders." But how can we possibly know that such is not the case? If my theory is true, it must follow, as Mr. Galton says, that such unions would be more or less sterile, and, as this sterility is itself the only variation which my theory supposes to have arisen *in the first instance*, *ex hypothesi* we can have no means of observing whether or not the individuals which present this variation "consort with outsiders," or with those individuals which do not present it. Lastly, in as far as it is true that "we hardly ever observe pairings between animals of different varieties when living at large in the same or contiguous districts," the fact in no way makes against my theory of physiological selection: it only serves to supplement this theory, in the case of higher animals, by what I regard with Mr. Galton as the proved facts of psychological selection.

The letter by Mr. Meldola is a masterpiece of Darwinian thinking, and on this account I am glad to find myself much more in agreement with him than he appears to suppose. For when he reads my full paper he will see that I have taken precisely the same view upon natural selection as a possible cause —or, rather, accessory promoter—of specific sterility as that to the statement of which the larger part of his letter is devoted. I may remark, however, that of all parts of my paper I regard this as the most speculative and least secure. And this, first, because Mr. Darwin himself, after profound meditation upon the subject, came to the conclusion that natural selection could not operate so as to induce sterility; and, next, because the supposition that it does so operate involves one of the most difficult and complex questions in the whole philosophy of evolution—namely, whether it is possible for natural selection to modify an entire *type* without reference to benefit of its constituent *individuals*. Now, although for reasons which need not here be detailed, I have been led, like Mr. Meldola, to take a different view from that of Mr. Darwin, and to conclude that natural selection may benefit the type without reference to the individual, still I regard this conclusion as so highly speculative that I am glad to think the much more certain theory of physiological selection is not vitally affected either by its acceptance or its rejection. If it is true that natural selection may be able to modify an organic type (as my critic and myself agree in arguing, the type in this case being a variety) by conferring on it the benefit of sterility with its parent form, notwithstanding that this cannot be effected through benefit conferred on any of the constituent individuals, then all we have to say in the present connection is that natnral selection is probably one of the many other causes which lead to physiological selection.

[1] A corresponding but not identical passage occurs in the sixth edition, p. 247.

[1] This, at least, is what I state in the paper. Mr. Galton, however, suggests that the principle may be extended even to plants, through "the selective appetites of the insects which carry the pollen." This suggestion is unquestionably original, and bears the stamp of its author's ingenious mind. Moreover, considerable probability is, I think, lent to the suggestion by the observations of Mr. Bennett and others on individual insects selecting similarly coloured flowers on which to feed (see *Journ. L.S.*, 1883).

On the other hand, if natural selection cannot thus operate, all we have to say is that there still remain many other causes adequate to explain the occurrence of physiological selection—to wit, those causes which are concerned in the occurrence of variation in general.

The essay by Prof. Weismann on the influence of isolation, to which Mr. Meldola refers, is so replete with facts and arguments unconsciously bearing on my theory, that in writing my preliminary paper it appeared advisable to reserve so rich a mine for subsequent working out in detail. In my paper, therefore, I have merely alluded to Prof. Weismann as one among the comparatively few evolutionists who have hitherto sufficiently considered the influence of independent variation (or the prevention of intercrossing) in the evolution of species.

It only remains to consider Mr. Meldola's extremely able criticism of my view that natural selection ought not in strictness to be regarded as a theory of the origin of species, but rather as a theory of the development of adaptive modifications. My argument is that natural selection can only be a theory of the origin of species in so far as species differ from one another in points of utilitarian significance; and that even then it is only a theory of the origin of species, as it were, incidentally: the *raison d'être* of natural selection is in all cases that of evolving adaptations (whether these be characteristic of species only, or likewise of higher taxonomic divisions); and if in some cases the result of performing this function is that of raising a variety into a species, such a result is merely collateral, or, in a sense, accidental. No doubt if species always and only differed from one another in points of utilitarian character, the collateral nature of the result might be disregarded, and the theory would become a theory of the origin of species in virtue of its being a theory of the development of adaptations. But, as a matter of fact, species are very far from being always and only distinguished from one another in points of utilitarian character, and in so far as they are not thus distinguished natural selection is obviously in no sense a theory of the origin of species. Again, and more particularly, the one feature which more than any other serves to distinguish species from species is that of mutual sterility, and it would be a bold flight of speculation to affirm that this has been in all cases the result of natural selection, when even Mr. Darwin was reluctantly compelled to conclude that such could not be the result of natural selection in any case. On the other hand, my theory of physiological selection explains this very general feature of specific distinction quite independently of natural selection; and then goes on to show that, when once this primary distinction has arisen, many others of a secondary kind will ensue, both with and without the assistance of natural selection.

Now, the objection which Mr. Meldola adduces against this argument is that I have not proved physiological selection to be independent of natural selection. In other words, he does not dispute the probable truth of my theory; but he says that, granting its truth, it is still only "one particular phase of natural selection." But surely the burden of proof here lies on the side of my critic. If he can show any sufficient reason for going much further than I have ventured to go in out-Darwining Darwin—or for holding that natural selection may not merely help in inducing sterility in some cases, but has been the sole cause of it in all cases—then I should welcome his proof as showing that the principles of physiological selection ultimately and in all cases rest on those of natural selection. But, clearly, it is for him to prove his positive: not for me to prove what I regard as an almost preposterous negative.

So much for the main criticism. But he adds this rider, namely, that, as the struggle for existence is always most severe between the most closely related forms, unless the new or sexually protected form arising under physiological selection possesses some distinct advantage over the old or parent form, it will be exterminated by the latter quite as effectually as it would be by intercrossing in the absence of physiological selection. To this I may answer in the words of my full paper:—"So long as there is no actual *detriment* arising to the variety on account of its being sexually separated from the parent, any ideas derived from the theory of natural selection are plainly without bearing upon the subject" (p. 406). In other words, so long as in all other respects of organisation the sexually separated variation is not less "fit" than its parent stock, so long there is no reason to anticipate any disadvantage in the struggle for existence. And forasmuch as the sexual separation arises only by way of a variation locally affecting the reproductive system, when the variation is first sexually separated, it will in all other respects resemble its parent stock, and so be able to compete with it on equal terms—mere numerical inferiority being without significance where intercrossing is prevented. It was in order to convey this meaning that I proposed as an alternative name of my theory, "Segregation of the Fit"; seeing that before any physiological segregation can take place there must be organisms to be segregated, and that unless these organisms had already proved themselves fitted to survive in the struggle for existence, in existence they could not be. But I do not call physiological selection "Segregation of the *Fittest*," because, unlike natural selection, it is in no way concerned with the principle of conflict. So long as the organisms which have been separated by physiological selection are sufficiently fit to have previously passed muster at the hands of natural selection, there is no reason why the daughter type should be fitter than the parent.

But, so far as I can see, the only material point of difference between Mr. Meldola and myself consists in his regarding physiological selection as "subordinate" to natural selection, while I consider the two as quite independent principles, although, as explained in my paper, I believe that they frequently and in several ways play into each other's hands.

GEORGE J. ROMANES

Geanies, Ross-shire, N.B., August 30

Earth-Currents and Aurora

THERE appears to have been a very remarkable and widespread earth-current storm on March 30 last, full particulars of which it would be extremely useful to have on record. My attention has been drawn to this storm through witnessing, on the evening of that day, one of the most vivid and interesting displays of the aurora that I have ever seen. Mr. G. H. Kinahan, writing in NATURE for April 8 (p. 537), describes the same aurora as observed by him in Donegal between 8 and 9 p.m., and notes its peculiar bright silvery type. It must, however, have been a far less imposing display at Donegal, where the weather was less favourable, than at Kingstown, where I saw it between 9 and 10 p.m., the most brilliant display occurring between 9.30 and 9.45 p.m. From the northerly horizon there rolled up to the zenith in quick succession streams and masses of white light, until the whole face of the sky to the north and west was illuminated with swiftly mounting flames of silvery whiteness and wonderful beauty. A correspondent in NATURE for April 15 (p. 559) describes the same aurora witnessed by him between 8 and 11 p.m. on March 30, at Konigsberg, in Prussia.

According to the *Electrician* (April 2, p. 404), on the morning of March 30 a violent earth-current storm occurred in London, stopping all telegraphic work for some time. During the same day strong earth-currents are reported on the Mediterranean cables, and in the afternoon of that day (March 30) this terrestrial electric storm had reached India, lasting from 2 to 5 p.m., and stopping work on the Bombay and Madras line. At the same time powerful earth-currents are reported on the Madras and Penang cable, causing work over it to be stopped from 3 till 7 p.m. on March 30. Similar disturbances are reported on the Java cable, beginning the same afternoon, and becoming fainter on all lines at 10 p.m. Recently an account has been published of a great earth-current storm on the China and Japan cable on March 30, and a diagram of the perturbations produced on that line is given in the *Electrician* for August 6. The storm began between 4 and 5 p.m., Shanghai time, on March 30, and lasted till 11 p.m. that day, the maximum strength of the earth-current on the cable occurring between 6 and 7 p.m. being then equal to 3·6 milliamperes. Smaller renewals of the storm took place both in Europe and Asia on the morning of March 31.

Perhaps some of the readers of NATURE can give further details of this extensive storm, and it would also be useful to have on record the magnetic perturbations noticed on this occasion in different localities, and the time of their occurrence.

W. F. BARRETT

Royal College of Science, Dublin

Chlamydomyxa in the Engadine

YOUR readers will be interested to hear that I have found here in Pontresina the very interesting Protozoon described twelve years ago, in the *Quarterly Journal of Microscopical Science*, by

Mr. Archer, of Dublin, under the name Chlamydomyxa. Mr. Archer obtained his specimens crawling upon, and into, the tissues of the bog-moss (Sphagnum) in moor-pools in Westmeath (so far as I recollect). He often found it in a spherical condition, encysted in a cellulose envelope, and more rarely expanding into a most curious network of fine protoplasmic threads, upon which were observed very numerous oblong corpuscles, which slowly travelled along the threads, thus recalling the structure of Labyrinthula described by Cienkowski.

No one has found Archer's Chlamydomyxa, or been in a position to confirm independently his description, up to the present date. He has kindly sent to me *encysted* specimens of the Chlamydomyxa, but these would not leave their cysts and exhibit the characteristic network and corpuscles; and others whom he has wished to oblige by a sight of this interesting organism have also, I believe, failed to obtain the characteristic phase.

I have from time to time searched for Chlamydomyxa when I have been in a moorland region and had my microscope with me, but have hitherto failed to find it. It was not, therefore, with any great amount of confidence that I gathered some brownish tufts of Sphagnum from a small ditch (with slowly-running water) in a clearing in the pine-wood behind the tennis-court at Pontresina, and brought them to the hotel to search them for Chlamydomyxa. But I found a number of yellowish spheres about 1/150 inch, and less in diameter, which excited my suspicions. After a brief delay these began to throw out protoplasmic filaments, and soon around each was a wonderful series of branching stems of protoplasmic threads, reaching far away from the central yellow granular body and in the most varied directions. Along the threads minute oval corpuscles slowly streamed. There is no doubt that this organism belongs to Archer's genus Chlamydomyxa, and probably enough is specifically distinct from that which he found in Ireland. It is extremely abundant in the Swiss locality.

From what I have seen of Chlamydomyxa, I am now inclined to admit that it is *less* closely related to Cienkowski's Labyrinthula than I had previously supposed. The moving corpuscles of Cienkowski's organism are very much larger bodies than are the ovoid corpuscles of Chlamydomyxa.

Pontresina, August 23 E. RAY LANKESTER

THE BRITISH ASSOCIATION

THE fifty-sixth annual meeting of the British Association was opened in Birmingham last night, when Lord Rayleigh resigned the Presidential chair to Sir William Dawson, Principal of McGill College, Montreal. The attendance at the fourth Birmingham meeting promises to be much above the average, and so far as the Birmingham people are concerned everything has been done to secure success. All the new public buildings, including Mason College and the fine new Art Gallery, have been placed at the disposal of the Sections. The reception-rooms, reading-rooms, writing-rooms, and other general rooms, not omitting the smoking-room, are all that could be desired. All the leading clubs of the town have been thrown open to members; facilities have been given for visiting the various manufactories in the town and neighbourhood; the industrial exhibition in Bingley Hall has been admirably arranged; while there is a formidable programme of social entertainments and excursions. Among the foreign visitors expected, one of the most distinguished is Prof. Haeckel. There is a very large representation, moreover, of colonial science.

The Local Committee have prepared a very excellent Hand-Book of Birmingham for the use of the visitors, the various sections of which are written by specialists. After a General Introduction by Mr. G. J. Johnson, Part 1 deals with Old Birmingham, by Mr. S. Timmins. The various aspects of Modern Birmingham are treated of by different writers. The section on Art is by four specialists, and the Manufacturing Industries are described by Mr. C. J. Woodward. Part 3 is devoted to Geology and Physiography, and is edited by Prof. C. Lapworth, who has been assisted in the various sections by several local geologists. Part 4, dealing with Zoology, is edited by Mr. W. R. Hughes, F.L.S., who also has had the assistance of various local specialists; so with the Botany, edited by Mr. W. Mathews. The appendix comprises a variety of curious and useful information, while a pocket contains a useful sketch-map of the Geology of the Birmingham district, by Prof. Lapworth. It will thus be seen that the Hand-Book, while well adapted for its special purpose, is likely to be of permanent value.

INAUGURAL ADDRESS BY SIR J. WILLIAM DAWSON, C.M.G., M.A., LL.D., F.R.S., F.G.S., PRINCIPAL AND VICE-CHANCELLOR OF MCGILL UNIVERSITY, MONTREAL, CANADA, PRESIDENT

TWENTY-ONE years have passed away since the last meeting of the British Association in this great central city of England. At the third Birmingham meeting—that of 1865—I had the pleasure of being present, and had the honour of being one of the Vice-Presidents of Section C. At that meeting my friend John Phillips, one of the founders of the Association, occupied the Presidential chair, and I cannot better introduce what I have to say this evening than by the eloquent words in which he then addressed you:—"Assembled for the third time in this busy centre of industrious England, amid the roar of engines and clang of hammers, where the strongest powers of nature are trained to work in the fairy chains of art, how softly and fittingly falls upon the ear the accent of Science, the friend of that art, and the guide of that industry! Here where Priestley analysed the air, and Watt obtained the mastery over steam, it well becomes the students of nature to gather round the standard which they carried so far into the fields of knowledge. And when on other occasions we meet in quiet colleges and academic halls, how gladly welcome is the union of fresh discoveries and new inventions with the solid and venerable truths which are there treasured and taught. Long may such union last; the fair alliance of cultivated thought and practical skill; for by it labour is dignified and science fertilised, and the condition of human society exalted." These were the words of a man who, while earnest in the pursuit of science, was full of broad and kindly sympathy for his fellow-men, and of hopeful confidence in the future. We have but to turn to the twenty Reports of this Association, issued since 1865, to see the realisation of that union of science and art to which he so confidently looked forward, and to appreciate the stupendous results which it has achieved. In one department alone—that to which my predecessor in this chair so eloquently adverted in Aberdeen, the department of education in science—how much has been accomplished since 1865. Phillips himself lived to see a great revolution in this respect at Oxford. But no one in 1865 could have anticipated that immense development of local schools of science of which your own Mason College and your admirable technical, industrial, and art schools are eminent examples. Based on the general education given by the new system of Board-schools, with which the name of the late W. E. Forster will ever be honourably connected, and extending its influence upward to special training and to the highest University examinations, this new scientific culture is opening paths of honourable ambition to the men and women of England scarcely dreamed of in 1865. I sympathise with the earnest appeal of Sir Lyon Playfair, in his Aberdeen address, in favour of scientific education; but, visiting England at rare intervals, I am naturally more impressed with the progress that has been made than with the vexatious delays which have occurred, and am perhaps better able to appreciate the vast strides that have been taken in the direction of that complete and all-pervading culture in science which he has so ably advocated.

No one could have anticipated twenty years ago that a Birmingham manufacturer, in whose youthful days there were no schools of science for the people, was about to endow a College, not only worthy of this great city, but one of its brightest ornaments.[1] Nor could any one have foreseen the great development of local scientific Societies, like your Midland Institute and Philosophical Society, which are now flourishing in every large town and in many of those of less magnitude. The period of twenty-one years that has elapsed since the last Birmingham meeting has also been an era of public museums and laboratories for the teaching of science, from the magnificent national

[1] It was in 1865 that Sir Josiah Mason was, quietly and without any public note, beginning to lay the foundation of his orphanage at Erdington

institutions at South Kensington and those of the great Universities and their Colleges down to those of the schools and field-clubs in country towns. It has besides been an era of gigantic progress in original work and in publication—a progress so rapid that workers in every branch of study have been reluctantly obliged to narrow in more and more their range of reading and of effort to keep abreast of the advance in their several departments. Lastly, these twenty-one years have been characterised as the "coming of age" of that great system of philosophy with which the names of three Englishmen—Darwin, Spencer, and Wallace—are associated as its founders. Whatever opinions one may entertain as to the sufficiency and finality of this philosophy, there can be no question as to its influence on scientific thought. On the one hand, it is inaccurate to compare it with so entirely different things as the discovery of the chemical elements and of the law of gravitation; on the other, it is scarcely fair to characterise it as a mere "confused development" of the mind of the age. It is indeed a new attempt of science in its maturer years to grapple with those mysterious questions of origins which occupied it in the days of its infancy, and it is to be hoped that it may not, like the Titans of ancient fable, be hurled back from heaven, or, like the first mother, find the knowledge to which it aspires a bitter thing. In any case we should fully understand the responsibility which we incur when in these times of full-grown science we venture to deal with the great problem of origins, and should be prepared to find that in this field the new philosophy, like those which have preceded it, may meet with very imperfect success. The agitation of these subjects has already brought science into close relations, sometimes friendly, sometimes hostile, it is to be hoped in the end helpful, with those great and awful questions of the ultimate destiny of humanity, and its relations to its Creator, which must always be nearer to the human heart than any of the achievements of science on its own ground. In entering on such questions we should proceed with caution and reverence, feeling that we are on holy ground, and that though, like Moses of old, we may be armed with all the learning of our time, we are in the presence of that which while it burns is not consumed; of a mystery which neither observation, experiment, nor induction can ever fully solve.

In a recent address, the late President of the Royal Society called attention to the fact that, within the life-time of the older men of science of the present day, the greater part of the vast body of knowledge included in the modern sciences of physics, chemistry, biology, and geology has been accumulated, and the most important advances made in its application to such common and familiar things as the railway, ocean navigation, the electric telegraph, electric lighting, the telephone, the germ-theory of disease, the use of anæsthetics, the processes of metallurgy, and the dyeing of fabrics. Even since the last meeting in this city much of this great work has been done, and has led to general results of the most marvellous kind. What at that time could have appeared more chimerical than the opening up, by the enterprise of one British colony, of a shorter road to the East by way of the extreme West, realising what was happily called by Milton and Cheadle "the new North-West Passage," making Japan the next neighbour of Canada on the west, and offering to Britain a new way to her Eastern possessions; or than the possibility of this Association holding a successful meeting on the other side of the Atlantic? We have now an invitation to meet in Australia, and may, if we please, proceed thither by the Canadian Pacific Railway and its new lines of steamers, returning by the Suez Canal.[1] To-day this is quite as feasible as the Canadian visit would have been in 1865. It is science that has thus brought the once widely separated parts of the world nearer to each other, and is breaking down those geographical barriers which have separated the different portions of our widely extended British race. Its work in this is not yet complete. Its goal to-day is its starting-point to-morrow. It is as far as at any previous time from seeing the limit of its conquests, and every victory gained is but the opening of the way for a further advance.

By its visit to Canada the British Association has asserted its Imperial character, and has consolidated the scientific interests of Her Majesty's dominions, in advance of that great gathering of the industrial products of all parts of the Empire now on exhibition in London, and in advance of any political plans of Imperial federation.[1] There has even been a project before us for an International Scientific Convention, in which the great English Republic of America shall take part—a project the realisation of which was to some extent anticipated in the fusion of the members of the British and American Associations at Montreal and Philadelphia in 1884. As a Canadian, as a past President of the American Association, and now honoured with the Presidency of this Association, I may be held to represent in my own person this scientific union of the British Islands, of the various Colonies, and of the great Republic, which, whatever the difficulties attending its formal accomplishment at present, is certain to lead to an actual and real union for scientific work. In furtherance of this I am glad to see here to-day influential representatives of most of the British Colonies, of India, and of the United States. We welcome here also delegates from other countries, and though the barrier of language may at present prevent a larger union, we may entertain the hope that Britain, America, India, and the Colonies, working together in the interest of science, may ultimately render our English tongue the most general vehicle of scientific thought and discovery—a consummation of which I think there are, at present, many indications.

But, while science marches on from victory to victory, its path is marked by the resting-places of those who have fought its battles and assured its advance. In looking back to 1865 there rise before me the once familiar countenances of Phillips, Murchison, Lyell, Forbes, Jeffreys, Jukes, Rolleston, Miller, Spottiswoode, Fairbairn, Gassiot, Carpenter, and a host of others, present in full vigour at that meeting, but no more with us. These were veterans of science; but, alas! many then young and rising in fame are also numbered with the dead. It may be that before another Birmingham meeting many of us, the older members now, will also have passed away. But these men have left behind them ineffaceable monuments of their work, in which they still survive, and we rejoice to believe that, though dead to us, they live in that company of the great and good of all ages who have entered into that unseen Universe where all that is high and holy and beautiful must go on accumulating till the time of the restitution of all things. Let us follow their example and carry on their work, as God may give us power and opportunity, gathering in precious stores of knowledge and of thought, in the belief that all truth is immortal, and must go on for ever bestowing blessings on mankind. Thus will the memory of the mighty dead remain to us as a power which,

"Like a star,
Beacons from the abode where the eternal are."

I do not wish, however, to occupy your time longer with general or personal matters, but rather to take the opportunity afforded by this address to invite your attention to some topics of scientific interest. In attempting to do this, I must have before me the warning conveyed by Prof. Huxley, in the address to which I have already referred, that in our time science, like Tarpeia, may be crushed with the weight of the rewards bestowed on her. In other words, it is impossible for any man to keep pace with the progress of more than one limited branch of science, and it is equally impossible to find an audience of scientific men of whom anything more than a mere fraction can be expected to take an interest in any one subject. There is, however, some consolation in the knowledge that a speaker who is sufficiently simple for those who are advanced specialists in other departments, will of necessity be also sufficiently simple to be understood by the general public who are specialists in nothing. On this principle a geologist of the old school, accustomed to a great variety of work, may hope so to scatter his fire as to reach the greater part of the audience. In endeavouring to secure this end, I have sought inspiration from that ocean which connects rather than separates Britain and America, and may almost be said to be an English sea—the North Atlantic. The geological history of this depression of the earth's crust, and its relation to the continental masses which limit it, may furnish a theme at once generally intelligible and connected with great questions as to the structure and history of the earth, which have excited the attention alike of physicists, geologists, biologists, geographers, and ethnologists. Should I, in treating of these questions, appear to be somewhat abrupt and dogmatic, and to indicate rather than state the

[1] It is expected that, on the completion of the whole of the connections of the Canadian Pacific Railway, the time from ocean to ocean may be reduced to 116 hours, and from London to Hong Kong to 27 days.

[1] I should note here, in connection with this, the valuable volume of "Canadian Economics," edited by Mr. D. A. P. Watt, which was one of the results of the Montreal meeting.

evidence of the general views announced, I trust you will kindly attribute this to the exigencies of a short address.

If we imagine an observer contemplating the earth from a convenient distance in space, and scrutinising its features as it rolls before him, we may suppose him to be struck with the fact that eleven-sixteenths of its surface are covered with water, and that the land is so unequally distributed that from one point of view he would see a hemisphere almost exclusively oceanic, while nearly the whole of the dry land is gathered in the opposite hemisphere. He might observe that the great oceanic area of the Pacific and Antarctic Oceans is dotted with islands—like a shallow pool with stones rising above its surface—as if its general depth were small in comparison with its area. He might also notice that a mass or belt of land surrounds each Pole, and that the northern ring sends off to the southward three vast tongues of land and of mountain-chains, terminating respectively in South America, South Africa, and Australia, towards which feebler and insular processes are given off by the Antarctic continental mass. This, as some geographers have observed,[1] gives a rudely three-ribbed aspect to the earth, though two of the three ribs are crowded together and form the Europ-asian mass, or double continent, while the third is isolated in the single continent of America. He might also observe that the northern girdle is cut across, so that the Atlantic opens by a wide space into the Arctic Sea, while the Pacific is contracted towards the north, but confluent with the Antarctic Ocean. The Atlantic is also relatively deeper and less cumbered with islands than the Pacific, which has the higher ridges near its shores, constituting what some visitors to the Pacific coast of America have not inaptly called the "back of the world," while the wider slopes face the narrower ocean, into which for this reason the greater part of the drainage of the land is poured.[2] The Pacific and Atlantic, though both depressions or flattenings of the earth, are, as we shall find, different in age, character, and conditions; and the Atlantic, though the smaller, is the older, and, from the geological point of view, in some respects the more important of the two.

If our imaginary observer had the means of knowing anything of the rock-formations of the continents, he would notice that those bounding the North Atlantic are in general of great age, some belonging to the Laurentian system. On the other hand, he would see that many of the mountain-ranges along the Pacific are comparatively new, and that modern igneous action occurs in connection with them. Thus he might be led to believe that the Atlantic, though comparatively narrow, is an older feature of the earth's surface, while the Pacific belongs to more modern times. But he would note in connection with this that the oldest rocks of the great continental masses are mostly toward their northern ends, and that the borders of the northern ring of land and certain ridges extending southwards from it constitute the most ancient and permanent elevations of the earth's crust, though now greatly surpassed by mountains of more recent age nearer the equator. Before leaving this general survey we may make one further remark. An observer looking at the earth from without would notice that the margins of the Atlantic and the main lines of direction of its mountain-chains are north-east and south-west, and north-west and south-east, as if some early causes had determined the occurrence of elevations along great circles of the earth's surface tangent to the polar circles.

We are invited by the preceding general glance at the surface of the earth to ask certain questions respecting the Atlantic:—(1) What has at first determined its position and form? (2) What changes has it experienced in the lapse of geological time? (3) What relations have these changes borne to the development of life on the land and in the water? (4) What is its probable future?

Before attempting to answer these questions, which I shall not take up formally in succession, but rather in connection with each other, it is necessary to state as briefly as possible certain general conclusions respecting the interior of the earth. It is popularly supposed that we know nothing of this beyond a superficial crust perhaps averaging 50,000 to 100,000 feet in thickness. It is true we have no means of exploration in the earth's interior, but the conjoined labours of physicists and geologists have now proceeded sufficiently far to throw much inferential light on the subject, and to enable us to make some general affirmations with certainty; and these it is the more necessary to state distinctly, since they are often treated as mere subjects of speculation and fruitless discussion.

(1) Since the dawn of geological science it has been evident that the crust on which we live must be supported on a plastic or partially liquid mass of heated rock, approximately uniform in quality under the whole of its area. This is a legitimate conclusion from the wide distribution of volcanic phenomena, and from the fact that the ejections of volcanoes, while locally of various kinds, are similar in every part of the world. It led to the old idea of a fluid interior of the earth, but this is now generally abandoned, and this interior heated and plastic layer is regarded as merely an under-crust.

(2) We have reason to believe, as the result of astronomical investigations,[1] that, notwithstanding the plasticity or liquidity of the under-crust, the mass of the earth—its nucleus as we may call it—is practically solid and of great density and hardness. Thus we have the apparent paradox of a solid yet fluid earth, solid in its astronomical relations, liquid or plastic for the purposes of volcanic action and superficial movements.[2]

(3) The plastic sub-crust is not in a state of dry igneous fusion, but in that condition of aqueo-igneous or hydro-thermic fusion which arises from the action of heat on moist substances, and which may either be regarded as a fusion or as a species of solution at a very high temperature. This we learn from the phenomena of volcanic action, and from the composition of the volcanic and plutonic rocks, as well as from such chemical experiments as those of Daubrée and of Tilden and Shenstone.[3]

(4) The interior sub-crust is not perfectly homogeneous, but may be roughly divided into two layers or magmas, as they have been called: an upper, highly siliceous or acidic, of low specific gravity and light-coloured, and corresponding to such kinds of plutonic and volcanic rocks as granite and trachyte; and a lower, less siliceous or more basic, more dense, and more highly charged with iron, and corresponding to such igneous rocks as the dolerites, basalts, and kindred lavas. It is interesting here to note that this conclusion, elaborated by Durocher and Von Waltershausen, and usually connected with their names, appears to have been first announced by John Phillips, in his "Geological Manual," and as a mere common-sense deduction from the observed phenomena of volcanic action and the probable results of the gradual cooling of the earth.[4] It receives striking confirmation from the observed succession of acidic and basic volcanic rocks of all geological periods and in all localities. It would even seem, from recent spectroscopic investigations of Lockyer, that there is evidence of a similar succession of magmas in the heavenly bodies, and the discovery by Nordenskjöld of native iron in Greenland basalts affords a probability that the inner magma is in part metallic.[5]

(5) Where rents or fissures form in the upper crust, the material of the lower crust is forced upward by the pressure of the less supported portions of the former, giving rise to volcanic phenomena either of an explosive or quiet character, as may be determined by contact with water. The underlying material

[1] Dana, "Manual of Geology," introductory part. Green, "Vestiges of a Molten Globe," has summed up these facts.

[2] Mr. Mellard Reade, in two Presidential addresses before the Geological Society of Liverpool, has well illustrated this point and its geological consequence.

[1] Hopkins, Mallet, Sir William Thomson, and Prof G. H. Darwin maintain the solidity and rigidity of the earth on astronomical grounds; but different conclusions have been reached by Hennesey, Delaunay, and Airy. In America, Barnard and Crosby, Dutton, Le Conte, and Wadsworth have discussed these questions.

[2] An objection has been taken to the effect that the supposed ellipsoidal form of the equator is inconsistent with a plastic sub-crust. But this ellipsoidal form is not absolutely certain, or, if it exists, is very minute. Bonney has in a recent lecture suggested the important consideration that a mass may be slowly mobile under long-continued pressure, while yet rigid with reference to more sudden movements.

[3] *Phil. Trans.*, 1884. Also Crosby in *Proc. Boston Soc. Nat. Hist.*, 1883.

[4] Phillips says ("Manual of Geology," 1855, p. 493):—"If we regard them (the internal crystalline rocks) as acquiring solidification by cooling in zones parallel to the surface, we should have sheets of granitic and basaltic rocks generated below, the first uppermost, the last undermost, while above the several strata were produced in a series beginning at the bottom. In this sense the rocks of fusion may be called, with Lyell, *hypogene*. Certainly under particular areas of country are found evidence of the liquefaction of one set of igneous products after the solidification of others. Many dykes of basalt traversing granite show themselves to have been in fusion after the solidification of the granite." In various forms Phillips returns to this idea, as at pp. 556 and 564, in that unpretending manner which was his wont. Dr. Sterry Hunt has kindly directed my attention to the fact of Phillips's right of priority in this matter. Durocher in 1857 elaborated the theory of magmas in the *Annales des Mines*, and we are indebted to Dutton, of the United States Geological Survey, for its detailed application to the remarkable volcanic outflows of Western America.

[5] These basalts occur at Ovifak, Greenland. Andrews has found small particles of iron in British basalts. Prestwich and Judd have referred to the bearing on general geology of these facts, and of Lockyer's suggestions.

may also be carried to the surface by the agency of heated water, producing those quiet discharges which Hunt has named crenitic. It is to be observed here that explosive volcanic phenomena, and the formation of cones, are, as Prestwich has well remarked, characteristic of an old and thickened crust; quiet ejection from fissures and hydro-thermal action may have been more common in earlier periods and with a thinner over-crust.

(6) The contraction of the earth's interior by cooling and by the emission of material from below the over-crust, has caused this crust to press downward, and therefore laterally, and so to effect great bends, folds, and plications; and these modified subsequently by surface denudation constitute mountain-chains and continental plateaus. As Hall long ago pointed out,[1] such lines of folding have been produced more especially where thick sediments had been laid down on the sea-bottom. Thus we have here another apparent paradox, namely, that the elevations of the earth's crust occur in the places where the greatest burden of detritus has been laid down upon it, and where consequently the crust has been softened and depressed. We must beware, in this connection, of exaggerated notions of the extent of contraction and of crumbling required to form mountains. Bonney has well shown, in lectures delivered at the London Institution, that an amount of contraction, almost inappreciable in comparison with the diameter of the earth, would be sufficient; and that as the greatest mountain-chains are less than 1/600 of the earth's radius in height, they would on an artificial globe a foot in diameter be no more important than the slight inequalities that might result from the paper gores overlapping each other at the edges.

(7) The crushing and sliding of the over-crust inplied in these movements raise some serious questions of a physical character. One of these relates to the rapidity or slowness of such movements, and the consequent degree of intensity of the heat developed, as a possible cause of metamorphism of rocks. Another has reference to the possibility of changes in the equilibrium of the earth itself as resulting from local collapse and ridging. These questions in connection with the present dissociation of the axis of rotation from the magnetic poles, and with changes of climate, have attracted some attention,[2] and probably deserve further consideration on the part of physicists. In so far as geological evidence is concerned, it would seem that the general association of crumbling with metamorphism indicates a certain rapidity in the process of mountain-making, and consequent development of heat, and the arrangement of the older rocks around the Arctic basin forbids us from assuming any extensive movement of the axis of rotation, though it does not exclude changes to a limited extent. I hope that Prof. Darwin will discuss these points in his address to the Physical Section.

I wish to formulate these principles as distinctly as possible, and as the result of all the long series of observations, calculations, and discussions since the time of Werner and Hutton, and in which a vast number of able physicists and naturalists have borne a part, because they may be considered as certain deductions from our actual knowledge, and because they lie at the foundation of a rational physical geology.

We may popularise these deductions by comparing the earth to a drupe or stone-fruit, such as a plum or peach, somewhat dried up. It has a large and intensely hard stone and kernel, a thin pulp made up of two layers, an inner more dense and dark-coloured, and an outer less dense and lighter-coloured. These constitute the under-crust. On the outside it has a thin membrane or over-crust. In the process of drying it has slightly shrunk, so as to produce ridges and hollows of the outer crust, and this outer crust has cracked in some places, allowing portions of the pulp to ooze out—in some of these its lower dark substance, in others its upper and lighter material. The analogy extends no further, for there is nothing in our withered fruit to represent the oceans occupying the lower parts of the surface or the deposits which they have laid down.

Keeping in view these general conclusions, let us now turn to their bearing on the origin and history of the North Atlantic.

Though the Atlantic is a deep ocean, its basin does not constitute so much a depression of the crust of the earth as a flattening of it, and this, as recent soundings have shown, with a slight ridge or elevation along its middle, and banks or terraces fringing the edges, so that its form is not so much that of a basin as that of a shallow plate with its middle a little raised. Its true permanent margins are composed of portions of the over-crust folded, ridged up, and crushed, as if by lateral pressure emanating from the sea itself. We cannot, for example, look at a geological map of America without perceiving that the Appalachian ridges, which intervene between the Atlantic and the St. Lawrence Valley, have been driven bodily back by a force acting from the east, and that they have resisted this pressure only where, as in the Gulf of St. Lawrence and the Catskill region of New York, they have been protected by outlying masses of very old rocks, as, for example, by that of the Island of Newfoundland and that of the Adirondack Mountains. The admirable work begun by my friend and fellow-student, Prof. James Nicol, followed up by Hicks, Lapworth, and others, and now, after long controversy, fully confirmed by the recent observations of the Geological Survey of Scotland, has shown the most intense action of the same kind on the east side of the ocean in the Scottish highlands; and the more widely distributed Eozoic rocks of Scandinavia may be appealed to in further evidence of this.[1]

If we now inquire as to the cause of the Atlantic depression, we must go back to a time when the areas occupied by the Atlantic and its bounding coasts were parts of a shoreless sea in which the earliest gneisses or stratified granites of the Laurentian age were being laid down in vastly extended beds. These ancient crystalline rocks have been the subject of much discussion and controversy, and as they constitute the lowest and probably the firmest part of the Atlantic sea-bed, it is necessary to inquire as to their origin and history. Dr. Bonney, the late President of the Geological Society, in his anniversary address, and Dr. Sterry Hunt, in an elaborate paper communicated to the Royal Society of Canada, have ably summed up the hypotheses as to the origin of the oldest Laurentian beds. At the basis of these hypotheses lies the admission that the immensely thick beds of orthoclase gneiss, which are the oldest stratified rocks known to us, are substantially the same in composition with the upper or siliceous magma or layer of the under-crust. They are, in short, its materials either in their primitive condition or merely re-arranged. One theory considers them as original products of cooling, owing their lamination merely to the successive stages of the process. Another view refers them to the waste and re-arrangement of the materials of a previously massive granite. Still another holds that all our granites really arise from the fusion of old gneisses of originally aqueous origin; while a fourth refers the gneisses themselves to molecular changes effected in granite by pressure. These several views, in so far as they relate to the oldest or fundamental Laurentian gneiss, may be arranged under the following heads:—(1) *Endoplutonic*, or that which regards all the old gneisses as molten rocks cooled from without inward in successive layers.[2] (2) *Exoplutonic*, or that which considers them as made up of matter ejected from below the upper crust in the manner of volcanic action.[3] (3) *Metamorphic*, which supposes the old gneisses to arise from the crystallisation of detrital matter spread over the sea-bottom, and either igneous or derived from the decay of igneous rocks.[4] (4) *Chaotic* or *Thermo-chaotic*, or the theory of deposit from the turbid waters of a primæval ocean either with or without the aid of heat.[5] In one form this was the old theory of Werner. (5) *Crenitic* or *Hydro-thermic*, which supposes the action of heated waters penetrating below the crust to be constantly bringing up to the surface mineral matters in solution and depositing these so as to form feldspathic and other rocks.[6]

[1] Hall (American Association Address, 1857, subsequently republished, with additions, as "Contributions to the Geological History of the American Continent," Mallet), Rogers, Dana, Le Conte, &c.

[2] See recent papers of Oldham and Fisher in the *Geological Magazine* and *Philosophical Magazine*, July 1886. Also Péroche, "Revol. Polaires" (Paris, 1886).

[1] Address to the Geological Section, by Prof. Judd, Aberdeen meeting, 1885. According to Rogers, the crumpling of the Appalachians has reduced a breadth of 158 miles to about 60.

[2] Naumann, Phillips, Durocher, McFarlane, &c.

[3] Clarence King, Tornebohm, Marr, &c.

[4] Lyell, Kopp, Reusch, Judd, &c. [5] Scrope, Delabeche, Daubrée.

[6] Hunt, *loc. cit.* The following is Dr. Hunt's summary statement of this theory:—"The globe consolidating at the centre left, it is conceived, a superficial layer of basic silicates, which has yielded all the fixed elements of the earth's crust. This layer formed the first land and the floor of the primæval sea, the acid waters of which, permeating and partially decomposing it, became thereby chemically neutralised. This last-cooled layer, mechanically disintegrated, saturated with water, and heated by the central mass, was the source of mineral springs, holding in solution the silicates which built up the ancient gneisses and similar rocks. Granitic veins and zeolites are due to survivals of the process which generated the gneissic rocks. The hypothesis of their formation from materials brought to the surface by mineral springs from the primitive basic layer affords, it is claimed, the elements of a complete and intelligible explanation of the origin of the Eozoic rocks. This upward lixiviation of the primitive mass, and the deposition over it of an acidic granite-like rock, would leave below a highly basic material, and the division of the mass thus established would correspond to that of the trachytic

It will be observed, in regard to these theories, that none of them supposes that the old gneiss is an ordinary sediment, but that all regard it as formed in exceptional circumstances, these circumstances being the absence of land and of sub-aërial decay of rock, and the presence wholly or principally of the material of the upper surface of the recently hardened crust. This being granted, the question arises, Ought we not to combine these several theories and to believe that the cooling crust has hardened in successive layers from without inward; that at the same time fissures were locally discharging igneous matter to the surface; that matter held in suspension in the ocean, and matter held in solution by heated waters rising from beneath the outer crust were mingling their materials in the deposits of the primitive ocean? It would seem that the combination of all these agencies may safely be invoked as causes of the pre-Atlantic deposits. This is the eclectic position which I endeavoured to maintain in my address before the Minneapolis meeting of the American Association in 1883, and which I still hold to be in every way probable.

A word here as to metamorphism, a theory which, like many others, has been first run to death and then discredited, but which to the moderate degree in which it was originally held by Lyell is still valid. Nothing can be more certain than that the composition of the Laurentian gneisses forbids us to suppose that they can be ordinary sediments metamorphosed. They are rocks peculiar in their origin, and not paralleled unless exceptionally in later times. On the other hand, they have undoubtedly experienced very important changes, more especially as to crystallisation, the state of combination of their ingredients, and the development of disseminated minerals;[1] and while this may in part be attributed to the mechanical pressure to which they have been subjected, it requires also the action of hydrothermic agencies. Any theory which fails to invoke both of these kinds of force must necessarily be partial and imperfect.

But all metamorphic rocks are not of the same character with the gneisses of the Lower Laurentian. Even in the Middle and Upper Laurentian we have metamorphic rocks, *e.g.* quartzite and limestone, which must originally have been ordinary aqueous deposits. Still more in the succeeding Huronian and its associated series of beds, and in the Lower Palæozoic, local metamorphic change has been undergone by rocks quite similar to those which in their unaltered state constitute regular sedimentary deposits. In the case of these later rocks it is to be borne in mind that, while some may have been of volcanic origin, others may have been sediments rich in undecomposed fragments of silicates. It is a mistake to suppose that the ordinary decay of stratified siliceous rocks is a process of kaolinisation so perfect as to eliminate all alkaline matters. On the contrary, the fact, which Judd has recently well illustrated in the case of the mud of the Nile, applies to a great number of similar deposits in all parts of the world, and shows that the finest sediments have not usually been so completely lixiviated as to be destitute of the basic matters necessary for their conversion into gneiss, mica-schist, and similar rocks when the necessary agencies of metamorphism are applied to them, and this quite independently of any extraneous matters introduced into them by water or otherwise. Still it must be steadily kept in view that many of the old pre-Cambrian crystalline rocks must have been different originally from those succeeding them, and that consequently these last even when metamorphosed present different characters.

I may remark here that, though a palæontologist rather than a lithologist, it gives me great pleasure to find so much attention now given in this country to the old crystalline rocks, and to their study microscopically and chemically as well as in the field, a work in which Sorby and Allport were pioneers. As a pupil of the late Prof. Jameson, of Edinburgh, my own attention was early attracted to the study of minerals and rocks as the stable foundations of geological science; and as far back as 1841 I had learnt of the late Mr. Sanderson, of Edinburgh, who worked at Nicol's sections, how to slice rocks and fossils; and since that time I have been in the habit of examining everything with the microscope. The modern developments in this direction are therefore very gratifying, even though, as is natural, they may sometimes appear to be pushed too far or their value over-estimated.

That these old gneisses were deposited not only in what is now the bed of the Atlantic, but also on the great continental areas of America and Europe, any one who considers the wide extent of these rocks represented on the map recently published by Prof. Hull can readily understand (*Trans.* Royal Irish Academy). It is true that Hull supposes that the basin of the Atlantic itself may have been land at this time, but there is no evidence of this, more especially as the material of the gneiss could not have been detritus derived from sub-aërial decay of rock.

Let us suppose, then, the floor of old Ocean covered with a flat pavement of gneiss, or of that material which is now gneiss, the next question is, How and when did this original bed become converted into sea and land? Here we have some things certain, others most debatable. That the cooling mass, especially if it was sending out volumes of softened rocky material, either in the exoplutonic or in the crenitic way, and piling this on the surface, must soon become too small for its shell, is apparent; but when and where would the collapse, crushing, and wrinkling inevitable from this cause begin? Where they did begin is indicated by the lines of mountain-chains which traverse the Laurentian districts; but the reason why is less apparent. The more or less unequal cooling, hardening, and conductive power of the outer crust we may readily assume. The driftage unequally of water-borne detritus to the south-west by the bottom currents of the sea is another cause, and, as we shall soon see, most effective. Still another is the greater cooling and hardening of the crust in the polar regions, and the tendency to collapse of the equatorial protuberance from the slackening of the earth's rotation. Besides these the internal tides of the earth's substance at the times of solstice would exert an oblique pulling force on the crust, which might tend to crack it along diagonal lines. From whichever of these causes or the combination of the whole, we know that within the Laurentian time folded portions of the earth's crust began to rise above the general surface in broad belts running from north-east to south-west, and from north-west to south-east, where the older mountains of Eastern America and Western Europe now stand, and that the subsidence of the oceanic areas allowed by this crumpling of the crust permitted other areas on both sides of what is now the Atlantic to form limited table-lands.[1] This was the beginning of a process repeated again and again in subsequent times, and which began in the Middle Laurentian, when for the first time we find beds of quartzite, limestone, and iron ore, and graphitic beds, indicating that there was already land and water, and that the sea, and perhaps the land, swarmed with animal and plant life of forms unknown to us for the most part now. Independently of the questions as to the animal nature of Eozoon, I hold that we know, as certainly as we can know anything inferentially, of the existence of these primitive forms of life. If I were to conjecture what were the early forms of plant and animal life, I would suppose that just as in the Palæozoic the acrogens culminated in gigantic and complex forest trees, so in the Laurentian the Algæ, the lichens, and the mosses grew to dimensions and assumed complexity of structure unexampled in later times, and that in the sea the humbler forms of Protozoa and Hydrozoa were the dominant types, but in gigantic and complex forms. The land of this period was probably limited, for the most part, to high latitudes, and its aspect, though more rugged and abrupt, and of greater elevation, must have been of that character which we still see in the Laurentian hills. The distribution of this ancient land is indicated by the long lines of old Laurentian rock extending from the Labrador coast and the north shore of the St. Lawrence, and along the eastern slopes of the Appalachians in America, and the like rocks of the Hebrides, the Western Highlands, and the Scandinavian mountains. A small but interesting remnant is that in the Malvern Hills, so well described by Holl. It will be well to note here, and to fix on our minds, that these ancient ridges of Eastern America and Western Europe have been greatly denuded and wasted since Laurentian times, and that it is along their eastern sides that the greatest sedimentary accumulations have been deposited.

From this time dates the introduction of that dominance of

and doleritic magmas, which have been conjectured to be the sources of two great types of eruptive rocks. Inasmuch, however, as according to the present hypothesis these two layers of basic and acidic matters are the results of aqueous action, and not of an original separation in a plutonic mass, as imagined by Phillips and Durocher, their composition would be subject to many local variations."

[1] The first of these is what Bonney has called *Metastasis*. The second and third come under the name *Metacrasis*. *Methylosis*, or change of substance, is altogether exceptional, and not to be credited except on the best evidence, or in cases where volatile matters have been expelled, as in the change of hæmatite into magnetite, or of bituminous coal into anthracite.

[1] Daubrée's curious experiments on the contraction of caoutchouc balloons partially hardened by coating with varnish, shows how small inequalities of the crust, from whatever cause arising, might affect the formation of wrinkles, and also that transverse as well as longitudinal wrinkling might occur.

existing causes which forms the basis of uniformitarianism in geology, and which had to go on with various and great modifications of detail, through the successive stages of the geological history, till the land and water of the northern hemisphere attained to their present complex structure.

So soon as we have a circumpolar belt or patches of Eozoic (or Archæan, or pre-Cambrian, if these terms are preferred) land, and ridges running southward from it, we enter on new and more complicated methods of growth of the continents and seas. Here we are indebted to Le Conte for clearly pointing out that our original Eozoic tracts of continent were in the earliest times areas of deposition, and that the first elevations of land out of the primæval ocean must have differed in important points from all that have succeeded them; but they were equally amenable to the ordinary laws of denudation. Portions of these oldest crystalline rocks, raised out of the protecting water, were now eroded by atmospheric agents, and especially by the carbonic acid, then existing in the atmosphere perhaps more abundantly than at present, under whose influence the hardest of the gneissic rocks gradually decay. The Arctic lands were subjected in addition to the powerful mechanical force of frost and thaw. Thus every shower of rain and every swollen stream would carry into the sea the products of the waste of land, sorting them into fine clays and coarser sands; and the cold currents which cling to the ocean bottom, now determined in their courses, not merely by the earth's rotation, but also by the lines of folding on both sides of the Atlantic, would carry south-westward, and pile up in marginal banks of great thickness, the debris produced from the rapid waste of the land already existing in the Arctic regions. The Atlantic, opening widely to the north, and having large rivers pouring into it, was especially the ocean characterised, as time advanced, by the prevalence of these phenomena. Thus throughout the geological history it has happened that, while the middle of the Atlantic has received merely organic deposits of shells of Foraminifera and similar organisms, and this probably only to a small amount, its margins have had piled upon them beds of detritus of immense thickness. Prof. Hall, of Albany, was the first geologist who pointed out the vast cosmic importance of these deposits, and that the mountains of both sides of the Atlantic owe their origin to these great lines of deposition, along with the fact, afterwards more fully insisted on by Rogers, that the portions of the crust which received these masses of debris became thereby weighted down and softened, and were more liable than other parts to lateral crushing.[1]

Thus in the later Eozoic and early Palæozoic times, which succeeded the first foldings of the oldest Laurentian, great ridges were thrown up, along the edges of which were beds of limestone, and on their summits and sides thick masses of ejected igneous rocks. In the bed of the central Atlantic there are no such accumulations. It must have been a flat, or slightly ridged, plate of the ancient gneiss, hard and resisting, though perhaps with a few cracks, through which igneous matter welled up, as in Iceland and the Azores in more modern times. In this condition of things we have causes tending to perpetuate and extend the distinctions of ocean and continent, mountain and plain, already begun; and of these we may more especially note the continued subsidence of the areas of greatest marine deposition. This has long attracted attention, and affords very convincing evidence of the connection of sedimentary deposit as a cause with the subsidence of the crust.[2]

We are indebted to a French physicist, M. Faye (*Revue Scientifique*, 1886), for an important suggestion on this subject. It is that the sediment accumulated along the shores of the ocean presented an obstacle to radiation, and consequently to cooling of the crust, while the ocean floor, unprotected and unweighted, and constantly bathed with currents of cold water, having great power of convection of heat, would be more rapidly cooled, and so would become thicker and stronger. This suggestion is complementary to the theory of Prof. Hall, that the areas of greatest deposit on the margins of the ocean are necessarily those of greatest folding and consequent elevation. We have thus a hard thick resisting ocean bottom which, as it settles down toward the interior, under the influence of gravity, squeezes upward and folds and plicates all the soft sediments deposited on its edges. The Atlantic area is almost an unbroken cake of this kind. The Pacific area has cracked in many places, allowing the interior fluid matter to ooze out in volcanic ejections.

It may be said that all this supposes a permanent continuance of the ocean basins, whereas many geologists postulate a mid-Atlantic continent[1] to give the thick masses of detritus found in the older formations both in Eastern America and Western Europe, and which thin off in proceeding into the interior of both continents. I prefer with Hall to consider these belts of sediment as in the main the deposits of northern currents, and derived from Arctic land, and that like the great banks of the American coast at the present day, which are being built up by the present Arctic current, they had little to do with any direct drainage from the adjacent shore. We need not deny, however, that such ridges of land as existed along the Atlantic margins were contributing their quota of river-borne material, just as on a still greater scale the Amazon and Mississippi are doing now, and this especially on the sides toward the present continental plateaus, though the greater part must have been derived from the wide tracts of Laurentian land within the Arctic Circle or near to it. It is further obvious that the ordinary reasoning respecting the necessity of continental areas in the present ocean basins would actually oblige us to suppose that the whole of the oceans and continents had repeatedly changed places. This consideration opposes enormous physical difficulties to any theory of alternations of the oceanic and continental areas, except locally at their margins. I would, however, refer you for a more full discussion of these points to the address to be delivered to-morrow by the President of the Geological Section.

But the permanence of the Atlantic depression does not exclude the idea of successive submergences of the continental plateaus and marginal slopes, alternating with periods of elevation, when the ocean retreated from the continents and contracted its limits. In this respect the Atlantic of to-day is much smaller than it was in those times when it spread widely over the continental plains and slopes, and much larger than it has been in times of continental elevation. This leads us to the further consideration that, while the ocean beds have been sinking, other areas have been better supported, and constitute the continental plateaus; and that it has been at or near the junctions of these sinking and rising areas that the thickest deposits of detritus, the most extensive foldings, and the greatest ejections of volcanic matter have occurred. There has thus been a permanence of the position of the continents and oceans throughout geological time, but with many oscillations of these areas, producing submergences and emergences of the land. In this way we can reconcile the vast vicissitudes of the continental areas in different geological periods with that continuity of development from north to south, and from the interiors to the margins, which is so marked a feature. We have for this reason to formulate another apparent geological paradox, namely, that while in one sense the continental and oceanic areas are permanent, in another they have been in continual movement. Nor does this view exclude extension of the continental borders or of chains of islands beyond their present limits, at certain periods; and indeed the general principle already stated, that subsidence of the ocean bed has produced elevation of the land, implies in earlier periods a shallower ocean and many possibilities as to volcanic islands, and low continental margins creeping out into

[1] The connection of accumulation with subsidence was always a familiar consideration with geologists; but Hall seems to have been the first to state its true significance as a geological factor, and to see that those portions of the crust which are weighted down by great detrital accumulations are necessarily those which, in succeeding movements, were elevated into mountains. Other American geologists, as Dana, Rogers, Hunt, Le Conte, Crosby, &c., have followed up Hall's primary suggestion, and in England, Hicks, Fisher, Starkie Gardner, Hull, and others, have brought it under notice, and it enters into the great generalisations of Lyell on these subjects.

[2] Dutton in "Report of U S. Geological Survey," 1881. From facts stated in this report and in my "Acadian Geology," it is apparent that in the Western States and in the coal-field of Nova Scotia shallow-water deposits have been laid down up to thicknesses of 10,000 to 20,000 feet in connection with continuous subsidence. See also a paper by Ricketts in the *Geol. Mag.* 1883. It may be well to add here that this doctrine of the subsidence of wide areas being caused by deposition does not justify the conclusion of certain glacialists that snow and ice have exercised a like power in glacial periods. In truth, as will appear in the sequel, great accumulations of snow and ice require to be preceded by subsidence, and wide continental areas can never be covered with deep snow, while of course ice can cause no addition of weight to submerged areas.

[1] Among American geologists, Dana and Le Conte, though from somewhat different premises, maintain continental permanence. Crosby has argued on the other side. In Britain, Hull has elaborated the idea of interchange of oceanic and continental areas in his memoir in *Trans.* Dublin Society, and in his work entitled "The Physical History of the British Islands." Godwin-Austen argues powerfully for the permanence of the Atlantic basin, *Q. J. Geol. Soc.* vol. xii. p. 42. Mellard Reade ably advocates the theory of mutation. The two views require, in my judgment, to be combined. More especially it is necessary to take into the account the existence of an Atlantic ridge of Laurentian rock on the west side of Europe, of which the Hebrides and the oldest rocks of Wales, Ireland, Western France, and Portugal are remnants.

the sea; while it is also to be noted that there are, as already stated, bordering shelves, constituting shallows in the ocean, which at certain periods have emerged as land.

We are thus compelled to believe in the contemporaneous existence in all geological periods, except perhaps the earliest of them, of three distinct conditions of areas on the surface of the earth. (1) Oceanic areas of deep sea, which always continued to occupy in whole or in part the bed of the present ocean. (2) Continental plateaus and marginal shelves, existing as low flats or higher table-lands liable to periodical submergence and emergence. (3) Lines of plication and folding, more especially along the borders of the oceans, forming elevated portions of land, rarely altogether submerged, and constantly affording the material of sedimentary accumulations, while they were also the seats of powerful volcanic ejections.

In the successive geological periods the continental plateaus when submerged, owing to their vast extent of warm and shallow sea, have been the great theatres of the development of marine life and of the deposition of organic limestones, and when elevated they have furnished the abodes of the noblest land faunas and floras. The mountain belts, especially in the north, have been the refuge and stronghold of land life in periods of submergence, and the deep ocean basins have been the perennial abodes of pelagic and abyssal creatures, and the refuge of multitudes of other marine animals and plants in times of continental elevation. These general facts are full of importance with reference to the question of the succession of formations and of life in the geological history of the earth.

So much time has been occupied with these general views that it would be impossible to trace the history of the Atlantic in detail through the ages of the Palæozoic, Mesozoic, and Tertiary. We may, however, shortly glance at the changes of the three kinds of surface already referred to. The bed of the ocean seems to have remained on the whole abyssal, but there were probably periods when those shallow reaches of the Atlantic which stretch across its most northern portion, and partly separate it from the Arctic basin, presented connecting coasts or continuous chains of islands sufficient to permit animals and plants to pass over.[1] At certain periods also there were not unlikely groups of volcanic islands, like the Azores, in the temperate or tropical Atlantic. More especially might this be the case in that early time when it was more like the present Pacific; and the line of the great volcanic belt of the Mediterranean, the mid-Atlantic banks, the Azores, and the West India Islands point to the possibility of such partial connections. These were stepping stones, so to speak, over which land organisms might cross, and some of these may be connected with the fabulous or prehistoric Atlantis.[2]

In the Cambrian and Ordovician periods the distinctions, already referred to, into continental plateaus, mountain-ridges, and ocean depths were first developed, and we find already great masses of sediment accumulating on the seaward sides of the old Laurentian ridges, and internal deposits thinning away from these ridges over the submerged continental areas, and presenting very dissimilar conditions of sedimentation. It would seem also that, as Hicks has argued for Europe, and Logan and Hall for America, this Cambrian age was one of slow subsidence of the land previously elevated, accompanied with or caused by thick deposits of detritus along the borders of the subsiding land, which was probably covered with the decomposing rock arising from long ages of sub-aërial waste.

In the coal-formation age, its characteristic swampy flats stretched in some places far into the shallower parts of the ocean.[3] In the Jurassic the American continent probably extended further to sea than at present. In the Wealden age there was much land to the west and north of Great Britain, and Prof. Bonney has directed attention to the evidence of the existence of this land as far back as the Trias, while Mr. Starkie Gardner has insisted on connecting-links to the southward, as evidenced by fossil plants. So late as the post-Glacial, or early human period, large tracts now submerged formed portions of the continents. On the other hand, the internal plains of America and Europe were often submerged. Such submergences are indicated by the great limestones of the Palæozoic, by the chalk and its representative beds in the Cretaceous, by the Nummulitic formation in the Eocene, and lastly by the great Pleistocene submergence, one of the most remarkable of all, one in which nearly the whole northern hemisphere participated, and which was probably separated from the present time by only a few thousands of years.[1] These submergences and elevations were not always alike on the two sides of the Atlantic. The Salina period of the Silurian, for example, and the Jurassic, show continental elevation in America not shared by Europe. The great subsidences of the Cretaceous and the Eocene were proportionally deeper and wider on the eastern continent, and this and the direction of the land being from north to south cause more ancient forms of life to survive in America. These elevations and submergences of the plateaus alternated with the periods of mountain-making plication, which was going on at intervals at the close of the Eozoic, at the beginning of the Cambrian, at the close of the Siluro-Cambrian, in the Permian, and in Europe and Western America in the Tertiary. The series of changes, however, affecting all these areas was of a highly complex character, and embraces the whole physical history of the geological ages.

We may note here that the unconformities caused by these movements and by subsequent denudation constitute what Le Conte has called "lost intervals," and one of the most important of which is supposed to have occurred at the end of the Eozoic. It is to be observed, however, that as every such movement is followed by a gradual subsidence, the seeming loss is caused merely by the overlapping of the successive beds deposited.

We may also note a fact which I have long ago insisted on ("Acadian Geology," 1865), the regular pulsations of the continental areas, giving us alternations in each great system of formations of deep-sea and shallow-water beds, so that the successive groups of formations may be divided into triplets of shallow-water, deep-water, and shallow-water strata, alternating in each period.

In referring to the ocean basins we should bear in mind that there are three of these in the northern hemisphere—the Arctic, the Pacific, and the Atlantic. De Rance has ably summed up the known facts as to Arctic geology, and more recently Dr. G. M. Dawson has prepared for the Royal Society of Canada a *résumé* and map of what is known of the geology of the Arctic basin (meeting of May 1886; the paper is not yet published), in comparison with Canadian geology. From this it appears that this area presents from without inwards a succession of older and newer formations from the Eozoic to the Tertiary, and that its extent must have been greater in former periods than at present, while it must have enjoyed a comparatively warm climate. The relations of its deposits and fossils are closer with those of the Atlantic than with those of the Pacific, as might be anticipated from its wider opening into the former. Blandford has recently remarked on the correspondence of the marginal deposits around the Pacific and Indian Oceans,[2] and Dr. Dawson informs me that this is equally marked in comparison with the west coast of America,[3] but these marginal areas have not yet gained much on the ocean. In the North Atlantic, on the other hand, there is a wide belt of comparatively modern rocks on both sides, more especially toward the south, and on the American side; but while there appears to be a perfect correspondence on both sides of the Atlantic and around the Pacific respect-

[1] It would seem, from Geikie's description of the Faroe Islands, that they may be a remnant of such connecting land, dating from the Cretaceous or Eocene period.

[2] Dr. Wilson has recently argued that the Atlantis of tradition was really America, and Mr. Hyde Clark has associated this idea with the early dominance in Western Europe of the Iberian race, which Dawkins connects with the Neolithic and Bronze Ages of archæology. My own attention has recently been directed, through specimens presented to the McGill College Museum, to the remarkable resemblances in cranial characters, wampum, and other particulars, of the Guanches of the Canaries with aborigines of Eastern America—resemblances which cannot be accidental.

[3] I have shown the evidence of this in the remnants of Carboniferous districts once more extensive on the Atlantic coast of Nova Scotia and Cape Breton ("Acadian Geology").

[1] The recent surveys of the Falls of Niagara coincide with a great many evidences to which I have elsewhere referred in proving that the Pleistocene submergence of America and Europe came to an end not more than ten thousand years ago, and was itself not of very great duration. Thus in Pleistocene times the land must have been submerged and re-elevated in a very rapid manner.

[2] A singular example is the recurrence in New Zealand of Triassic rocks and fossils of types corresponding to those of British Columbia. A curious modern analogy appears in the works of art of the Maoris with those of the Haida Indians of the Queen Charlotte Islands, and both are eminently Pacific in contradistinction to Atlantic.

[3] *Journal* of Geological Society, May 1886. Blandford's statements respecting the mechanical deposits of the close of the Palæozoic in the Pacific area, whether these are glacial or not, would seem to show a correspondence with the Permian conglomerates and earth-movements of the Atlantic area; but since that time the Atlantic has enjoyed comparative repose. The Pacific also seems to have reproduced the conditions of the Carboniferous in the Cretaceous age, and seems to have been less affected by the great changes of the Pleistocene.

ively, there seems to be less parallelism between the deposits and forms of life of the two oceans as compared with each other, and less correspondence in forms of life, especially in modern times. Still in the earlier geological ages, as might have been anticipated from the imperfect development of the continents, the same forms of life characterise the whole ocean from Australia to Arctic America, and indicate a grand unity of Pacific and Atlantic life not equalled in later times,[1] and which speaks of contemporaneity rather than of what has been termed homotaxis.

We may pause here for a moment to notice some of the effects of Atlantic growth on modern geography. It has given us rugged and broken shores composed of old rocks in the north, and newer formations and softer features towards the south. It has given us marginal mountain-ridges and internal plateaus on both sides of the sea. It has produced certain curious and by no means accidental correspondences of the eastern and western sides. Thus the solid basis on which the British Islands stand may be compared with Newfoundland and Labrador, the English Channel with the Gulf of St. Lawrence, the Bay of Biscay with the Bay of Maine, Spain with the projection of the American land at Cape Hatteras, the Mediterranean with the Gulf of Mexico. The special conditions of deposition and plication necessary to these results, and their bearing on the character and productions of the Atlantic basin, would require a volume for their detailed elucidation.

Thus far our discussion has been limited almost entirely to physical causes and effects. If we now turn to the life-history of the Atlantic, we are met at the threshold with the question of climate, not as a thing fixed and immutable, but as changing from age to age in harmony with geographical mutations, and producing long cosmic summers and winters of alternate warmth and refrigeration.

We can scarcely doubt that the close connection of the Atlantic and Arctic Oceans is one factor in those remarkable vicissitudes of climate experienced by the former, and in which the Pacific area has also shared in connection with the Antarctic Sea. No geological facts are indeed at first sight more strange and inexplicable than the changes of climate in the Atlantic area, even in comparatively modern periods. We know that in the early Tertiary perpetual summer reigned as far north as the middle of Greenland, and that in the Pleistocene the Arctic cold advanced, until an almost perennial winter prevailed, half-way to the equator. It is no wonder that nearly every cause available in the heavens and the earth has been invoked to account for these astounding facts.

It will, I hope, meet with the approval of your veteran glaciologist, Dr. Crosskey, if, neglecting most of these theoretical views, I venture to invite your attention in connection with this question chiefly to the old Lyellian doctrine of the modification of climate by geographical changes. Let us, at least, consider how much these are able to account for.[2]

The ocean is a great equaliser of extremes of temperature. It does this by its great capacity for heat and by its cooling and heating power when passing from the solid into the liquid and gaseous states, and the reverse. It also acts by its mobility, its currents serving to convey heat to great distances, or to cool the air by the movement of cold icy waters. The land, on the other hand, cools or warms rapidly, and can transmit its influence to a distance only by the winds, and the influence so transmitted is rather in the nature of a disturbing than of an equalising cause. It follows that any change in the distribution of land and water must affect climate, more especially if it changes the character or course of the ocean currents.[3]

[1] Daintree and Etheridge, "Queensland Geology," *Journal* of Geological Society, August 1872; R. Etheridge, Junior, "Australian Fossils," *Trans. Phys. Soc.*, Edin., 1880.

[2] The late Mr. Searles V. Wood, in an able summary of the possible causes of the succession of cold and warm climates in the northern hemisphere, enumerates no fewer than seven theories which have met with more or less acceptance. These are:—

(1) The gradual cooling of the earth from a condition of original incandescence.

(2) Changes in the obliquity of the ecliptic.

(3) Changes in the position of the earth's axis of rotation.

(4) The effect of the precession of the equinoxes along with changes of the eccentricity of the earth's orbit.

(5) Variations in the amount of heat given off by the sun.

(6) Differences in the temperature of portions of space passed through by the earth.

(7) Differences in the distribution of land and water in connection with the flow of oceanic currents.

[3] Von Woeikoff has very strongly put these principles in a review of Croll's recent book, "Climate and Cosmology," *American Journal of Science*, March 1886.

At the present time the North Atlantic presents some very peculiar, and in some respects exceptional, features, which are most instructive with reference to its past history. The great internal plateau of the American continent is now dry land; the passage across Central America between the Atlantic and Pacific is blocked; the Atlantic opens very widely to the north; the high mass of Greenland towers in its northern part. The effects are that the great equatorial current running across from Africa and embayed in the Gulf of Mexico, is thrown northward and eastward in the Gulf Stream, acting as a hot-water apparatus to heat up to an exceptional degree the western coast of Europe. On the other hand, the cold Arctic current from the Polar seas is thrown to the westward, and runs down from Greenland past the American shore.[1] The pilot chart for June of this year shows vast fields of drift ice on the western side of the Atlantic as far south as the latitude of 40°. So far, therefore, the Glacial age in that part of the Atlantic still extends; this at a time when, on the eastern side of the Atlantic, the culture of cereals reaches in Norway beyond the Arctic Circle. Let us inquire into some of the details of these phenomena.

The warm water thrown into the North Atlantic not only increases the temperature of its whole waters, but gives an exceptionally mild climate to Western Europe. Still the countervailing influence of the Arctic currents and the Greenland ice is sufficient to permit icebergs which creep down to the mouth of the Strait of Belle Isle, in the latitude of the south of England, to remain unmelted till the snows of a succeeding winter fall upon them. Now let us suppose that a subsidence of land in tropical America were to allow the equatorial current to pass through into the Pacific. The effect would at once be to reduce the temperature of Norway and Britain to that of Greenland and Labrador at present, while the latter countries would themselves become colder. The northern ice, drifting down into the Atlantic, would not, as now, be melted rapidly by the warm water which it meets in the Gulf Stream. Much larger quantities of it would remain undissolved in summer, and thus an accumulation of permanent ice would take place, along the American coast at first, but probably at length even on the European side. This would still further chill the atmosphere, glaciers would be established on all the mountains of temperate Europe and America,[2] the summer would be kept cold by melting ice and snow, and at length all Eastern America and Europe might become uninhabitable, except by Arctic animals and plants, as far south as perhaps 40° of north latitude. This would be simply a return of the Glacial age. I have assumed only one geographical change; but other and more complete changes of subsidence and elevation might take place, with effects on climate still more decisive; more especially would this be the case if there were a considerable submergence of the land in temperate latitudes.

We may suppose an opposite case. The high plateau of Greenland might subside, or be reduced in height, and the openings of Baffin's Bay and the North Atlantic might be closed. At the same time the interior plain of America might be depressed, so that, as we know to have been the case in the Cretaceous period, the warm waters of the Mexican Gulf would circulate as far north as the basins of the present great American lakes. In these circumstances there would be an immense diminution of the sources of floating ice, and a correspondingly vast increase in the surface of warm water. The effects would be to enable a temperate flora to subsist in Greenland, and to bring all the present temperate regions of Europe and America into a condition of subtropical verdure.

It is only necessary to add that we know that vicissitudes not dissimilar from those above sketched have actually occurred in comparatively recent geological times, to enable us to perceive that we can dispense with all other causes of change of climate, though admitting that some of them may have occupied a secondary place.[3] This will give us, in dealing with the distribution of life, the great advantage of not being tied up to definite astronomical cycles of glaciation, which may not always suit the geological facts, and of correlating elevation and subsidence of the land with changes of climate affecting living beings. It

[1] I may refer here to the admirable expositions of these effects by the late Dr. Carpenter in his papers on the results of the explorations of the *Challenger*.

[2] According to Bonney, the west coast of Wales is about 12° above the average for its latitude, and if reduced to 12 below the average its mountains would have large glaciers.

[3] More especially the ingenious and elaborate arguments of Croll deserve consideration; and, though I cannot agree with him in his main thesis, I gladly acknowledge the great utility of the work he has done.

will, however, be necessary, as Wallace well insists, that we shall hold to that degree of fixity of the continents in their position, notwithstanding the submergences and emergences they have experienced, to which I have already adverted. Sir Charles Lyell, more than forty years ago, published in his "Principles of Geology" two imaginary maps which illustrate the extreme effects of various distribution of land and water. In one all the continental masses are grouped around the equator. In the other they are all placed around the poles, leaving an open equatorial ocean. In the one case the whole of the land and its inhabitants would enjoy a perpetual summer, and scarcely any ice could exist in the sea. In the other the whole of the land would be subjected to an Arctic climate, and it would give off immense quantities of ice to cool the ocean. But Lyell did not suppose that any such distribution as that represented in his maps had actually occurred, though this supposition has been sometimes attributed to him. He merely put what he regarded as an extreme case to illustrate what might occur under conditions less exaggerated. Sir Charles, like other thoughtful geologists, was well aware of the general fixity of the areas of the continents, though with great modifications in the matter of submergence and of land conditions. The union, indeed, of these two great principles of fixity and diversity of the continents lies at the foundation of theoretical geology.

We can now more precisely indicate this than was possible when Lyell produced his "Principles," and can reproduce the conditions of our continents in even the more ancient periods of their history. Some examples may be taken from the history of the American continent, which is more simple in its arrangements than the double continent of Europ-asia. We may select the early Devonian or Erian period, in which the magnificent flora of that age—the earliest certainly known to us—made its appearance. Imagine the whole interior plain of North America submerged, so that the continent is reduced to two strips on the east and west, connected by a belt of Laurentian land on the north. In the great mediterranean sea thus produced, the tepid water of the equatorial current circulated, and it swarmed with corals, of which we know no less than 150 species, and with other forms of life appropriate to warm seas. On the islands and coasts of this sea was introduced the Erian flora, appearing first in the north; and with that vitality and colonising power of which, as Hooker has well shown, the Scandinavian flora is the best modern type, spreading itself to the south.[1] A very similar distribution of land and water in the Cretaceous age gave a warm and equable climate in those portions of North America not submerged, and coincided with the appearance of the multitude of broad-leaved trees of modern types introduced in the early and middle Cretaceous, and which prepared the way for the mammalian life of the Eocene. We may take a still later instance from the second continental period of the later Pleistocene or early modern, when there would seem to have been a partial or entire closure of the North Atlantic against the Arctic ice, and wide extensions seaward of the European and American land, with possibly considerable tracts of land in the vicinity of the equator, while the Mediterranean and the Gulf of Mexico were deep inland lakes (Dawkins, *Popular Science Monthly*, 1873). The effect of such conditions on the climates of the northern hemisphere must have been prodigious, and their investigation is rendered all the more interesting because it would seem that this continental period of the post-Glacial age was that in which man made his first acquaintance with the coasts of the Atlantic, and possibly made his way across its waters.

We have in America ancient periods of cold as well as of warmth. I have elsewhere referred to the boulder conglomerates of the Huronian, of the Cambrian and Ordovician, of the Millstone-grit period of the Carboniferous and of the early Permian, but would not venture to affirm that either of these periods was comparable in its cold with the later Glacial age, still less with that imaginary age of continental glaciation assumed by certain of the more extreme theorists ("Notes on Post-Pliocene of Canada"; "Pre-Cambrian Glaciers," *Geol. Mag.*, 1880). These ancient conglomerates were probably produced by floating ice, and this at periods when in areas not very remote temperate floras and faunas could flourish. The glacial periods of our old continent occurred in times when the surface of the submerged land was opened up to the northern currents, drifting over it mud and sand and stones, and rendering nugatory, in so far at least as the bottom of the sea was concerned, the effects of the superficial warm streams. Some of these beds are also peculiar to the eastern margin of the continent, and indicate ice-drift along the Atlantic coast in the same manner as at present, while conditions of greater warmth existed in the interior. Even in the more recent Glacial age, while the mountains were covered with snow and the lowlands submerged under a sea laden with ice, there were interior tracts in somewhat high latitudes of America in which hardy forest trees and herbaceous plants flourished abundantly; and these were by no means exceptional "interglacial" periods. Thus we can show that, while from the remote Huronian period to the Tertiary the American land occupied the same position as at present, and while its changes were merely changes of relative level as compared with the sea, these have so influenced the ocean currents as to cause great vicissitudes of climate.

Without entering on any detailed discussion of that last and greatest Glacial period which is best known to us, and is more immediately connected with the early history of man and the modern animals, it may be proper to make a few general statements bearing on the relative importance of sea-borne and land ice in producing those remarkable phenomena attributable to ice action in this period. In considering this question it must be borne in mind that the greater masses of floating ice are produced at the seaward extremities of land glaciers, and that the heavy field-ice of the Arctic regions is not so much a result of the direct freezing of the surface of the sea as of the accumulation of snow precipitated on the frozen surface. In reasoning on the extent of ice action, and especially of glaciers in the Pleistocene age, it is necessary to keep this fully in view. Now in the formation of glaciers at present—and it would seem also in any conceivable former state of the earth—it is necessary that extensive evaporation should conspire with great condensation of water in the solid form. Such conditions exist in mountainous regions sufficiently near to the sea, as in Greenland, Norway, the Alps, and the Himalayas; but they do not exist in low Arctic lands like Siberia or Grinnell Land, nor in inland mountains. It follows that land-glaciation has narrow limits, and that we cannot assume the possibility of great confluent or continental glaciers covering the interior of wide tracts of land. No imaginable increase of cold could render this possible, inasmuch as there could not be a sufficient influx of vapour to produce the necessary condensation; and the greater the cold, the less would be the evaporation. On the other hand, any increase of heat would be felt more rapidly in the thawing and evaporation of land ice and snow than on the surface of the sea.

Applying these very simple geographical truths to the North Atlantic continents, it is easy to perceive that no amount of refrigeration could produce a continental glacier, because there could not be sufficient evaporation and precipitation to afford the necessary snow in the interior. The case of Greenland is often referred to, but this is the case of a high mass of cold land with sea, mostly open, on both sides of it, giving, therefore, the conditions most favourable to precipitation of snow. If Greenland were less elevated, or if there were dry plains around it, the case would be quite different, as Nares has well shown by his observations on the summer verdure of Grinnell Land, which, in the immediate vicinity of North Greenland, presents very different conditions as to glaciation and climate.[1] If the plains were submerged, and the Arctic currents allowed free access to the interior of the continent of America, it is conceivable that the mountainous regions remaining out of water would be covered with snow and ice, and there is the best evidence that this actually occurred in the Glacial period; but with the plains out of water this would be impossible. We see evidence of this at the present day in the fact that in unusually cold winters the great precipitation of snow takes place south of Canada, leaving the north comparatively bare, while as the temperature becomes milder the area of snow deposit moves farther to the north. Thus a greater extension of the Atlantic, and especially of its cold ice-laden Arctic currents, becomes the most potent cause of a glacial age.

I have long maintained these conclusions on general geographical grounds, as well as on the evidence afforded by the Pleistocene deposits of Canada; and in an address the theme of

[1] As I have elsewhere endeavoured to show ("Report on Silurian and Devonian Plants of Canada"), a warm climate in the Arctic region seems to have afforded the necessary conditions for the great colonising floras of all geological periods. Gray had previously illustrated the same fact in the case of the more modern floras.

[1] These views have been admirably illustrated by Von Woeikoff in the paper already referred to, and in previous geographical papers.

which is the ocean I may be excused for continuing to regard the supposed terminal moraines of great continental glaciers as nothing but the southern limit of the ice-drift of a period of submergence. In such a period the southern margin of an ice-laden sea where its floe-ice and bergs grounded, or where its ice was rapidly melted by warmer water, and where consequently its burden of boulders and other debris was deposited, would necessarily present the aspect of a moraine, which by the long continuance of such conditions might assume gigantic dimensions. Let it be observed, however, that I fully admit the evidence of the great extension of local glaciers in the Pleistocene age, and especially in the times of partial submergence of the land.

I am quite aware that it has been held by many able American geologists[1] that in North America a continental glacier extended in temperate latitudes from sea to sea, or at least from the Atlantic to the Rocky Mountains, and that this glacier must, in many places, have exceeded a mile in thickness. The reasons above stated appear, however, sufficient to compel us to seek for some other explanation of the observed facts, however difficult this may at first sight appear. With a depression such as we know to have existed, admitting the Arctic currents along the St. Lawrence Valley, through gaps in the Laurentian watershed, and down the great plains between the Laurentian areas and the Rocky Mountains, we can easily understand the covering of the hills of Eastern Canada and New England with ice and snow, and a similar covering of the mountains of the west coast. The sea also in this case might be ice-laden and boulder-bearing as far south as 40°, while there might still be low islands far to the north on which vegetation and animals continued to exist. We should thus have the conditions necessary to explain all the anomalies of the glacial deposits. Even the glaciation of high mountains south of the St. Lawrence Valley would then become explicable by the grounding of floe-ice on the tops of these mountains when reefs in the sea. In like manner we can understand how on the isolated trappean hill of Belœil, in the St. Lawrence Valley, Laurentian boulders far removed from their native seats to the north are perched at a height of about 1200 feet on a narrow peak where no glacier could possibly have left them. The so-called moraine, traceable from the great Missouri Coteau in the west to the coasts of New Jersey, would thus become the mark of the western and southern limit of the subsidence, or of the line along which the cold currents bearing ice were abruptly cut off by warm surface waters. I am glad to find that these considerations are beginning to have weight with European geologists in their explanation of the glacial drift of the great plains of Northern Europe.

Whatever difficulties may attend such a supposition, they are small compared with those attendant on the belief of a continental glacier, moving without the aid of gravity, and depending for its material on the precipitation taking place on the interior plains of a great continent.

I have elsewhere endeavoured to show, on the evidence found in Canada, that the occurrence of marine shells, land plants, and insects in the glacial deposits of that country indicates not so much the effect of general interglacial periods as the local existence of conditions like those of Grinnell Land and Greenland, in proximity to each other at one and the same period, and depending on the relative levels of land and the distribution of ocean currents and ice-drift.[2]

I am old enough to remember the sensation caused by the delightful revelations of Edward Forbes respecting the zones of animal life in the sea, and the vast insight which they gave into the significance of the work on minute organisms previously done by Ehrenberg, Lonsdale, and Williamson, and into the meaning of fossil remains. A little later the soundings for the Atlantic cable revealed the chalky foraminiferal ooze of the abyssal ocean; still more recently the wealth of facts disclosed by the *Challenger* voyage, which naturalists have not yet had time to digest, have opened up to us new worlds of deep-sea life.

The bed of the deep Atlantic is covered for the most part by a mud or ooze largely made up of the debris of Foraminifera and other minute organisms mixed with fine clay. In the North Atlantic the Norwegian naturalists call this Biloculina mud. Further south the *Challenger* naturalists speak of it as Globigerina ooze. In point of fact it contains different species of foraminiferal shells, Globigerina and Orbulina being in some localities dominant, and in others other species, and these changes are more apparent in the shallower portions of the ocean.

It is also to be observed that there are means for disseminating coarse material over the ocean bed. There are in the line of the Arctic current on the American coast great sand-banks, and off the coast of Norway sand constitutes a considerable part of the bottom material. Soundings and dredgings off Great Britain, and also off the American coast, have shown that fragments of stone referable to Arctic lands are abundantly strewn over the bottom along certain lines, and the Antarctic continent, otherwise almost unknown, makes its presence felt to the dredge by the abundant masses of crystalline rock drifted far from it to the north. These are not altogether new discoveries. I had inferred many years ago, from stones taken up by the hooks of fishermen on the banks of Newfoundland, that rocky material from the north is dropped on these banks by the heavy ice which drifts over them every spring, that these stones are glaciated, and that after they fall to the bottom sand is drifted over them with sufficient velocity to polish the stones and to erode the shelly coverings of Arctic animals attached to them ("Notes on Post-Pliocene of Canada," 1872). If then the Atlantic basin were upheaved into land we should see beds of sand, gravel, and boulders with clay flats and layers of marl and limestone. According to the *Challenger* Reports, in the Antarctic seas south of 64° there is blue mud with fragments of rock in depths of 1200 to 2000 fathoms. The stones, some of them glaciated, were granite, diorite, amphibolite, mica-schist, gneiss, and quartzite. This deposit ceases and gives place to Globigerina ooze and red clay at 46° to 47° S., but even further north there is sometimes as much as 49 per cent. of crystalline sand. In the Labrador current a block of syenite weighing 490 pounds was taken up from 1340 fathoms, and in the Arctic current, 100 miles from land, was a stony deposit, some stones being glaciated. Among these were smoky quartz, quartzite, limestone, dolomite, mica-schist, and serpentine; also particles of monoclinic and triclinic feldspar, hornblende, augite, magnetite, mica, and glauconite, the latter no doubt formed in the sea-bottom, the others drifted from Eozoic and Palæozoic formations to the north ("General Report, *Challenger* Expedition").

A remarkable fact in this connection is that the great depths of the sea are as impassable to the majority of marine animals as the land itself. According to Murray, while twelve of the *Challenger's* dredgings taken in depths greater than 2000 fathoms gave 92 species, mostly new to science, a similar number of dredgings in shallower water near the land gave no less than 1000 species. Hence arises another apparent paradox relating to the distribution of organic beings. While at first sight it might seem that the chances of wide distribution are exceptionally great for marine species, this is not so. Except in the case of those which enjoy a period of free locomotion when young, or are floating and pelagic, the deep ocean sets bounds to their migrations. On the other hand, the spores of cryptogamic plants may be carried for vast distances by the wind, and the growth of volcanic islands may effect connections which, though only temporary, may afford opportunity for land animals and plants to pass over.

But I must here call your attention to still another geological paradox, namely, that the deep sea, which is so great a barrier to the passage of the shallow-water animals, seems, under certain conditions, to afford facilities for the transmission of land animals and plants. The connections established by the observations of the *Challenger*, and so well expounded by Wallace and Hemsley ("Continental and Island Life"; "Botany of the *Challenger* Expedition"), between the floras of oceanic islands and the continents, establish this conclusively. Thus the Bermudas, altogether recent islands, have been stocked by the agency chiefly of the ocean currents and of birds, with nearly 150 species of continental plants, and the facts collected by Hemsley as to the present facilities of transmission, along with the evidence afforded by older oceanic islands which have been receiving animal and vegetable colonists for longer periods, go far to show that, time being given, the sea actually affords facilities for the migrations of the inhabitants of the land, greater than those of continuous continents.

With reference to the transmission of living beings across the Atlantic, we have before us the remarkable fact that from the Cambrian age onwards there were on the two sides of the ocean many species of invertebrate animals which were either

[1] Report of Mr. Carvill Lewis in "Pennsylvania Geological Survey," 1884; also Dana's "Manual."

[2] "Notes on Post-Pliocene of Canada," 1872. One well-marked interval only has been established in the glacial deposits of Canada.

identical or so closely allied as to be possibly varietal forms.[1] In like manner the early plants of the Upper Silurian, Devonian, and Carboniferous present many identical species; but this identity becomes less marked in the vegetation of the more modern times.

In so far as plants are concerned, it is to be observed that the early forests were largely composed of cryptogamous plants, and the spores of these in modern times have proved capable of transmission for great distances. In considering this we cannot fail to conclude that the union of simple cryptogamous fructification with arboreal stems of high complexity, so well illustrated by Dr. Williamson, had a direct relation to the necessity for a rapid and wide distribution of these ancient trees. It seems also certain that some spores, as, for example, those of the Rhizocarps (see paper by the author on "Palæozoic Rhizocarps," *Chicago Transactions*, 1886), a type of vegetation abundant in the Palæozoic, and certain kinds of seeds, as those named *Ætheotesta* and *Pachytheca*, were fitted for flotation. Further, the periods of Arctic warmth permitted the passage around the northern belt of many temperate species of plants, just as now happens with the Arctic flora; and when these were dispersed by colder periods they marched southward along both sides of the sea on the mountain-chains.

The same remark applies to northern forms of marine invertebrates, which are much more widely distributed in longitude than those further south. The late Mr. Gwyn Jeffreys, in one of his latest communications to this Association, stated that 54 per cent. of the shallow-water mollusks of New England and Canada are also European, and of the deep-sea forms 30 out of 35; these last of course enjoying greater facilities for migration than those which have to travel slowly along the shallows of the coasts in order to cross the ocean and settle themselves on both sides. Many of these animals, like the common mussel and sand clam, are old settlers which came over in the Pleistocene period, or even earlier. Others, like the common periwinkle, seem to have been slowly extending themselves in modern times, perhaps even by the agency of man. The older immigrants may possibly have taken advantage of lines of coast now submerged, or of warm periods, when they could creep around by the Arctic shores. Mr. Herbert Carpenter and other naturalists employed on the *Challenger* collections have made similar statements respecting other marine invertebrates, as, for instance, the Echinoderms, of which the deep-sea crinoids present many common species, and my own collections prove that many of the shallow-water forms are common. Dall ("Report on Alaska") and Whiteaves (*Transactions R. S. C.*) have shown that some mollusks and Echinoderms are common even to the Atlantic and Pacific coasts of North America; a remarkable fact, testifying at once to the fixity of these species and to the manner in which they have been able to take advantage of geographical changes. Some of the species of whelks common to the Gulf of St. Lawrence and the Pacific are animals which have no special locomotive powers even when young, but they are northern forms not proceeding far south, so that they may have passed through the Arctic seas. In this connection it is well to remark that many species of animals have powers of locomotion when young which they lose when adult, and that others may have special means of transit. I once found at Gaspé a specimen of the Pacific species of Coronula, or whale-barnacle, the *C. reginæ* of Darwin, attached to a whale taken in the Gulf of St. Lawrence, and which had probably succeeded in making that passage around the north of America which so many navigators have essayed in vain.

It is to be remarked here that while many plants and marine invertebrates are common to the two sides of the Atlantic, it is different with land animals, and especially vertebrates. I do not know that any fossil insects or land-snails or millipedes of Europe and America are specifically identical, and of the numerous species of batrachians of the Carboniferous and reptiles of the Mesozoic all seem to be distinct on the two sides. The same appears to be the case with the Tertiary mammals, until in the later stages of that great period we find such genera as the horse, the camel, and the elephant appearing on the two sides of the Atlantic; but even then the species seem different, except in the case of a few northern forms.

[1] See Davidson's "Monographs on Brachiopods": Etheridge, "Address to Geological Society of London"; Woodward, "Address to Geologists' Association"; also Barrande's "Special Memoirs on the Brachiopods, Cephalopods, &c."; Hall, "Palæontology of New York"; Billings, "Reports on Canadian Fossils"; and Matthews, "Cambrian of New Brunswick," *Trans. R.S.C.*

Some of the longer-lived mollusks of the Atlantic furnish suggestions which remarkably illustrate the biological aspect of these questions. Our familiar friend the oyster is one of these. The first known oysters appear in the Carboniferous in Belgium and in the United States of America. In the Carboniferous and Permian they are few and small, and they do not culminate till the Cretaceous, in which there are no less than ninety-one so-called species in America alone; but some of the largest known species are found in the Eocene. The oyster, though an inhabitant of shallow water, and very limitedly locomotive when young, has survived all the changes since the Carboniferous age, and has spread itself over the whole northern hemisphere (White, "Report U.S. Geol. Survey, 1882-83").

I have collected fossil oysters in the Cretaceous clays of the *coulées* of Western Canada, in the Lias shales of England, in the Eocene and Cretaceous beds of the Alps, of Egypt, of the Red Sea coast, of Judæa, and the heights of Lebanon. Everywhere and in all formations they present forms which are so variable and yet so similar that one might suppose all the so-called species to be mere varieties. Did the oyster originate separately on the two sides of the Atlantic, or did it cross over so promptly that its appearance seems to be identical on the two sides? Are all the oysters of a common ancestry, or did the causes, whatever they were, which introduced the oyster in the Carboniferous act over again in later periods? Who can tell? This is one of the cases where causation and development—the two scientific factors which constitute the basis of what is vaguely called evolution—cannot easily be isolated. I would recommend to those biologists who discuss these questions to addict themselves to the oyster. This familiar mollusk has successfully pursued its course and has overcome all its enemies, from the flat-toothed selachians of the Carboniferous, to the oyster-dredgers of the present day, has varied almost indefinitely, and yet has continued to be an oyster, unless indeed it may at certain portions of its career have temporarily assumed the disguise of a Gryphæa or an Exogyra. The history of such an animal deserves to be traced with care, and much curious information respecting it will be found in the report which I have cited.

But in these respects the oyster is merely an example of many forms. Similar considerations apply to all those Pliocene and Pleistocene mollusks which are found in the raised sea-bottoms of Norway and Scotland, on the top of Moel Tryfaen in Wales, and at similar great heights on the hills of America, many of which can be traced back to early Tertiary times, and can be found to have extended themselves all over the seas of the northern hemisphere. They apply in like manner to the ferns, the conifers, and the angiosperms, many of which we can now follow without even specific change to the Eocene and Cretaceous. They all show that the forms of living things are more stable than the lands and seas in which they live. If we were to adopt some of the modern ideas of evolution, we might cut the Gordian knot by supposing that, as like causes can produce like effects, these types of life have originated more than once in geological time, and need not be genetically connected with each other. But while evolutionists repudiate such an application of their doctrine, however natural and rational, it would seem that nature still more strongly repudiates it, and will not allow us to assume more than one origin for one species. Thus the great question of geographical distribution remains in all its force, and, by still another of our geological paradoxes, mountains become ephemeral things in comparison with the delicate herbage which covers them, and seas are in their present extent but of yesterday when compared with the minute and feeble organisms that creep on their sands or swim in their waters.

The question remains, Has the Atlantic achieved its destiny and finished its course, or are there other changes in store for it in the future? The earth's crust is now thicker and stronger than ever before, and its great ribs of crushed and folded rock are more firm and rigid than in any previous period. The stupendous volcanic phenomena manifested in Mesozoic and early Tertiary times along the borders of the Atlantic have apparently died out. These facts are in so far guarantees of permanence. On the other hand, it is known that movements of elevation along with local depression are in progress in the Arctic regions, and a great weight of new sediment is being deposited along the borders of the Atlantic, especially on its western side, and this is not improbably connected with the earthquake shocks and slight movements of depression which have occurred in North America. It is possible that these slow and secular movements may go on uninterruptedly until

considerable changes are produced; but it is quite as likely that they may be retarded or reversed.

It is possible, on the other hand, that after the long period of quiescence which has elapsed there may be a new settlement of the ocean bed, accompanied with foldings of the crust, especially on the western side of the Atlantic, and possibly with renewed volcanic activity on its eastern margin. In either case a long time relatively to our limited human chronology may intervene before the occurrence of any marked change. On the whole, the experience of the past would lead us to expect movements and eruptive discharges in the Pacific rather than in the Atlantic area. It is therefore not unlikely that the Atlantic may remain undisturbed, unless secondarily and indirectly, until after the Pacific area shall have attained to a greater degree of quiescence than at present. But this subject is one too much involved in uncertainty to warrant us in following it further.

In the meantime the Atlantic is to us a practically permanent ocean, varying only in its tides, its currents, and its winds, which science has already reduced to definite laws, so that we can use if we cannot regulate them. It is ours to take advantage of this precious time of quietude, and to extend the blessings of science and of our Christian civilisation from shore to shore until there shall be no more sea, not in the sense of that final drying-up of old Ocean to which some physicists look forward, but in the higher sense of its ceasing to be the emblem of unrest and disturbance, and the cause of isolation.

I must now close this address with a short statement of the general objects which I have had in view in directing your attention to the geological development of the Atlantic. We cannot, I think, consider the topics to which I have referred without perceiving that the history of ocean and continent is an example of progressive design, quite as much as that of living beings. Nor can we fail to see that, while in some important directions we have penetrated the great secret of Nature, in reference to the general plan and structure of the earth and its waters, and the changes through which they have passed, we have still very much to learn, and perhaps quite as much to unlearn, and that the future holds out to us and to our successors higher, grander, and clearer conceptions than those to which we have yet attained. The vastness and the might of Ocean, and the manner in which it cherishes the feeblest and most fragile beings, alike speak to us of Him who holds it in the hollow of His hand, and gave to it of old its boundaries and its laws; but its teaching ascends to a higher tone when we consider its origin and history, and the manner in which it has been made to build up continents and mountain-chains, and at the same time to nourish and sustain the teeming life of sea and land.

SECTION A

MATHEMATICAL AND PHYSICAL SCIENCE

Opening Address by Prof. G. H. Darwin, M.A., LL.D., F.R.S., F.R.A.S., President of the Section

A mere catalogue of facts, however well arranged, has never led to any important scientific generalisation. For in any subject the facts are so numerous and many sided that they only lead us to a conclusion when they are marshalled by the light of some leading idea. A theory is, then, a necessity for the advance of science, and we may regard it as the branch of a living tree, of which facts are the nourishment. In the struggle between competing branches to reach the light some perish, and others form vigorous limbs. And as in a tree the shape of the young shoot can give us but little idea of the ultimate form of the branch, so theories become largely transformed in the course of their existence, and afford in their turn the parent stem for others.

The success of a theory may be measured by the extent to which it is capable of assimilating facts, and by the smallness of the change which it must undergo in the process. Every theory which is based on a true perception of facts is to some extent fertile in affording a nucleus for the aggregation of new observations. And a theory, apparently abandoned, has often ultimately appeared to contain an element of truth, which receives acknowledgment by the light of later views.

It will, I think, be useful to avail myself of the present occasion to direct your attention to a certain group of theories which are still in an undeveloped and somewhat discordant condition, but which must form the nucleus round which many observations have yet to be collected before these theories and their descendants can make a definitely accepted body of truth. If I am disposed to criticise some of them in their actual form, I shall not be understood as denying the great service which has been rendered to science by their formulation.

Great as have been the advances of geology during the present century, we have no precise knowledge of one of its fundamental units. The scale of time on which we must suppose geological history to be drawn is important not only for geology itself, but it has an intimate relation with some of the profoundest questions of biology, physics, and cosmogony.

We can hardly hope to obtain an accurate measure of time from pure geology, for the extent to which the events chronicled in strata were contemporaneous is not written in the strata themselves, and there are long intervals of time of which no record has been preserved.

An important step has been taken by Alfred Tylor, Croll, and others, towards the determination of the rate of action of geological agents (Geikie, "Text-book of Geology," 1882, p. 442). From estimates of the amount of sediment carried down by rivers, it appears that it takes from 1000 to 6000 years to remove one foot of rock from the general surface of a river basin.

From a consideration of the denuding power of rivers, and a measurement of the thickness of stratified rock, Phillips has made an estimate of the period of time comprised in geological history, and finds that, from stratigraphical evidence alone, we may regard the antiquity of life on the earth as being possibly between 38 and 96 millions of years ("Life on the Earth," Rede Lecture, 1860, p. 119).

Now while we should perhaps be wrong to pay much attention to these figures, yet at least we gain some insight into the order of magnitude of the periods with which we have to deal, and we may feel confident that a million years is not an infinitesimal fraction of the whole of geological time.

It is hardly to be hoped, however, that we shall ever attain to any very accurate knowledge of the geological time scale from this kind of argument.

But there is another theory which is precise in its estimate, and which, if acceptable from other points of view, will furnish exactly what is requisite. Mr. Croll claims to prove that great changes of climate must be brought about by astronomical events of which the dates are known or ascertainable ("Climate and Time"). The perturbation of the planets causes a secular variability in the eccentricity of the earth's orbit, and we are able confidently to compute the eccentricity of many thousands of years forward and backward from to-day, although it appears that, in the opinion of Newcomb and Adams, no great reliance can be placed on the values deduced from the formulæ at dates so remote as those of which Mr. Croll speaks. According to Mr. Croll, when the eccentricity of the earth's orbit is at its maximum, that hemisphere which has its winter in aphelion would undergo a glacial period. Now, as the date of great eccentricity is ascertainable, this would explain the great Ice Age and give us its date.

The theory has met with a cordial acceptance on many sides, probably to a great extent from the charm of the complete answer it affords to one of the great riddles of geology.

Adequate criticism of Mr. Croll's views is a matter of great difficulty on account of the diversity of causes which are said to co-operate in the glaciation. In the case of an effect arising from a number of causes, each of which contributes its share, it is obvious that if the amount of each cause and of each effect is largely conjectural the uncertainty of the total result is by no means to be measured by the uncertainty of each item, but is enormously augmented. Without going far into details it may be said that these various concurrent causes result in one fundamental proposition with regard to climate, which must be regarded as the keystone of the whole argument. That proposition amounts to this—that climate is unstable.

Mr. Croll holds that the various causes of change of climate operate *inter se* in such a way as to augment their several efficiencies. Thus the trade-winds are driven by the difference of temperature between the frigid and torrid zones, and if from the astronomical cause the northern hemisphere becomes cooler the trade-winds on that hemisphere encroach on those of the other, and the part of the warm oceanic current which formerly flowed into the cold north zone, will be diverted into the southern hemisphere. Thus the cold of the northern hemisphere is augmented, and this in its turn displaces the trade-winds further, and this again acts on the ocean currents, and so on; and this is neither more nor less than instability.

But if climate be unstable, and if, from some of those temporary causes for which no reasons can as yet be assigned, there occurs a short period of cold, then surely some even infinitesimal portion of the second link in the chain of causation must exist; and this should proceed as in the first case to augment the departure from the original condition, and the climate must change.

In a matter so complex as the weather, it is at least possible that there should be instability when the cause of disturbance is astronomical, whilst there is stability in an ordinary sense. If this is so, it might be explained by the necessity for a prolonged alteration in the direction of prevailing winds in order to affect oceanic currents (Zöppritz, *Phil. Mag.* 1878).

However this may be, so remarkable a doctrine as the instability of climate must certainly be regarded with great suspicion, and we should require abundant proof before accepting it. Now there is one result of Mr. Croll's theory which should afford almost a crucial test of its acceptability. In consequence of the precession of the equinoxes the conditions producing glaciation in one hemisphere must be transferred to the other every 10,000 years. If there is good geological evidence that this has actually been the case, we should allow very great weight to the astronomical theory, notwithstanding the difficulties in its way. Mr. Croll has urged that there is such evidence, and this view has been recently strongly supported by M. Blytt (NATURE, July 8 and 15, 1886). Other geologists do not, however, seem convinced of the conclusiveness of the evidence.

Thus Mr. Wallace ("Island Life"), whilst admitting that there was some amelioration of climate from time to time during the last glacial period, cannot agree in the regular alternations of cold and warm demanded by Mr. Croll's theory. To meet this difficulty he proposes a modification. According to his view, large eccentricity in the earth's orbit will only produce glaciation when accompanied by favourable geographical conditions. And when extreme glaciation has once been established in the hemisphere which has its winter in aphelion, the glaciation will persist, with some diminution of intensity, when precession has brought round the perihelion to the winter. In this case, according to Wallace, glaciation will be simultaneous on both hemispheres.

Again, he contends that, if the geographical conditions are not favourable, astronomical causes alone are not competent to produce glaciation.

There is agreement between the two theories in admitting instability of climate at first, when glaciation is about to begin under the influence of great eccentricity of the orbit, but afterwards Wallace demands great stability of climate. Thus he maintains that there is great stability in extreme climates, either warm or cold, whilst there is instability in moderate climates. I cannot perceive that we have much reason from physical considerations for accepting these remarkable propositions, and the acceptance or rejection of them demands an accurate knowledge of the most nicely balanced actions, of which we have as yet barely an outline.

Ocean currents play a most prominent part in these theories, but at this moment our knowledge of the principal oceanic circulation, and of its annual variability, is very meagre. In the course of a few years we may expect a considerable accession to our knowledge, when the Meteorological Office shall have completed a work but just begun—viz. the analysis of ships' logs for some sixty years, for the purpose of laying down in charts the oceanic currents.

With regard to the great atmospheric currents even the general scheme is not yet known. Nearly thirty years ago Prof. James Thomson gave before this Association at Dublin an important suggestion on this point. As it has been passed over in complete silence ever since, the present seems to be a good opportunity of redirecting attention to it.

According to Halley's theory of atmospheric circulation, the hot air rises at the equator and floats north and south in two grand upper currents, and it then acquires a westward motion relatively to the earth's surface, in consequence of the earth's rotation. Also the cold air at the pole sinks and spreads out over the earth's surface in a southerly current, at first with a westerly tendency, because the air comes from the higher regions of the atmosphere, and afterwards due south, and then easterly, when it is left behind by the earth in its rotation.

Now Prof. Thomson remarks that this theory disagrees with fact in as far as that in our latitudes, the winds, though westerly, have a poleward tendency, instead of the reverse.

In the face of this discrepancy he maintains that "the great circulation already described does actually occur, but occurs subject to this modification, that a thin stratum of air on the surface of the earth in the latitudes higher than 30°—a stratum in which the inhabitants of those latitudes have their existence, and of which the movements constitute the observed winds of those latitudes—being, by friction and impulses on the surface of the earth, retarded with reference to the rapid whirl or vortex-motion from west to east of the great mass of air above it, tends to flow towards the pole, and actually does so flow to supply the partial void in the central parts of that vortex due to the centrifugal force of its revolution. Thus it appears that in the temperate latitudes there are three currents at different heights—that the uppermost moves towards the pole, and is part of a grand primary circulation between equatorial and polar regions; that the lowermost moves also towards the pole, but is only a thin stratum forming part of a secondary circulation; that the middle current moves from the pole, and constitutes the return current for both the preceding; and that all these three currents have a prevailing motion from west to east" (Brit. Assoc. *Report*, Dublin, 1857, pp. 38, 39).

Such, then, appears to be our present state of ignorance of these great terrestrial actions, and any speculations as to the precise effect of changes in the annual distribution of the sun's heat must be very hazardous until we know more precisely the nature of the thing changed.

When looking at the astronomical theory of geological climate as a whole, one cannot but admire the symmetry and beauty of the scheme, and nourish a hope that it may be true; but the mental satisfaction derived from our survey must not blind us to the doubts and difficulties with which it is surrounded.

And now let us turn to some other theories bearing on this important point of geological time.

Amongst the many transcendent services rendered to science by Sir William Thomson, it is not the least that he has turned the searching light of the theory of energy on to the science of geology. Geologists have thus been taught that the truth must lie between the cataclysms of the old geologists and the uniformitarianism of forty years ago. It is now generally believed that we must look for a greater intensity of geologic action in the remote past, and that the duration of the geologic ages, however little we may be able mentally to grasp their greatness, must bear about the same relation to the numbers which were written down in the older treatises on geology, as the life of an ordinary man does to the age of Methuselah.

The arguments which Sir William Thomson has adduced in limitation of geological time are of three kinds. I shall refer first to that which has been called the argument from tidal friction; but before stating the argument itself it will be convenient to speak of the data on which the numerical results are based.

Since water is not frictionless, tidal oscillations must be subject to friction, and this is evidenced by the delay of twenty-four to thirty-six hours which is found to occur between full and change of moon and spring-tide. An inevitable result of this friction is that the diurnal rotation of the earth must be slowly retarded, and that we who accept the earth as our timekeeper must accuse the moon of a secular acceleration of her motion round the earth, which cannot be otherwise explained. It is generally admitted by astronomers that there actually is such an unexplained secular acceleration of the moon's mean motion.

No passage in Thomson and Tait's "Natural Philosophy" has excited more general interest than that in which Adams is quoted as showing that, *with a certain value for the secular acceleration*, the earth must in a century fall behind a perfect chronometer, set and rated at the beginning of the century, by twenty-two seconds. Unfortunately this passage in the first edition gave an erroneous complexion to Adams's opinion, and being quoted without a statement of the premises, has been used in popular astronomy as an authority for establishing the statement that the earth is actually a false timekeeper to the precise amount specified.

In the second edition (in the editing of which I took part) this passage has been rewritten, and it is shown that Newcomb's estimate of the secular acceleration only gives about one third of the retardation of the earth's rotation, which resulted from Adams's value. The last sentence of the paragraph here runs as follows:—"It is proper to add that Adams lays but little stress on the actual numerical values which have been used in this computation, and is of opinion that the amount of tidal retardation of the earth's rotation is quite uncertain." Thus, in the

opinion of our great physical astronomer, a datum is still wanting for the determination of a limit to geological time, according to Thomson's argument.

However, subject to this uncertainty, with the values used by Adams in his computation, and with the assumption that the rate of tidal friction has remained constant, then a thousand million years ago the earth was rotating twice as fast as at present. In the last edition of the "Natural Philosophy" the argument from these data runs thus:—

"If the consolidation of the earth took place then or earlier, the ellipticity of the upper layers (of the earth's mass) must have been $\frac{1}{230}$ instead of about $\frac{1}{300}$, as it is at present. It must necessarily remain uncertain whether the earth would from time to time adjust itself completely to a figure of equilibrium adapted to the rotation. But it is clear that a want of complete adjustment would leave traces in a preponderance of land in equatorial regions. The existence of large continents and the great effective rigidity of the earth's mass render it improbable that the adjustment, if any, to the appropriate figure of equilibrium would be complete. The fact, then, that the continents are arranged along meridians, rather than in an equatorial belt, affords some degree of proof that the consolidation of the earth took place at a time when the diurnal rotation differed but little from its present value. It is probable, therefore, that the date of consolidation is considerably more recent than a thousand million years ago."

I trust it may not be presumptuous in me to criticise the views of my great master, at whose intuitive perception of truth in physical questions I have often marvelled, but this passage does not even yet seem to me to allow a sufficiently large margin of uncertainty.

It will be observed that the argument reposes on our certainty that the earth possesses rigidity of such a kind as to prevent its accommodation to the figure and arrangement of density appropriate to its rotation. In an interesting discussion on subaerial denudation Croll has concluded that nearly one mile may have been worn off the equator during the past 12,000,000 years, if the rate of denudation all along the equator be equal to that of the basin of the Ganges ("Climate and Time," 1885, p. 336). Now, since the equatorial protuberance of the earth when the ellipticity is $\frac{1}{232}$ is fourteen miles greater than when it is $\frac{1}{300}$, it follows that 170,000,000 years would suffice to wear down the surface to the equilibrium figure. Now let these numbers be halved or largely reduced, and the conclusion remains that denudation would suffice to obliterate external evidence of some early excess of ellipticity.

If such external evidence be gone,[1] we must rely on the incompatibility of the known value of the precessional constant with an ellipticity of internal strata of equal density greater than that appropriate to the actual ellipticity of the surface. Might there not be a considerable excess of internal ellipticity without our being cognisant of the fact astronomically?

And, further, have we any right to feel so confident of the internal structure of the earth as to be able to allege that the earth would not through its whole mass adjust itself almost completely to the equilibrium figure?

Tresca has shown in his admirable memoirs on the flow of solids that when the stresses rise above a certain value the solid becomes plastic, and is brought into what he calls the state of fluidity. I do not know, however, that he determined at what stage the flow ceases when the stresses are gradually diminished. It seems probable, at least, that flow will continue with smaller stresses than were initially necessary to start it. But if this is so, then, when the earth has come to depart both internally and externally from the equilibrium condition, a flow of solid will set in, and will continue until a near approach to the equilibrium condition is attained.

When we consider the abundant geological evidence of the plasticity of rock, and of the repeated elevation and subsidence of large areas on the earth's surface, this view appears to me more probable than Sir William Thomson's.

On the whole, then, I can neither feel the cogency of the argument from tidal friction itself, nor, accepting it, can I place any reliance on the limits which it assigns to geological history.

[1] I find by a rough calculation that [illegible]ths of the land in the northern hemisphere are in the equatorial half of that hemisphere, viz. between 0° and 30° N. lat.; and that [illegible]ths of the land in the southern hemisphere are in the equatorial half of that hemisphere, viz. between 0° and 30° S. lat. Thus for the whole earth, [illegible]ths of all the land lie in the equatorial half of its surface, between 30° N. and S. lat. In this computation the Mediterranean, Caspian, and Black Seas are treated as land.

The second argument concerning geological time is derived from the secular cooling of the earth.

We know in round numbers the rate of increase of temperature, or temperature gradient, in borings and mines, and the conductivity of rock. These data enable us to compute how long ago the surface must have had the temperature of melting rock, and when it must have been too hot for vegetable and animal life.

Sir William Thomson, in his celebrated essay on this subject (republished in Thomson and Tait's "Natural Philosophy," Appendix D), concludes from this argument that "for the last 96,000,000 years the rate of increase of temperature underground has gradually diminished from about $\frac{1}{10}$th to about $\frac{1}{50}$th of a degree Fahrenheit per foot. . . . Is not this, on the whole, in harmony with geological evidence, rightly interpreted? Do not the vast masses of basalt, the general appearances of mountain-ranges, the violent distortions and fractures of strata, *the great prevalence of metamorphic action* (which must have taken place at depths of not many miles, if so much), all agree in demonstrating that the rate of increase of temperature downwards must have been much more rapid, and in rendering it probable that volcanic energy, earthquake shocks, and every kind of so-called plutonic action, have been, on the whole, more abundantly and violently operative in geological antiquity than in the present age?"

Now, while I entirely agree with the general conclusion of Sir William Thomson, it is not unimportant to indicate a possible flaw in the argument. This flaw will only be acknowledged as possible by those who agree with the previous criticism on the argument from tidal friction.

The present argument as to the date of the consolidation of the earth reposes on the hypothesis that the earth is simply a cooling globe, and there are reasons why this may not be the case. The solidification of the earth probably began from the middle and spread to the surface. Now is it not possible, if not probable, that, after a firm crust had been formed, the upper portion still retained some degree of viscosity? If the interior be viscous, some tidal oscillations must take place in it, and, these being subject to friction, heat must be generated in the viscous portion; moreover the diurnal rotation of the earth must be retarded. Some years ago, in a paper on the tides of a spheroid, viscous throughout the whole mass (*Phil. Trans.*, part ii. 1879), I estimated the amount and distribution of the heat generated whilst the planet's rotation is being retarded and the satellite's distance is being increased. It then appeared that on that hypothesis the distribution of the heat must be such that it would only be possible to attribute a very small part of the observed temperature gradient to such a cause. Now, with a more probable internal constitution for the earth in early times, the result might be very different. Suppose, in fact, that it is only those strata which are within some hundreds of miles of the surface which are viscous, whilst the central portion is rigid. Then, when tidal friction does its work the same amount of heat is generated as on the hypothesis of the viscosity of the whole planet, but instead of being distributed throughout the whole mass, and principally towards the middle, it is now to be found in the more superficial layers.

In my paper it is shown that with Thomson's data for the conductivity of rock and the temperature gradient, the annual loss of heat by the earth is 1/260,000,000 part of the earth's kinetic energy of rotation.

Also, if by tidal friction the day is reduced from D_0 hours to D hours, and the moon's distance augmented from Π_0 to Π earth's radii, the energy which has been converted into heat in the process is

$$\left(\frac{D}{D_0}\right)^2 - 1 - 8 \cdot 84 \left(\frac{1}{\Pi} - \frac{1}{\Pi_0}\right) \text{ times the earth's kinetic energy}$$

of rotation.

From these data it results that the heat generated in the lengthening of the day from twenty-three to twenty-four hours is equal to the amount of heat lost by the earth, at its present rate of loss, in 23,000,000 years.

Now if this amount of heat, or any sensible fraction of it, was actually generated within a few hundred miles of the earth's surface, the temperature gradient in the earth must be largely due to it, instead of to the primitive heat of the mass.

Such an hypothesis precludes the assumption that the earth is simply a cooling mass, and would greatly prolong the possible extension of geological time. It must be observed that this view is not acceptable unless we admit that the earth can adjust itself to the equilibrium figure adapted to its rotation.

It seems also worthy of suggestion that our data for the average gradient of temperature may be somewhat fallacious. Recent observations (*Challenger* Expedition) show that the lower stratum of the ocean is occupied by water at near the freezing temperature, whilst the mean annual temperature of the earth's surface, where the borings have been made, must be at least 30° higher. It does not then seem impossible that the mean temperature gradient for the whole earth should differ sensibly from the mean gradient in the borings already made.

The foregoing remarks have not been made with a view of showing Sir William Thomson's argument from the cooling of the earth to be erroneous, but rather to maintain the scientific justice of assigning limits of uncertainty at the very least as wide as those given by him. Prof. Tait ("Recent Advances in Physical Science," 1885) cuts the limit down to 10,000,000 years; he may be right, but the uncertainties of the case are far too great to justify us in accepting such a narrowing of the conclusion.

The third line of argument by which a superior limit is sought for the age of the solar system appears by far the strongest. This argument depends on the amount of radiant energy which can have been given out by the sun.

The amount of work done in the concentration of the sun from a condition of infinite dispersion may be computed with some accuracy, and we have at least a rough idea of the rate of the sun's radiation. From these data Sir William Thomson concludes (Thomson and Tait, "Natural Philosophy," Appendix E):—

"It seems, therefore, on the whole most probable that the sun has not illuminated the earth for 100,000,000 years, and almost certain that he has not done so for 500,000,000 years. As for the future, we may say, with equal certainty, that inhabitants of the earth cannot continue to enjoy the light and heat essential to their life for many million years longer unless sources now unknown to us are prepared in the great storehouse of creation."

This result is based on the value assigned by Pouillet and Herschel to the sun's radiation. Langley has recently made a fresh determination, which exceeds Pouillet's in the proportion of eight to five.[1] With Langley's value Thomson's estimate of time would have to be reduced by the factor five-eighths.

It has been suggested by Croll that the primitive solar nebula may have been hot. This heat must have arisen from the collision of two or more masses; if their relative velocity before collision was that due simply to their mutual attraction, the heat so generated is already counted in the heat generated by the concentration of the sun from a state of infinite dispersion. But if the relative velocity existed otherwise than from their mutual attraction, then the total heat in the sun might exceed that due simply to concentration. Sir William Thomson considers the hypothesis very improbable. The term improbability seems, however, almost to lose its meaning in these speculations, and at least we know by the spectroscope that actual nebulæ do consist of incandescent gases.

In considering these three arguments I have adduced some reasons against the validity of the first argument, and have endeavoured to show that there are elements of uncertainty surrounding the other two; nevertheless they undoubtedly constitute a contribution of the first importance to physical geology. Whilst then we may protest against the precision with which Prof. Tait seeks to deduce results from them, we are fully justified in following Sir William Thomson, who says that "the existing state of things on the earth, life on the earth, all geological history showing continuity of life, must be limited within some such period of past time as 100,000,000 years."

If I have carried you with me in this survey of theories bearing on geological time, you will agree that something has been acquired to our knowledge of the past, but that much more remains still to be determined.

Although speculations as to the future course of science are usually of little avail, yet it seems as likely that meteorology and geology will pass the word of command to cosmical physics as the converse.

At present our knowledge of a definite limit to geological time has so little precision that we should do wrong to summarily reject any theories which appear to demand longer periods of time than those which now appear allowable.

In each branch of science hypothesis forms the nucleus for the aggregation of observation, and as long as facts are assimilated and co ordinated we ought to follow our theory. Thus even if there be some inconsistencies with a neighbouring science we may be justified in still holding to a theory, in the hope that further knowledge may enable us to remove the difficulties. There is no criterion as to what degree of inconsistency should compel us to give up a theory, and it should be borne in mind that many views have been utterly condemned, when later knowledge has only shown us that we were in them only seeing the truth from another side.

[1] Langley (*Ann. Rep. R. A. S.* 1885) estimates that 3 calories per minute are received by a square centimetre at distance unity. This gives for the total annual radiation of the sun $4 \cdot 38 \times 10^{33}$ calories. Thomson gives as Pouillet's estimate 6×10^{3} times the heat required to raise 1 lb. of water 1° Cels., or $2 \cdot 72 \times 10^{33}$ calories.

SECTION B

CHEMICAL SCIENCE

Opening Address by William Crookes, F.R.S., V.P.C.S., President of the Section

A glance over the Presidential addresses delivered before this Section on former occasions will show that the occupiers of this chair have ranged over a fairly wide field. Some of my predecessors have given a general survey of the progress of chemical science during the past year; some, taking up a technological aspect of the subject, have discussed the bearings of chemistry upon our national industries; others, again, have passed in review the various institutions in this country for teaching chemistry; and in yet other cases the speaker has had the opportunity of bringing before the scientific world, for the first time, an account of some important original researches.

On this occasion I venture to ask your attention to a few thoughts on the very foundations of chemistry as a science—on the nature and the probable, or at least possible, origin of the so-called elements. If the views to which I have been led may at first glance appear heretical, I must remind you that in some respects they are shared more or less, as I shall subsequently show, by not a few of the most eminent authorities, and notably by one of my predecessors in this chair, Dr. J. H. Gladstone, F.R.S., to whose brilliant address, delivered in 1883, I must beg to refer you.

Should it not sometimes strike us, chemists of the present day, that after all we are in a position unpleasantly akin to that of our forerunners, the alchemists of the Middle Ages? These necromancers of a time long past did not, indeed, draw so sharp a line as do we between bodies simple and compound; yet their life-task was devoted to the formation of new combinations, and to the attempt to transmute bodies which we commonly consider as simple and ultimate—that is, the metals. In the department of synthesis they achieved very considerable successes; in the transmutation of metals their failure is a matter of history.

But what are we of this so-called nineteenth century doing in our laboratories and our libraries? Too many of us are content to acquire simply what others have already observed and discovered, with an eye directed mainly to medals, certificates, diplomas, and other honours recognised as the fruits of "passing." Others are seeking to turn the determined facts of chemistry to useful purposes; whilst a third class, sometimes not easily distinguished from the second, are daily educing novel organic compounds, or are racking their ingenuity to prepare artificially some product which Nature has hitherto furnished us through the instrumentality of plants and animals. The practical importance of such investigations, and their bearing on the industrial arts and on the purposes and needs of daily life, have been signally manifested during the last half-century.

Still a fourth class of inquirers working at the very confines of our knowledge find themselves, occasionally at least, face to face with a barrier which has hitherto proved impassable, but which must be overthrown, surmounted, or turned, if chemical science is ever to develop into a definite, an organised, unity. This barrier is nothing less than the chemical elements commonly so called, the bodies as yet undecomposed into anything simpler than themselves. There they extend before us, as stretched the wide Atlantic before the gaze of Columbus, mocking, taunting, and murmuring strange riddles which no man yet has been able to solve.

The first riddle, then, which we encounter in chemistry is, "What are the elements?" Of the attempts hitherto made to

define or explain an element, none satisfy the demands of the human intellect. The text-books tell us that an element is "a body which has not been decomposed"; that it is "a something to which we can add, but from which we can take away nothing," or, "a body which increases in weight with every chemical change." Such definitions are doubly unsatisfactory: they are provisional, and may cease to-morrow to be applicable in any given case. They take their stand, not on any attribute of the things to be defined, but on the limitations of human power; they are confessions of intellectual impotence.

Just as to Columbus long philosophic meditation led him to the fixed belief of the existence of a yet untrodden world beyond that waste of Atlantic waters, so to our most keen-eyed chemists, physicists, and philosophers a variety of phenomena suggest the conviction that the elements of ordinary assumption are not the ultimate boundary in this direction of the knowledge which man may hope to attain. Well do I remember, soon after I had obtained evidence of the distinct nature of thallium, that Faraday said to me—"To discover a new element is a very fine thing, but if you could decompose an element and tell us what it is made of—that would be a discovery indeed worth making." And this was no new speculation of Faraday's, for in one of his early lectures he remarked—"At present we begin to feel impatient, and to wish for a new state of chemical elements. For a time the desire was to add to the metals, now we wish to diminish their number. . . . To decompose the metals, then, to re-form them, to change them from one to another, and to realise the once absurd notion of transmutation, are the problems now given to the chemist for solution."

Mr. Herbert Spencer, in his hypothesis of the constitution of matter, says: "All material substances are divisible into so-called elementary substances composed of molecular particles of the same nature as themselves; but these molecular particles are complicated structures consisting of congregations of truly elementary atoms, identical in nature and differing only in position, arrangement, motion, &c., and the molecules or chemical atoms are produced from the true or physical atoms by processes of evolution under conditions which chemistry has not yet been able to reproduce."

Mr. Norman Lockyer has shown, I think on good evidence, that in the heavenly bodies of the highest temperature a large number of our reputed elements are dissociated, or, as it would perhaps be better to say, have never been formed. Mr. Lockyer holds that "the temperature of the sun and the electric arc is high enough to dissociate some of the so-called chemical elements, and give us a glimpse of the spectra of their bases"; and he likewise says that "a terrestrial element is an exceedingly complicated thing that is broken up into simpler things at the temperature of the sun, and some of these things exist in some sun-spots, while other constituents exist in others."

The late Sir Benjamin Brodie, in a lecture on "Ideal Chemistry" delivered before the Chemical Society in 1867, goes even further than this. He says:—"We may conceive that, in remote time or in remote space, there did exist formerly, or possibly do exist now, certain simpler forms of matter than we find on the surface of our globe—α, χ, ξ, ν, and so on. . . . We may consider that in remote ages the temperature of matter was much higher than it is now, and that these other things existed then in the state of perfect gases—separate existences—uncombined. . . . We may then conceive that the temperature began to fall, and these things to combine with one another and to enter into new forms of existence, appropriate to the circumstances in which they were placed. . . . We may further consider that, as the temperature went on falling, certain forms of matter became more permanent and more stable, to the exclusion of other forms. . . . We may conceive of this process of the lowering of the temperature going on, so that these substances, when once formed, could never be decomposed—in fact, that the resolution of these bodies into their component elements could never occur again. You would then have something of our present system of things. . . .

"Now this is not purely an imagination, for when we look upon the surface of our globe we have actual evidence of similar changes in Nature. . . . When we look at some of the facts which have been revealed to us by the extraordinary analyses which have been made of the matter of distant worlds and nebulæ, by means of the spectroscope, it does not seem incredible to me that there may even be evidence, some day, of the independent existence of such things as χ and ν."

In his Burnett Lectures "On Light as a Means of Investigation," Prof. Stokes, speaking of a line in the spectrum of the nebulæ, says:—"It may possibly indicate some form of matter more elementary than any we know on earth. There seems no *a priori* improbability in such a supposition so great as to lead us at once to reject it. Chemists have long speculated on the so-called elements, or many of them, being merely very stable compounds of elements of a higher order, or even perhaps of a single kind of matter."

In 1868 Graham wrote of Sir W. Thomson's vortex-ring theory as enlivening "matter into an individual existence and constituting it a distinct substance or element."

From these passages, which might easily be multiplied, it plainly appears that the notion—not necessarily of the decomposability, but at any rate of the complexity, of our supposed elements—is, so to speak, in the air of science, waiting to take a further and more definite development. It is important to keep before men's minds the idea of the genesis of the elements; this gives some form to our conceptions, and accustoms the mind to look for some physical production of atoms. It is still more important, too, to keep in view the great probability that there exist in Nature laboratories where atoms are formed, and laboratories where atoms cease to be. We are on the track and are not daunted, and fain would we enter the mysterious region which ignorance tickets "Unknown." It is for us to strive to unravel the secret composition even of the so-called elements—to undauntedly persevere—and "still bear up right onward."

If we adopt the easy-going assumption that the elements, whether self-existent or created, are absolutely and primordially distinct; that they existed as we now find them prior to the origin of stars and their attendant planets, constituting, in fact, the primal "fire-mist," we are little, if any, the wiser. We look at their number and at their distinctive properties, and we ask, Are all these points accidental or determinate? In other words, might there as well have been only 7, or 700, or 7000 absolutely distinct elements as the 70 (in round numbers) which we now commonly recognise? The number of the elements does not, indeed, commend itself to our reason from any *a priori* or extraneous considerations. Might their properties have conceivably differed from those which we actually observe? Are they formed by a "fortuitous concatenation," or do they constitute together a definite whole, in which each has its proper part to play, and from which none could be extruded without leaving a recognisable deficiency?

If their peculiarities were accidental, it would scarcely be possible for the elements to display those mutual relations which we find brought into such prominent light and order in the periodic classification of Newlands, Mendeléeff, and Meyer. Has not the relation between the atomic weights of the three halogens, chlorine, bromine, and iodine, and their serially varying properties, physical and chemical, been worn nearly threadbare? And the same with the calcium and the sulphur groups? Surely the probability of such relations existing among some seventy bodies which had come into fortuitous existence would prove to be vanishingly small!

We ask whether these elements may not have been evolved from some few antecedent forms of matter—or possibly from only one such—just as it is now held that all the innumerable variations of plants and animals have been developed from fewer and earlier forms of organic life? As Dr. Gladstone well puts it, they "have been built up from one another, according to some general plan." This building up, or evolution, is above all things not fortuitous: the variation and development which we recognise in the universe run along certain fixed lines which have been preconceived and foreordained. To the careless and hasty eye, design and evolution seem antagonistic; the more careful inquirer sees that evolution, steadily proceeding along an ascending scale of excellence, is the strongest argument in favour of a preconceived plan.

The array of the elements cannot fail to remind us of the general aspect of the organic world. In both cases we see certain groups well filled up, even crowded with forms having among themselves but little specific difference. On the other hand, in both, other forms stand widely isolated. Both display species that are common and species that are rare; both have groups widely distributed—it might be said cosmopolitan, and other groups of very restricted occurrence. Among animals I may mention as instances the Monotremata of Australia and New Guinea, and among the elements the metals of the so-called rare earths.

Now as these facts in the distribution of organic forms are generally considered by biological experts to rank among the weightiest evidences in favour of the origin of species by a process of evolution, it seems natural, in this case as in the other, to view existing elements not as primordial but as the gradual outcome of a process of development, possibly even of a "struggle for existence." Bodies not in harmony with the present general conditions have disappeared or perhaps have never existed. Others—the asteroids among the elements—have come into being and have survived, but only on a limited scale; whilst a third class are abundant because surrounding conditions have been favourable to their formation and preservation.

The analogy here suggested between elements and organisms is, indeed, not the closest, and must not be pushed too far. From the nature of the case there cannot occur in the elements a difference corresponding to the difference between living and fossil organic forms. The "great stone book" can tell us nothing of extinct elements. Nor would I for a moment suggest that any one of our present elements, however rare, is like a rare animal or plant in process of extinction; that any new element is in the course of formation, or that the properties of existing elements are gradually undergoing modification. All such changes must have been confined to that period so remote as not to be grasped by the imagination, when our earth, or rather the matter of which it consists, was in a state very different from its present condition. The epoch of elemental development is decidedly over, and I may observe that in the opinion of not a few biologists the epoch of organic development is verging upon its close.

Making, however, every allowance for these distinctions, if evolution be a cosmic law, manifest in heavenly bodies, in organic individuals, and in organic species, we shall in all probability recognise it, though under especial aspects, in those elements of which stars and organisms are in the last resort composed.

Is there then, in the first place, any direct evidence of the transmutation of any supposed "element" of our existing list into another, or of its resolution into anything simpler?

To this question I am obliged to reply in the negative.

I doubt whether any chemist here present could suggest a process which would hold out a reasonable prospect of dissociating any of our accepted simple bodies. The highest temperatures and the most powerful electric currents at our disposal have been tried, and tried in vain. At one time there seemed a possibility at least that the interesting researches of Prof. Victor Meyer might show the two higher members of the halogen group, bromine and iodine, as entering upon the path of dissociation. These hopes have not been fulfilled. It may be said, in the general opinion of the most eminent and judicious chemists, that none of the phenomena thus elicited prove that even an approach has been made to the object in view.

Even if we leave our artificial laboratories and seek an escape from the difficulty by observing the processes of the great laboratories of Nature, we feel no sufficiently firm ground.

We find ourselves thus driven to indirect evidence—to that which we may glean from the mutual relations of the elementary bodies. Such evidence of great value is by no means lacking, and to this I now beg to direct attention. First, we may consider the conclusion arrived at by Herschel, and pursued by Clerk-Maxwell, that atoms bear the impress of manufactured articles. Let us look a little more closely at this view. A manufactured article may well be supposed to involve a manufacturer. But it does something more: it implies certainly a raw material, and probably, though not certainly, the existence of by-products, residues, paraleipomena. What or where is here the raw material? Can we detect any form of matter which bears to the chemical elements a relation like that of a raw material to the finished product, like that, say, of coal-tar to alizarin? Or can we recognise any elementary bodies which seem like waste or refuse? Or are all the elements, according to the common view, co-equals? To these questions no direct answer is as yet forthcoming.

And this leads us up to an hypothesis which, if capable of full demonstration, would show us that the accepted elements are not co-equal, but have been formed by a process of expansion or evolution. I refer to the well-known hypothesis of Prout, which regards the atomic weights of the elements as multiples, by a series of whole numbers, of unity = the atomic weight of hydrogen. Every one is aware that the recent more accurate determinations of the atomic weights of different elements do not by any means bring them into close harmony with the values which Prout's law would require. Still, in no small number of cases the actual atomic weights approach so closely to those which the hypothesis demands, that we can scarcely regard the coincidence as accidental. Accordingly, not a few chemists of admitted eminence consider that we have here an expression of the truth, masked by some residual or collateral phenomena which we have not yet succeeded in eliminating.

The original calculations on which the most accurate numbers for the atomic weights are founded, have recently been recalculated by Mr. F. W. Clarke. In his concluding remarks, speaking of Prout's law, Mr. Clarke says that "none of the seeming exceptions are inexplicable. In short, admitting half-multiples as legitimate, it is more probable that the few apparent exceptions are due to undetected constant errors than that the great number of close agreements should be merely accidental. I began this re-calculation of the atomic weights with a strong prejudice against Prout's hypothesis, but the facts as they came before me have forced me to give it a very respectful consideration."

But if the evidence in favour of Prout's hypothesis in its original guise is deemed insufficient, may not Mr. Clarke's suggestion of half-multiples place it upon an entirely new basis? Suppose that the unit of the scale, the body whose atomic weight if multiplied by a series of whole numbers gives the atomic weights of the remaining elements, is not hydrogen, but some element of still lower atomic weight? We are here at once reminded of helium—an element purely hypothetical as far as our earth is concerned, but supposed by many authorities, on the faith of spectroscopic observations, to exist in the sun and in other stellar bodies. Most solar eruptions present merely the characteristic lines of hydrogen, C, F, and H, and along with them one particular line which at first was classed in the sodium group, but which is a little more refrangible, and is designated by the symbol D_3. According to Mr. Norman Lockyer and the late Father Secchi, this ray undergoes modifications not comparable to those affecting other rays of the chromosphere. In the corresponding region of the spectrum no dark ray has been observed. That the accompanying lines, C, F, and H, pertain to hydrogen is evident; and as D_3 has never been obtained in any other spectrum it is supposed to belong to a body foreign to our earth, though existing in abundance in the chromosphere of the sun. To this hypothetical body the name helium is assigned.

In an able memoir on this subject read before the Academy of Brussels, the Abbé E. Spée shows that, if helium exists, it enjoys two very remarkable properties. Its spectrum consists of a single ray, and its vapour possesses no absorbent power. The simple single ray, though I believe unexampled, is by no means an impossible phenomenon, and indicates a remarkable simplicity of molecular constitution. The non-absorbent property of its vapour seems to be a serious objection to a general physical law. Prof. Tyndall has demonstrated that the absorptive power increases with the complexity of molecular structure, and hence he draws the conclusion that the simpler the molecule the feebler the absorption. This conclusion the Abbé Spée regards as perfectly legitimate; but it neither explains nor even necessitates the absence of *all* absorptive power.

Granting that helium exists, all analogy points to its atomic weight being below that of hydrogen. Here, then, we may have the very element with atomic weight half that of hydrogen, required by Mr. Clarke as the basis of Prout's law.

But a more important piece of evidence for the compound nature of the chemical elements has yet to be considered. Many chemists must have been struck with certain peculiarities in the occurrence of the elements in the earth's crust; it is a stale remark that we do not find them evenly distributed throughout the globe. Nor are they associated in accordance with their specific gravities; the lighter elements placed on or near the surface, and the heavier ones following serially deeper and deeper. Neither can we trace any distinct relation between local climate and mineral distribution. And by no means can we say that elements are always or chiefly associated in Nature in the order of their so-called chemical affinities; those which have a strong tendency to form with each other definite chemical combinations being found together, whilst those which have little or no such tendency exist apart. We certainly find calcium as

carbonate and sulphate, sodium as chloride, silver and lead as sulphides; but why do we find certain groups of elements with little affinity for each other yet existing in juxtaposition or commixture? The members of some of these groups are far from plentiful, not generally or widely diffused, and certainly they are not easy to separate.

As instances of such grouping we may mention:—

(1) Nickel and cobalt, of which it may be said that had their compounds been colourless, they would have been long regarded as identical, and possibly even yet would not have been separated.

(2) The two groups of platinum metals.

(3) The so-called "rare earths," occurring in gadolinite, samarskite, &c., and evidently becoming more numerous the more closely they are examined.

Certain questions here suggest themselves:—Is the series of these elements like a staircase or like an inclined plane? Will they, the more closely they are scrutinised, be found to fade away the more gradually the one into the other? Further, will a mixture hitherto held to be simple, like (*e.g.*) didymium, be capable of being split up in one direction only, or in several? I have been led to ask this last question because I have separated from didymium bodies which seem to agree neither with the praseodymium and neodymium of Dr. Auer von Welsbach, nor with the components detected by M. de Boisbaudran and M. Demarçay.

Why, then, are these respective elements so closely associated? What agency has brought them together?

An eminent physicist evades the difficulty by suggesting that their joint occurrence is simply an instance of the working of the familiar principle, "Birds of a feather flock together." In their chemical and physical attributes these rare earths are so closely similar, that they may be regarded as substantially identical in all the circumstances of solution and precipitation to which they may have been exposed during geological ages.

But do we, in point of fact, recognise any such agency at work in Nature? Is there any power which regularly and systematically sorts out the different kinds of matter from promiscuous heaps, conveying like to like and separating unlike from unlike? I must confess that I fail to trace any such distributive agency, nor, indeed, do I feel able to form any distinct conception of its nature.

I must here remark that coral worms in some cases do effect a separation of certain kinds of matter. Thus a *Gorgonia* of the species of *Melithæa*, and *Mussa sinuosa*, undoubtedly eliminate from sea-water not merely lime, but even yttria; and other recent corals, *Pocillopora damicornis*, and a *Symphyllia* close to the yttria-secreting *Mussa*, separate samaria from sea-water. Sea-weeds and aquatic mollusks contain a larger proportion of iodide and bromine than the waters which they inhabit, and may thus be said to separate out these elements from the chlorine with which they are mingled.

But if we examine these cases of elimination we see that they are limited in their scope. They extend only to substances existing in solution of which there is a fresh supply always at hand, and which are capable of entering into the animal or vegetable economy. Again, the elimination of iodine and bromine, effected as just described, is of a very imperfect character, and, when such water-plants and animals die and decay, their constituents will be again distributed in the water.

We cannot well consider that nickel and cobalt have been deposited in admixture by organic agency, nor yet the groups iridium, osmium, and platinum—ruthenium, rhodium, and palladium.

Since the earthy metals to which I have referred—such as yttrium, samarium, holmium, erbium, thulium, ytterbium, &c.—are very rare, the probability of their ever having been brought together in some few uncommon minerals discovered only in a few localities must be regarded as trifling indeed, if we suppose that these metals had at any time been widely diffused in a state of great dilution with other matter. The features which we have just recognised in these earths seem to point to their formation severally from some common material placed in conditions in each case nearly identical. The case is strengthened by a consideration of the other groups of elements, also similar in properties, having little affinity for each other and occurring in admixture; either all or at least some of the elements concerned being moreover decidedly rare. Thus we have nickel and cobalt not plentiful or widely distributed; cobalt, perhaps, never found absolutely free from nickel, and *vice versâ*. We have also the two platinum groups, where very similar features prevail.

A weighty argument in favour of the compound nature of the elements is that drawn from a consideration of the compound radicals, or, as they might be called, pseudo-elements. Their similarity with the accepted elements is perfectly familiar to all chemists. If, for example, we suppose that in some age or in some country men of science were cognisant of the existence and of the behaviour of cyanogen, but had not succeeded in resolving it into its constituents, nothing, surely, would prevent their viewing it as an element, and assigning it a place with the halogens. It may fairly be held that if a body which we know to be compound can be found playing the part of an element, this fact lends a certain plausibility to the supposition that the elements also are not absolutely simple. This line of thought, or at least one closely approximating to it, was worked out by Dr. Carnelley in a paper read before this Association at its last meeting. From a comparison of the physical properties of inorganic with those of organic compounds, Dr. Carnelley concludes that "*the elements, as a whole, are analogous to the hydrocarbon radicals.*" This conclusion, if true, he adds, should lead to the further inference that the so-called elements are not truly elementary, being made up of at least two absolute elements, named provisionally A and B. Hence, he argues, it should be possible to build up a series of compounds of these two primary elements which would correspond to what we now call elements. Such an arrangement, to be admissible, would have to fulfil certain conditions:—The secondary elements thus generated from A and B must exhibit the phenomena of periodicity, and the series would have to form octaves: the entire system is bound to display some feature corresponding to the "odd and even series" of Mendeléeff's classification; the atomic weights must increase across the system from the first to the seventh group—that is, from the positive to the negative end of each series; the atomicity would have to increase from the first to the middle group, and then either increase or decrease to the seventh group; some feature should appear corresponding to the eighth group; and, lastly, the atomic weights in such a system ought to agree with the atomic weights as experimentally determined.

This last condition Dr. Carnelley rightly regards as the most crucial, and he finds his arrangement gives atomic weights which in a majority of instances coincide approximately with the actual atomic weights. Thus out of a total of sixty-one elements whose atomic weights have been determined with at least approximate accuracy, and whose places in the periodic system is not disputed, twenty-seven agree almost exactly with the actual numbers, whilst nineteen others are not more than one unit astray.

For a detailed consideration of the conclusions which follow from Dr. Carnelley's views I must refer to his paper as read at our last meeting. Two points bear more especially upon the subject now under consideration—that is, if this speculation on the genesis of the elements is well-founded. First, the existence of elements of identical atomic weights, isomeric with each other, would be possible; as such, Dr. Carnelley mentions respectively nickel and cobalt (now found to have slightly different atomic weights), rhodium and ruthenium, osmium and iridium, and the metals of some of the rare earths. Secondly, in Dr. Carnelley's scheme all the chemical elements save hydrogen are supposed to be composed of two simpler elements, $A = 12$ and $B = -2$. Of these he regards A as a tetrad identical with carbon, and B as a monad of negative weight—perhaps the ethereal fluid of space.

Dr. Carnelley's three primary elements therefore are carbon, hydrogen, and the ether.

Starting from the supposition that pristine matter was once in an intensely heated condition, and that it has reached its present state by a process of free cooling, Dr. E. J. Mills suggests that the elements, as we now have them, are the result of successive polymerisations. Dr. Mills reminds us that chemical substances in the process of cooling naturally increase in density, and, if such increase be measured as a function of time or of temperature, we sometimes observe that there are critical points corresponding to the formation of new and well-defined substances. In this manner ordinary phosphorus is converted into the red variety, I is transformed into I_2, S_2 becomes S_6, and NO_2 N_2O_4. Among organic bodies styrol, in like manner, according to Dr.

Mills, is converted into metastyrol, aldehyd into paraldehyd, the cyanates into cyanurates, and turpentine into metaterebenthene. At the critical points above referred to heat is liberated in especial abundance, and the bodies thus formed are known as polymers. If we could gradually cool down substances through a vast range of temperature, we should then probably discover a much greater number of such critical points, or points of multiple proportion, than we have been able to discover experimentally.

The heat given out in the act of polymerisation naturally reverses to some extent the polymerisation itself, and so causes a partial return to the previous condition of things. This forward and backward movement, several times repeated, constitutes "periodicity." Dr. Mills regards variable stars as instances, now in evidence, of the genesis of elementary bodies.

From a study of the classification of the elements, Dr. Mills is of opinion that the only known polymers of the primitive matter are arsenic, antimony, and perhaps erbium and osmium; whilst zirconium, ruthenium, samarium, and platinum approximate to the positions of other polymers. Hence, from this genetic view, these elements may be described as products of successive polymerisations.

I must now call attention to a method of illustrating the periodic law, proposed by my friend Prof. Emerson Reynolds, of the University of Dublin, which will here assist us. Prof. Reynolds points out that in each period the general properties of the elements vary from one to another with approximate regularity until we reach the seventh member, which is in more or less striking contrast with the first element of the same period, as well as with the first of the next. Thus chlorine, the seventh member of Mendeléeff's third period, contrasts sharply both with sodium, the first member of the same series, and with potassium, the first member of the next series, whilst, on the other hand, sodium and potassium are closely analogous. The six elements whose atomic weights intervene between sodium and potassium vary in properties, step by step, until chlorine, the contrast to sodium, is reached. But from chlorine to potassium, the analogue of sodium, there is a change in properties *per saltum*. Further, such alternations of gradual and abrupt transitions are observed as the atomic weights increase. If we thus recognise a contrast in properties—more or less decided—between the first and the last members of each series, we can scarcely help admitting the existence of a point of mean variation within each system. In general, the *fourth* element of each series possesses the properties we might expect a transition-element to exhibit. If we examine a particular period—for instance, that one whose meso-element is silicon, we note:—*First*, that the three elements of lower atomic weight than silicon, viz. sodium, magnesium, and aluminium, are distinctly *electro-positive* in character, while those of higher atomic weight, viz. phosphorus, sulphur, and chlorine, are as distinctly *electro-negative*. Throughout the best-known periods this remarkable subdivision is observable, although, as might be anticipated, the differences become less strongly marked as the atomic weights increase. *Secondly*, that the members above and below the meso-element fall into pairs of elements, which, while exhibiting certain analogies, are generally in more or less direct chemical contrast. Thus, in the silicon period we have—

$$\begin{array}{cc} \multicolumn{2}{c}{\mathrm{Si}^{\mathrm{iv}}} \\ +\mathrm{Al}''' & \mathrm{P}'''- \\ +\mathrm{Mg}'' & \mathrm{S}''- \\ +\mathrm{Na}' & \mathrm{Cl}'- \end{array}$$

This divison also happens, in many cases, to coincide with some characteristic valence of the contrasted elements. It is noteworthy, however, that the members on the electro-negative side exhibit the most marked tendency to vibration in atom-fixing power, so that valence alone is an untrustworthy guide to the probable position of an element in a period.

Thus for the purpose of graphic translation Prof. Reynolds considers that the fourth member of a period—silicon, for example—may be placed at the apex of a symmetrical curve, which shall represent, for that particular period, the direction in which the properties of the series of elements vary with rising atomic weights.

In the drawing before you (Fig. 1) I have modified Prof. Reynolds's diagram in one or two points. I have turned it the reverse way, as it is more convenient to start from the top and proceed downwards. I have represented the pendulous swing as gradually declining in amplitude, according to a mathematical law, and I have introduced another half-swing of the pendulum between cerium and lead, which not only renders the oscillations more symmetrical, but brings gold, mercury, thallium, lead, and bismuth on the side where they are in complete harmony with members of foregoing groups, instead of being out of harmony with them. This modification has another advantage, inasmuch as it leaves many gaps to be hereafter filled in with new elements just when the development of research is beginning to demand room for such expansion.

I do not, however, wish to infer that the gaps in Mendeléeff's table, and in this graphic representation of it, necessarily mean that there are elements actually existing to fill up the gaps; these gaps may only mean that at the birth of the elements there was an easy potentiality of the formation of an element which would fit into the place.

Following the curve from hydrogen downwards we find that the elements forming Mendeléeff's eighth group are to be found near three of the ten nodal points. These bodies are "interperiodic," both because their atomic weights exclude them from the small periods into which the other elements fall, and because their chemical relations with certain members of the adjacent periods show that they are probably interperiodic in the sense of being transitional.

This eighth group is divided into the three triplets—iron, nickel, and cobalt; rhodium, ruthenium, and palladium; iridium, osmium, and platinum. The members of each triplet have often been regarded as modifications of one single form of matter.

Notice how accurately the series of like bodies fits into this scheme. Beginning at the top, run the eye down analogous positions in each oscillation, taking either the electro-positive or electro-negative swings:—

N	Be	Li	Na	Mg	Al	Si	P	S	Cl	C
V	Ca	K	Cu	Zn	Ga	Ge	As	Se	Br	Ti
Nb	Sr	Rb	Ag	Cd	In	Sn	Sb	Te	I	Zr
—	Ba	Cs	—	—	—	—	—	—	—	—
Ta			Au	Hg	Tl	Pb	Bi			

Notice, also, how orderly the metals discovered by spectrum analysis fit in their places—gallium, indium, and thallium.

The symmetry of nearly all this series proclaims at once that we are working in the right direction. We can also learn much from the anomalies here visible. Look at the places marked with a circle: didymium, samarium, holmium, erbium, ytterbium, and thulium. Didymium cannot follow in order after the triad nitrogen, vanadium, columbium; nor erbium follow phosphorus, arsenic, antimony; nor thulium follow chlorine, bromine, iodine; nor ytterbium follow potassium, rubidium, cæsium. The inference to be drawn is that these bodies are out of place, owing to their atomic weights not having been correctly determined—an inference which is strengthened by the knowledge that the elementary character of some of these bodies is more than doubtful, whilst the chemical attributes of most of them are unknown.

The more I study the arrangement of this zigzag curve the more I am convinced that he who grasps the key will be permitted to unlock some of the deepest mysteries of creation. Let us imagine if it is possible to get a glimpse of a few of the secrets here hidden. Let us picture the very beginnings of time, before geological ages, before the earth was thrown off from the central nucleus of molten fluid, before even the sun himself had consolidated from the original *protyle*.[1] Let us still imagine that at this primal stage all was in an ultra-gaseous state, at a temperature inconceivably hotter[2] than anything now existing in the visible universe; so high, indeed, that the chemical atoms could not yet have been formed, being still far above their dissociation-point. In so far as *protyle* is capable of radiating or reflecting light, this vast sea of incandescent mist, to an astronomer in a distant star, might have appeared as a nebula, showing in the spectroscope a few isolated lines, forecasts of hydrogen, carbon, and nitrogen spectra.

[1] We require a word, analogous to protoplasm, to express the idea of the original primal matter existing before the evolution of the chemical elements. The word I have ventured to use for this purpose is compounded of προ (*earlier than*) and ὕλη (*the stuff of which things are made*). The word is scarcely a new coinage, for 600 years ago Roger Bacon wrote in his "De Arte Chymiæ":—"The elements are made out of ὕλη, and every element is converted into the nature of another element."

[2] I am constrained to use words expressive of high temperature; but I confess I am unable clearly to associate with *protyle* the idea of hot or cold. *Temperature, radiation*, and *free cooling* seem to require the periodic motions that take place in the chemical atoms; and the introduction of centres of periodic motion into *protyle* would constitute its being so far changed into chemical atoms.

But in course of time some process akin to cooling, probably internal, reduces the temperature of the cosmic *protyle* to a point at which the first step in granulation takes place; matter as we know it comes into existence, and atoms are formed. As soon as an atom is formed out of *protyle* it is a store of energy, potential (from its tendency to coalesce with other atoms by gravitation or chemically) and kinetic (from its internal motions). To obtain this energy, the neighbouring *protyle* must be refrigerated by it,[1] and thereby the subsequent formation of other atoms will be accelerated. But with atomic matter the various forms of energy which require matter to render them evident begin to act; and, amongst others, that form of energy which has for one of its factors what we now call *atomic weight*. Let us assume that the elementary *protyle* contains within itself the potentiality of every possible combining proportion or atomic weight. Let it be granted that the whole of our known elements were not at this epoch simultaneously created. The easiest formed element, the one most nearly allied to the *protyle*

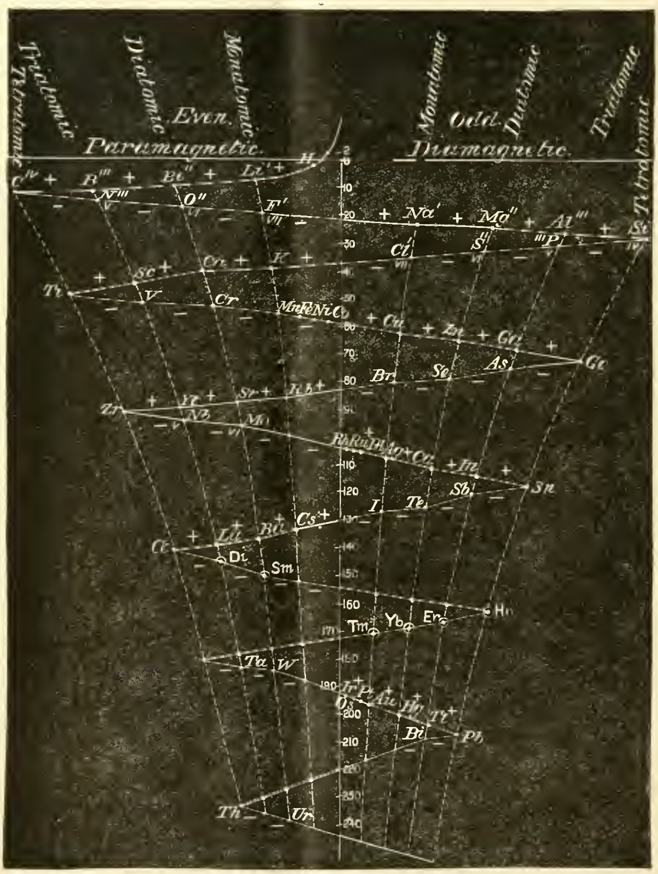


FIG. 1.

in simplicity, is first born. Hydrogen — or shall we say helium?—of all the known elements the one of simplest structure and lowest atomic weight, is the first to come into being. For some time hydrogen would be the only form of matter (as we now know it) in existence, and between hydrogen and the next formed element there would be a considerable gap in time, during the latter part of which the element next in order of simplicity would be slowly approaching its birth-point: pending this period we may suppose that the evolutionary process which soon was to determine the birth of a new element, would also determine its atomic weight, its affinities, and its chemical position.

In the original genesis, the longer the time occupied in that portion of the cooling down during which the hardening of the *protyle* into atoms took place the more sharply defined would be the resulting elements; and, on the other hand, with more irregularity in the original cooling we should have a nearer approach to the state of the elemental family such as we know it at present

[1] I am indebted to my friend, G. Johnstone Stoney, F.R.S., for the idea here put forward, as well as for other valuable suggestions and criticisms on some of the theoretical questions here treated of.

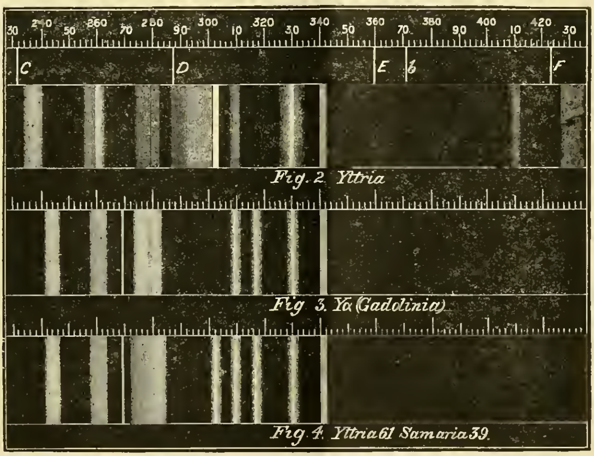
In this way it is conceivable that the succesion of events which gave us such groups as platinum, osmium, and iridium—palladium, ruthenium, and rhodium—iron, nickel, and cobalt, if the operation of genesis had been greatly more prolonged, would have resulted in the birth of only one element of these groups. It is also probable that, by a much more rapid rate of cooling, elements would originate even more closely related than are nickel and cobalt, and thus we should have formed the nearly allied elements of the cerium, yttrium, and similar groups; in fact, the minerals of the class of samarskite and gadolinite may be regarded as the cosmical lumber-room where the elements in a state of arrested development—the unconnected missing links of inorganic Darwinism—are finally aggregated.

I have said that the original *protyle* contained within itself the potentiality of all possible atomic weights. It may well be questioned whether there is an absolute uniformity in the mass of every ultimate atom of the same chemical element. Probably our atomic weights merely represent a mean value around which the actual atomic weights of the atoms vary within certain narrow limits.

Each well-defined element represents a platform of stability connected by ladders of unstable bodies. In the first accreting together of the primitive stuff the smallest atoms would form, then these would join together to form larger groups, the gulf across from one stage to another would be gradually bridged over, and the stable element appropriate to that stage would absorb, as it were, the unstable rungs of the ladder which led up to it. I conceive, therefore, that when we say the atomic weight of, for instance, calcium is 40, we really express the fact that, while the majority of calcium atoms have an actual atomic weight of 40, there are not a few which are represented by 39 or 41, a less number by 38 or 42, and so on. We are here reminded of Newton's "old worn particles."

Is it not possible, or even feasible, that these heavier and lighter atoms may have been in some cases subsequently sorted out by a process resembling chemical fractionation? This sorting out may have taken place in part while atomic matter was condensing from the primal state of intense ignition, but also it may have been partly effected in geological ages by successive solutions and re-precipitations of the various earths.

This may seem an audacious speculation, but I do not think it is beyond the power of chemistry to test its feasibility. An investigation on which I have been occupied for several years has yielded results which to me appear apposite to the question, and I therefore beg permission here to allude briefly to some of the



Figs. 2–4.

results, reserving details for a subsequent communication to the Section.

My work has been with the earths present in samarskite and gadolinite, separating them by systematic fractionation. Chemical fractionation, on which I hope to say more on another occasion, is very similar to the formation of a spectrum with a wide slit and a succession of shallow prisms. The centre portion remains unchanged for a long time, and the only approach to purity at first is at the two ends, while a considerable series of operations is needed to produce an appreciable change in the centre. The groups of didymium and yttrium earths are those which have chiefly occupied my attention. On comparing these rare earths we are at once struck with the close mutual similarity, verging almost into identity, of the members of the same group.

The phosphorescent spectra of these earths when their anhydrous sulphates are submitted to the induction discharge *in vacuo* are extremely complicated, and change in their details in a puzzling manner. For many years I have been persistently groping on in almost hopeless endeavour to get a clue to the meaning I felt convinced was locked up in these systems of bands and lines. It was impossible to divest myself of the conviction that I was looking at a series of autograph inscriptions from the molecular world, evidently of intense interest, but written in a strange and baffling tongue. All attempts to decipher the mysterious signs were, however, for a long time, fruitless. I required a Rosetta stone.

Down to a date comparatively recent nothing was more firmly fixed in my mind than the notion that yttria was the oxide of a simple body, and that its phosphorescent spectrum gave a definite system of coloured bands, such as you see in the drawing before you (Fig. 2). Broadly speaking, there is a deep red band, a red band, a very luminous citron-coloured band, a pair of greenish-blue bands, and a blue band. It is true these bands varied slightly in relative intensities and in sharpness with almost every sample of yttria I examined; but the general character of the spectrum remained unchanged, and I had got into the way of looking upon this spectrum as characteristic of yttria: all the bands being visible when the earth was present in quantity, whilst only the strongest band of all—the citron band—was visible when traces, such as millionths, were present. But that the whole system of bands spelled yttria and nothing but yttria I was firmly convinced.

During the later fractionations of the yttria earths, and the continued observations of their spectra, certain suspicions which had troubled me for some time assumed consistent form. The bands which hitherto I had thought belonged to yttria began to

vary in intensity among themselves, and continued fractionation increased the differences first observed. Whilst I was in this state of doubt and uncertainty, and only beginning to see my way towards arranging into a consistent whole the facts daily coming to light, help came from an unexpected quarter. M. de Marignac, with whom I had been for some time in correspondence, kindly sent me a small specimen of the earth which he had discovered and provisionally named $Y\alpha$. In the radiant-matter tube this earth gave a bright spectrum like the one in the diagram before you (Fig. 3). The spectrum above it (Fig. 2) is that ascribed to yttria. Look at the two. Omitting minor details, $Y\alpha$ is yttria with the chief characteristic band—the citron band—left out, and with the double green band of samaria added to it. Now look at Fig. 4, which represents the spectrum of a mixture of 61 parts of yttria and 39 parts of samaria. It is identical almost to its minutest detail with the spectrum of $Y\alpha$, with this not unimportant difference—the citron band is as prominent as any other line. $Y\alpha$ consists, therefore, of samaria with the greenish blue of yttria and some of the other yttria bands added to it.

I may aptly call the $Y\alpha$ spectrum my Rosetta stone. It threw a flood of light on all the obscurities and contradictions I had found so plentiful, and showed me that a much wider law than the one I had been working upon was the true law governing the occurrence of these obscure phenomena. For what does the spectrum of $Y\alpha$ show? It proves that what I had hitherto thought was one of the chief bands in the yttria spectrum—the citron band—could be entirely removed, whilst another characteristic group—the double green of yttria—could also be separated from the citron.

It would exceed legitimate limits were I to enter into details respecting the chemical and physical reasons which led me to these definite conclusions. To settle one single point more than 2000 fractionations have been performed.

The meaning of the strongly marked symbolic lines had first to be ascertained. For a long time I had to be content with roughly translating one group of coloured symbols as "yttrium" and another group as "samarium," disregarding the fainter lines, shadows, and wings frequently common to both. Constant practice in the decipherment has now given me fuller insight into what I may call the grammar of these hieroglyphic inscriptions. Every line and shadow of a line, each faint wing attached to a strong band, and every variation in intensity of the shadows and wings among themselves, now has a definite meaning which can be translated into the common symbolism of chemistry.

In a mineral containing the rarer earths, those most widely separated in chemical properties are most easily obtained in a state of comparative purity by simple chemical means. For instance, in separating didymium from lanthanum, or samarium from yttrium, a few simple chemical reactions and a little waste will give these bodies in a state of purity; but when it comes to splitting up yttrium into its components ordinary chemical separation is useless, and fractionation must be pushed to the utmost limit, many thousand operations and enormous waste of material being necessary to effect even a partial separation.

Returning therefore, after this explanatory digression, to the idea of heavy and light atoms, we see how well this hypothesis accords with the new facts here brought to light. From every chemical point of view the stable molecular group, yttrium, behaves as an element. Excessive and systematic fractionation has acted the part of a chemical "sorting Demon," distributing the atoms of yttrium into several groups, with certainly different phosphorescent spectra, and presumably different atomic weights, though all these groups behave alike from the usual chemical point of view. Here, then, is one of the elements the spectrum of which does not emanate equally from all its atoms, but some atoms furnish some, other atoms others, of the lines and bands of the compound spectrum of the element. And as this is the case with one element, it is probably so in a greater or less degree with all. Hence the atoms of this element differ probably in weight, certainly in the internal motions they undergo.

Another important inference which may be drawn from the facts is, that the atoms of which yttrium consists, though differing, do not differ continuously, but *per saltum*. We have evidence of this in the fact that the spectroscopic bands characteristic of each group are distinct from those of the other groups, and do not pass gradually into them. We must accordingly expect, in the present state of science, that this is probably the case with the other elements. And the atoms of a chemical element being known to differ in one respect may differ in other respects, and presumably do somewhat differ in mass.

Restricted by limited time and means, even a partial separation of these atomic groupings is possible to me only with enormous difficulty. Have we any evidence that Nature has effected such a separation? The following facts, I think, supply this evidence.

The earth yttria occurs in several minerals, all extremely rare. These minerals are of very diverse chemical composition, and occur in localities widely separated geographically. Does the pure yttria (*pure* in respect to every other known element) from these different sources behave differently to the radiant-matter test? To the chemist hitherto the earth yttria has been the same thing, and has possessed the same properties whatever its source; but armed with this new power of seeing into the atomic groupings which go to make up yttrium, we find evidence of differentiation between one yttrium and another.

Thus when the samarskite yttrium was formed, all the constituent atoms—deep red, red, orange, citron, greenish blue, and blue[1]—condensed together in fair proportion, the deep red being faintest. In gadolinite yttrium the citron and greenish blue constituents are plentiful, the red is very deficient, the orange is absent, and the others occur in moderate quantities. In the yttrium from xenotime the citron is most plentiful, the greenish blue occurs in smaller proportion, the red is all but absent, and the orange is quite absent. Yttrium from monazite contains the greenish blue and citron, with a fair proportion of the other constituents; the greenish blue is plentiful, and the red is good. Yttrium from fluocerite is very similar to that from monazite, but the blue is weaker. Yttrium from hielmite is very rich in citron, has a fair quantity of blue and greenish blue, less of red, no orange, and only a very faint trace of deep red. Yttria from euxenite is almost identical with that from hielmite.

This is unlikely to be an isolated case. The principle is very probably of general application to all the elements. In some, possibly in all elements, the whole spectrum does not emanate from all its atoms, but different spectral rays may come from different atoms, and in the spectrum as we see it all these partial spectra are present together. This being interpreted means that there are definite differences in the internal motions which go on in the several groups of which the atoms of a chemical element consist. For example, we must now be prepared for some such events as that the seven series of bands in the absorption-spectrum of iodine may prove not all to emanate from every molecule, but that some of these molecules emit some of these series, others others, and in the jumble of all these kinds of molecules, to which is given the name "iodine vapour," the whole seven series are contributors.

To me it appears the theory I have here ventured to formulate, taken in conjunction with the diagram in Fig. 1, may aid the scientific imagination to proceed a step or two further in the order of elemental evolution. In the undulating curve may be seen the action of two forces, one acting in the direction of the vertical line, and the other pulsating backwards and forwards like a pendulum. Assume the vertical line to represent temperature slowly sinking through an unknown number of degrees, from the dissociation point of the first-formed element down to the dissociation point of those last shown on the scale. But what form of energy is represented by the oscillating line? Swinging to and fro like a mighty pendulum to points equidistant from a neutral centre; the divergence from neutrality conferring atomicity of one, two, three, and four degrees as the distance from the centre is one, two, three, or four divisions; and the approach to, or retreat from, the neutral line deciding the electro-negative or electro-positive character of the element—all on the retreating half of the swing being positive and all on the approaching half negative—this oscillating force must be intimately connected with the imponderable matter, essence, or source of energy we call electricity.

Let us examine this a little more closely. Let us start at the moment when the first element came into existence. Before this time matter, as we know it, was not. It is equally impossible to conceive of matter without energy, as of energy without matter; from one point of view the two are convertible terms. Before the birth of atoms all those forms of energy which become evident when matter acts upon matter, could not have existed—they were locked up in the *protyle* as latent potentialities

[1] For brevity I call them by their dominant spectrum band.

only. Coincident with the creation of atoms all those attributes and properties which form the means of discriminating one chemical element from another start into existence fully endowed with energy.

The pendulum begins its swing from the electro-positive side; lithium, next to hydrogen in simplicity of atomic weight, is now formed; then glucinum, boron, and carbon. Definite quantities of electricity are bestowed on each element at the moment of birth, on these quantities its atomicity depends,[1] and the types of monatomic, diatomic, triatomic, and tetratomic elements are fixed. The electro-negative part of the swing now commences; nitrogen appears, and notice how curiously position governs the mean dominant atomicity. Nitrogen occupies the position below boron, a triatomic element, therefore nitrogen is triatomic. But nitrogen also follows carbon, a tetratomic body, and occupies the fifth position counting from the place of origin; how beautifully these opposing tendencies are harmonised by the endowment of nitrogen with at least a double atomicity, and making its atom capable of acting as tri- and pentatomic. With oxygen (di- and hexatomic) and fluorine (mon- and heptatomic) the same law holds, and one half-oscillation of the pendulum is completed. Again passing the neutral line the electro-positive elements, sodium (monatomic), magnesium (diatomic), aluminium (triatomic), and silicon (tetratomic) are successively formed, and the first complete oscillation of the pendulum is finished by the birth of the electro-negative elements phosphorus, sulphur, and chlorine; these three—like the corresponding elements formed on the opposite homeward swing—having each at least a double atomicity depending on position.

Let us pause at the end of the first complete vibration and examine the result. We have already formed the elements of water, ammonia, carbonic acid, the atmosphere, plant and animal life, phosphorus for the brain, salt for the sea, clay for the solid earth, two alkalies, an alkaline earth, an earth, together with their carbonates, borates, nitrates, fluorides, chlorides, sulphates, phosphates, and silicates, sufficient for a world and inhabitants not so very different from what we enjoy at the present day. True the human inhabitants would have to live in a state of more than Arcadian simplicity, and the absence of calcic phosphate would be awkward as far as bone is concerned. But what a happy world it would be! No silver or gold coinage, no iron for machinery, no platinum for chemists, no copper wire for telegraphy, no zinc for batteries, no mercury for pumps, and, alas! no rare earths to be separated.

The pendulum does not, however, stop at the end of the first complete vibration; it crosses the neutral point, and now the forces at work are in the same position as they were at the beginning. Had everything been as it was at first the next element again would have been lithium, and the original cycle would have recurred, repeating for ever the same elements. But the conditions are not quite the same; the form of energy represented by the vertical line has declined a little—the temperature has sunk—and not lithium, but the one next allied to it in the series comes into existence—potassium, which may be regarded as the lineal descendant of lithium, with the same hereditary tendencies, but with less molecular mobility and higher atomic weight.

Pass we rapidly along the to-and-fro curve, and in nearly every case the same law is seen to hold good. The last element of the first complete vibration is chlorine. In the corresponding place in the second vibration we do not have an exact repetition of chlorine, but the very similar body bromine; and when for a third time the position recurs we see iodine. I need not multiply examples.

In this far-reaching evolutionary scheme it could not come to pass that the potential elements would all be equal to one another. Some would be unable to resist the slightest disturbance of the unstable equilibrium in which they took their rise; others would endure longer, but would ultimately break down as temperature and pressure varied. Many degrees of stability would be here represented; not all the chemical elements are equally stable, and if we look with scrutinising eyes we shall still see our old friend the missing link, coarse enough to be detected by ordinary chemical processes, associated in the groups containing such elements as iron, nickel, and cobalt; palladium, ruthenium, and rhodium; iridium, osmium, and platinum. Whilst in their more subtile form these missing links present themselves as representatives of the differences which I have detected and described between the atoms of the same chemical element.

Dr. Carnelley has pointed out that "those elements belonging to the even series of Mendeléeff's classification are always paramagnetic, whereas the elements belonging to the odd series are always diamagnetic." On this curve the even series to the left, as far as can be ascertained, are paramagnetic, and, with a few exceptions, all to the right are diamagnetic. The very powerful magnetic metals, iron, nickel, cobalt, and manganese, occur close together on the proper side. The interperiodic groups, of which palladium and platinum are examples, are said to be feebly magnetic, and, if so, they form the exceptions. Oxygen, which weight for weight is more magnetic than iron, comes near the beginning of the curve, while the powerfully diamagnetic metals, bismuth and thallium, are at the opposite end of the curve.

On the odd, or diamagnetic, half of the swing the energy appears to have considerable regularity, whilst it is very irregular on the opposite side of the curve. Thus, between the extreme odd elements, silicon (28), germanium (73), tin (118), the missing element (163), and lead (208), there is a difference of exactly 45 units, conferring remarkable symmetry on one half of the curve. The differences on the even side are 36, 42, 51, 39, and 53 (giving the missing element between cerium and thorium an atomic weight of 180); these at first sight appear conformable to no law, but they become of great interest when it is seen that the mean difference of these figures is almost exactly the same as that on the other side of the curve—viz. 44·2.

This uniformity of difference—actual on the one side and average on the other—brings out the important inference that, whilst on the odd side there has been little or no variation in the vertical force, minor irregularities have been the rule on the even side. That is to say, the fall of temperature has been very uniform on the odd side—where every element formed during this half of the vibration is the representative of a strongly-marked group—sodium, magnesium, aluminium, silicium, phosphorus, sulphur, and chlorine; whilst on the even side of the swing the temperature has sunk with considerable fluctuations, which have prevented the formation here of any well-marked groups of elements, with the exception of those of which lithium and glucinum are the types.

If we can thus trace irregularities in the fall of temperature, can we also detect any variation in the force represented by the pendulous movement? I have assumed that this represents chemical energy. In the early-formed elements we have those in which chemical energy is at its maximum intensity, while, as we descend, affinities for oxygen are getting less and the chemism is becoming more and more sluggish. Part may be due to the lower temperature of generation not permitting such molecular mobility in the elements, but there can be little doubt that the chemism-forming energy, like the fires of the cosmical furnace, is itself dying out. I have endeavoured to represent this gradual fading out by a diminution of amplitude, the curve being traced from a photographic record of the diminution of the arc of vibration of a body swinging in a resisting medium.

When we look on a curve of this kind there is a tendency to ask, What is there above and below that portion which is seen? At the lower end of our curve what is there to be noted? We see a great hiatus between barium (137) and iridium (192·5), which it seems likely will be filled up by the so-called rare elements. Judging from my own researches, it is probable that many of these earthy elements will be found included in one or more interperiodic groups, whilst the higher members of the calcium, the potassium, the chlorine, and the sulphur groups, together with the elements between silver and gold, cadmium and mercury, indium and thallium, antimony and bismuth, are still waiting to be discovered. We now come to an oasis in the desert of blanks. Platinum, gold, mercury, thallium, lead, and bismuth, all familiar friends, form a close little group by themselves, and then after another desert space the list is closed with thorium (233) and uranium (240).

This oasis, and the blanks which precede and follow it, may

[1] "Nature presents us with a single definite quantity of electricity. . . . For each chemical bond which is ruptured within an electrolyte a certain quantity of electricity traverses the electrolyte, which is the same in all cases."—G. Johnstone Stoney, "On the Physical Units of Nature," British Association Meeting, 1874, Section A. *Phil. Mag.*, May 1881.

"The same definite quantity of either positive or negative electricity moves always with each univalent ion, or with every unit of affinity of a multivalent ion."—Helmholtz, Faraday Lecture, 1881.

"Every monad atom has associated with it a certain definite quantity of electricity; every dyad has twice this quantity associated with it; every triad three times as much, and so on."—O. Lodge, "On Electrolysis," "British Association Report," 1885.

be referred with much probability to the particular way in which our earth developed into a member of our solar system. If this be so it may be that on our earth only these blanks occur, and not generally throughout the universe.

What comes after uranium? I should consider that there is little prospect of the existence of an element much lower than this. Look at the vertical line of temperature slowly sinking from the upper to the lower part of the curve; the figures representing the scale of atomic weights may be also supposed to represent, inversely, the scale of a gigantic pyrometer dipping into the cauldron where suns and worlds are in process of formation. Our thermometer shows us that the heat has been sinking gradually, and, *pari passu*, the elements formed have increased in density and atomic weight. This cannot go on for an indefinite extent. Below the uranium point the temperature may be so reduced that some of the earlier formed elements which have the strongest affinities are able to enter into combination among themselves, and the result of the next fall in temperature will then be—instead of elements lower in the scale than uranium —the combination of oxygen with hydrogen, and the formation of those known compounds the dissociation of which is not beyond the powers of our terrestrial sources of heat.

Let us now turn to the upper portion of the scheme. With hydrogen of atomic weight = 1, there is little room for other elements, save perhaps for hypothetical helium. But what if we get "through the looking-glass," and cross the zero-line in search of new principles—what shall we find the other side of zero? Dr. Carnelley asks for an element of negative atomic weight; here is ample room and verge enough for a shadow series of such unsubstantialities. Helmholtz says that electricity is probably as atomic as matter:[1] is electricity one of the negative elements, and the luminiferous ether another? Matter, as we now know it, does not here exist; the forms of energy which are apparent in the motions of matter are as yet only latent possibilities. A substance of negative weight is not inconceivable.[2] But can we form a clear conception of a body which combines with other bodies in proportions expressible by negative quantities?

A genesis of the elements such as is here sketched out would not be confined to our little solar system, but would probably follow the same general sequence of events in every centre of energy now visible as a star.

Before the birth of atoms to gravitate towards one another, no pressure could be exercised; but at the outskirts of the fire-mist sphere, within which all is *protyle*—at the shell on which the tremendous forces involved in the birth of a chemical element exert full sway—the fierce heat would be accompanied by gravitation sufficient to keep the newly-born elements from flying off into space. As temperature increases expansion and molecular motion increase, molecules tend to fly asunder, and their chemical affinities become deadened; but the enormous pressure of the gravitation of the mass of atomic matter outside what I may for brevity call the birth-shell would counteract this action of heat.

Beyond this birth-shell would be a space in which no chemical action could take place, owing to the temperature there being above what is called the dissociation point for compounds. In this space the lion and the lamb would lie down together; phosphorus and oxygen would mix without union; hydrogen and chlorine would show no tendency to closer bonds; and even fluorine, that energetic gas which chemists have only isolated within the last month or two, would float about free and uncombined.

Outside this space of free atomic matter would be another shell, in which the formed chemical elements would have cooled down to the combination-point, and the sequence of events so graphically described by Mr. Mattieu Williams in "The Fuel of the Sun" would now take place, culminating in the solid earth and the commencement of geological time.

And now I must draw to a close, having exhausted not indeed my subject, but the time I may reasonably occupy. We have glanced at the difficulty of defining an element; we have noticed ton the revolt of many leading physicists and chemists against the ordinary acceptation of the term element. We have weighed the improbability of their eternal self-existence, or their origination by chance. As a remaining alternative we have suggested their origin by a process of evolution like that of the heavenly bodies according to Laplace, and the plants and animals of our globe according to Lamarck, Darwin, and Wallace. In the general array of the elements, as known to us, we have seen a striking approximation to that of the organic world. In lack of direct evidence of the decomposition of any element, we have sought and found indirect evidence. We have taken into consideration the light thrown on this subject by Prout's law, and by the researches of Mr. Lockyer in solar spectroscopy. We have reviewed the very important evidence drawn from the distribution and collocation of the elements in the crust of our earth. We have studied Dr. Carnelley's weighty argument in favour of the compound nature of the so-called elements from their analogy to the compound radicals. We have next glanced at the view of the genesis of the elements; and, lastly, we have reviewed a scheme of their origin suggested by Prof. Reynolds's method of illustrating the periodic classification.

Summing up all the above considerations we cannot, indeed, venture to assert positively that our so-called elements have been evolved from one primordial matter; but we may contend that the balance of evidence, I think, fairly weighs in favour of this speculation.

This, then, is the intricate question which I have striven to unfold before you, a question that I especially commend to the young generation of chemists, not only as the most interesting, but the most profoundly important, in the entire compass of our science.

I say deliberately and advisedly the *most interesting*. The doctrine of evolution, as you well know, has thrown a new light upon and given a new impetus to every department of biology, leading us, may we not hope, to anticipate a corresponding wakening light in the domain of chemistry?

I would ask investigators not necessarily either to accept or to reject the hypothesis of chemical evolution, but to treat it as a provisional hypothesis; to keep it in view in their researches, to inquire how far it lends itself to the interpretation of the phenomena observed, and to test experimentally every line of thought which points in this direction. Of the difficulties of this investigation none can be more fully aware than myself. I sincerely hope that this my imperfect attempt may lead some minds to enter upon the study of this fundamental chemical question, and to examine closely and in detail what I, as if amidst the clouds and mists of a far distance, have striven to point out.

[1] "If we accept the hypothesis that the elementary substances are composed of atoms, we cannot avoid concluding that electricity also, positive as well as negative, is divided into definite elementary portions, which behave like atoms of electricity."—HELMHOLTZ, Faraday Lecture, 1881.

[2] "I can easily conceive that there are plenty of bodies about us not subject to this intermutual action, and therefore not subject to the law of gravitation."—SIR GEORGE AIRY, "Faraday's Life and Letters," vol. ii. p. 54.

NOTES

A REUTER's telegram dated Grenada, August 29, states that during the solar eclipse of that morning good photometric observations were made by Prof. Thorpe. The light during the middle of totality was less than from the full moon. We learn from later telegrams dated Grenada, August 31, that the eclipse of the sun has been well observed by the British Astronomical Expedition, and that in the observations taken it was noticed that the corona extended nearly two diameters from the sun, and exhibited a feathery structure at the poles. Good photographs have been obtained of the coronal spectrum in the blue end. The spectrum was similar to that of the eclipse of 1883, observed on the Caroline Islands.

THE celebrations connected with the Chevreul centenary took place in Paris on Tuesday last. The first demonstration was that of the National Society of Agriculture, to which M. Chevreul was elected member forty-six years ago, and of which he is elected President every alternate year. A commemoration medal was struck by the same Society. At three o'clock M. Chevreul received the congratulations of the members of the Academy of Sciences. The principal ceremony of the day was the unveiling of the statue of M. Chevreul in the hall of the new Museum at the Jardin des Plantes. The walls of the room, which are of vast dimensions, were hung with red velvet, and

decorated with Gobelins tapestry and flowers. Spaces were reserved for Senators, Deputies, members of the Diplomatic Corps, and other distinguished persons. M. Goblet, Minister of Public Instruction, sat on M. Chevreul's right, and M. Floquet on his left. M. Frémy, the Director of the Museum, addressing M. Chevreul, said that the professors of the Museum, anticipating the future, had presented to him this statue. He then referred to the work of M. Chevreul in various branches of science, mainly in chemistry, and the results of his discoveries in industry. M. Zeller, President of the five Academies, M. Brock, representing the three Academies of Science of Scandinavian countries, M. Boscha, delegate of the Agricultural Society of Moscow, and the Italian Ambassador, also offered their congratulations to the venerable *savant*. The last speaker was the Minister of Public Instruction, who spoke, on behalf of the State, of the many ties which connected the great professor and chemist with the Ministry of Public Instruction. "The century which precedes our time," said M. Goblet, "belonged above all to science. What gave it its true character was the recent rise of scientific research, pursuing in the study of Nature the means of extending the domain of human power. Amongst the various branches of science, that to which M. Chevreul devoted himself—chemistry—is certainly one of the most fruitful, and one of those which owes most to French genius. Thanks to great French chemists and to M. Chevreul, France marches in the foremost rank of the nations which modern science guides." A banquet was given in the evening at the Hôtel de Ville. Several Ministers of State were present. The toast of M. Chevreul's health was proposed by M. Floquet, and supported by the Minister of Public Instruction. The festival which followed was very brilliant, ending with a torchlight procession, in which two squadrons of cuirassiers and a considerable body of infantry with several bands took part. Various Societies, all the members carrying Chinese lanterns, also joined the procession, which marched from the Hôtel de Ville through the principal streets and boulevards to the Place de la République. The streets through which the procession passed were thronged with dense masses of spectators.

On the 28th ult. His Excellency Tcheou Meou-Ki, Director of the Chinese Mission of Public Instruction, paid a visit, with the mandarins attached to his person, to M. Chevreul at his house. He handed to the illustrious chemist a Chinese document expressing in old characters every wish for his happiness and long life. It appears that there is living at this moment in China a Chinese *savant* who at the age of 100 years has just passed his examinations and been admitted a member of the highest academy of the mandarins. The interpreter explained to M. Chevreul that his Chinese visitors considered the fact that two *savants* a hundred years of age were living, one in France and the other in China, was a link connecting the learning of the two countries. When the Chinese Mission had retired, M. Chevreul received a deputation of the inhabitants of the Rue Chevreul, who presented him with a bouquet.

The last number of *La Nature* contains a biographical sketch, with several portraits, of M. Chevreul, from which it appears that he was born at Angers on August 31, 1786, his father being a physician and surgeon. It is noticeable that the father reached the age of ninety-one, and the mother died aged ninety-three. M. Chevreul as a lad witnessed some of the scenes of the conflict in La Vendée, and he saw the guillotine at work in Angers. The old University of the town having been swept away by the Revolution, he received his early education at the Central School of the place. He left this at the age of seventeen, and turned towards Paris, which was at that time peculiarly fortunate in possessing as teachers and professors eminent men in every branch of science. Fourcroy, the Professor of Chemistry at the College of France, was engaged in improving higher education, and left the work of his Chair largely to his demonstrator, Vauquelin, of whom Dumas said that he was wholly a chemist, a chemist every day of his life, and during the whole of each day. Chevreul entered under this teacher, and soon distinguished himself so much that he was allowed to take charge of the laboratory when twenty years of age. At the same time he taught at the Collège Charlemagne; four years later he was appointed Demonstrator to the Museum, and at thirty was appointed special Professor of Chemistry in charge of the dyeing department at the Gobelins. One of his earliest discoveries was that of margarine, oleine, and stearine in oils and fats. The last of these furnishes stearic acid, and thus an important industry like that in stearic candles was founded. His studies in fatty bodies, and his theory of saponification (1823) have not only created new industries, but they opened immense horizons in organic chemistry. Between 1828 and 1864 he studied colours, and from time to time published memoirs on the progress of his work. In 1826 he took his seat in the Academy of Sciences, and in 1830 he was appointed Director of the Museum of Natural History. His life now is spent between this institution, the Gobelins, and the Institute of France. He never fails to attend the Monday meetings of the Academy of Sciences. The number of his papers, memoirs, &c., is very great. Amongst them is one written in 1832, on the divining-rod, and another in 1853, in which he dissipated the mystery surrounding table-turning and similar manifestations. M. Chevreul remained in Paris during the siege of 1870-71, working steadily in his laboratory. It was soon after this that an expression in a letter he wrote to a friend led to the honourable title of "Doyen des étudiants de France" being affixed to his name. Although he possesses a large fortune, he still carries on his work at the institutions with which he is connected, and prosecutes his experiments with a juvenile lightness of touch. He is exceedingly temperate, drinking nothing but water or beer, but his longevity is not due to this; he owes it to a robust constitution and to a life wisely ordered, regular, and laborious. "It is," concludes M. Tissandier, "a great and beautiful sight presented by this centenarian, who, like an old oak, shelters under his shadow successive generations. Deaf to the sounds of this world, he has chosen to work alone in his laboratory, where his ever-wakeful intelligence is unceasingly attracted to the rays of eternal truth."

In reply to a question from Sir John Lubbock, Mr. Ritchie, the President of the Local Government Board, stated last week that considerable progress had been made by the Committee appointed last session to inquire and report on the subject of M. Pasteur's researches with reference to the prevention of hydrophobia. A sub-Committee visited Paris and had several long interviews with M. Pasteur, who explained to the members most fully and unreservedly the whole details of his treatment. The Committee examined a large number of the persons who had undergone treatment, and so far as this investigation is concerned, the Committee were fully satisfied that M. Pasteur's treatment had been effective. They hope before long to be in a position to report the results of their investigation to the Local Government Board, but before doing so they are anxious that time should be allowed for the Committee to ascertain that experiments on animals conducted on behalf of the Committee have yielded the same results as those detailed and demonstrated to them by M. Pasteur.

We regret to announce the death of Dr. James G. Wakley, editor of the *Lancet*, at his residence, Heathlands Park, Longcross, near Chertsey, on August 30. He was the youngest son of the late Thomas Wakley, founder of the *Lancet*, Member of

Parliament for Finsbury, and coroner for Middlesex. He became a member of the Royal College of Surgeons of England in 1849, and graduated Doctor of Medicine at King's College, Aberdeen, in 1852. At his father's death in 1862 he became editor of the *Lancet*, the duties of which position he discharged for nearly twenty-five years, continuing, in spite of much recent suffering, active in his work up to last Easter.

THE death is announced from Paris of M. Laguerre, Professor of Mathematical Physics in the College of France, and a distinguished mathematician, at the age of fifty-two. In his earlier years he was an officer in the artillery. He wrote and published in the *Proceedings* of various learned Societies numerous mathematical papers, but he never published a volume.

THE annual Pharmaceutical Congress commenced its sittings at the Mason College, Birmingham, on the 31st ult., under the presidency of Mr. Greenish, of London.

ON Friday, August 27, about midnight, an earthquake was felt all over the Levant, and as far to the west as Malta. At Alexandria the shock was felt fifteen minutes after midnight; its apparent direction was from west to east; at Athens a severe shock was felt about the same time. Its force was greatest and most destructive in Greece and the Ionian Islands. In the South-Western Peloponnesus, and particularly in the department of Messenia, towns and villages were destroyed. The towns of Filiatra and Gargaliano and Marathoupolis were laid in ruins, and Kyparassi and Choremi in Arcadia are similarly destroyed. The Eparch has, in consequence of the destruction of the houses, had to telegraph for tents for the people. Over 120 persons were killed, and a large number injured. The Greek Government has despatched four war-vessels with necessaries to relieve the inabitants. From Zante comes news that all the houses in Pyrgos have been destroyed. The shock was felt also in Zante, not a house having escaped damage, although no loss of life has occurred. Strong shocks were felt all over Greece. Some indications of the nature of the weather prior and subsequent to the shock are given by Reuter's Correspondent at Zante. For some time past extraordinary atmospherical disturbances, excessive heat, dead calms, and unusually high tides occurred. At 25 minutes past 11 on Friday night, after a day of heavy and threatening weather, the whole of Zante was racked with a most violent, but steady and undulating, earthquake lasting 15 seconds. The centre of the earthquake, the same correspondent says, was in the sea, 30 miles south-east of Zante, where it smashed the telegraph cable. After the shock the weather was threatening, indicating an approaching storm, and soon after a fearful storm burst over Corfu. Patras and the whole of the Ionian Islands suffered from the earthquake. The position of the centre of this shock is said to indicate some violent volcanic submarine agitation to the south of Zante. The weather after the earthquake remained menacingly heavy, and it was expected that other shocks would follow. At 11 o'clock the same night a shock was felt at Naples. At Brindisi an upheaval movement of the earth, lasting two minutes, was felt, followed by an undulatory movement of about the same duration. There were also two successive prolonged shocks at Foggia, and two undulatory movements at Caserta. At Taranto there were two very sharp shocks, one vertical, the other undulatory, causing great alarm to the inhabitants, who passed the night in the fields for safety. There was however no loss of life nor any serious damage at the places visited by the seismic disturbance. The Governor of Malta also telegraphs to the Colonial Office that a severe earthquake visited the island at 11 o'clock the previous night, causing much alarm, but no serious injury to buildings. No lives were lost. It appears that the correspondent in Alexandria who states that the earthquake appeared to travel from west to east is correct. The wave was first felt at Malta about 11, a little later in Naples (very slight), and in various parts of Calabria, then about midnight in the Ionian Islands, then in Greece, and at fifteen minutes past midnight in Alexandria.

THE Premier of New South Wales has laid before Parliament the proposals of his Government for the celebration of the centenary of that colony in 1888. Amongst others, the New South Wales Government propose inviting the members of the British Association to hold their annual meeting for that year in Sydney, and the Premier stated that he had already communicated with the Association on the importance of the visit. Great stress was laid on the fact that the invitations would be extended to all British Universities, literary, scientific, and art Societies.

WE have received parts 1 and 2 of vol. ix. of the *Transactions* of the Seismological Society of Japan. Prof. Knott discusses the well-worn subject of earthquake frequency, but in a wholly new and original way. His object is to determine the effect of the various causes which are said to influence earthquakes, and his conclusion is that the annual periodicity in earthquake frequency, when it does exist, finds a possible explanation in the annual periodicity of two well-known meteorological phenomena—namely, snow accumulations over continental areas and barometric gradients. No other cause, he says, that can be imagined or named fulfils all the conditions. Mr. Shida describes an automatic current recorder of his own invention. The resolution of the International Electric Congress held in Paris in 1884, that observations of earth-currents should be pursued in all countries was communicated to various Governments, to that of Japan amongst the number. Mr. Shida is Chief Engineer to the Japanese Telegraph Department, and it devolved on him to take the subject up. To this fact, doubtless, we also owe the third paper, which is by the same gentleman, on earth-currents. He describes briefly the work that has been done in the subject in the past, what is being done, and what might be done in Japan. From an examination of the magnetic observations made at the Meteorological Department of Tokio it appears that the declination variations are not the effect of earth-currents, for, if this were so, then an increase of the western declination ought to correspond to a decrease of earth-current flowing from north to south, not an increase, as has been found by actual observation. The results show that both magnetic and earth-current variations are regulated by the same cause or causes, and that the sun plays an important part in producing the effects which are observed. The fact, however, that there appear to be two maxima and two minima in these variations tends to show that they are in part due to the action of the moon. The study of earth-currents, says Mr. Shida, has not advanced *pari passu* with other branches of seismological science, and he desires to establish a system of observation of the currents, and to devise and improve methods of observing. The second part of the *Transactions* contains an elaborate and exhaustive paper (it fills nearly 200 printed octavo pages), by Prof. Milne, on the volcanoes of Japan. It represents the labour and collections of about ten years, and is deserving of special detailed notice. We are glad to observe, from a notice sent with the numbers, that the Japanese *Transactions* of the Society (*i.e.* those written or translated into Japanese) have now reached their third volume. Taken all in all, this Society is by far the most active and thriving of all the learned Societies of the Far East, and we have little doubt that it owes a great part of its vitality to the fact that Japanese men of science are pursuing the work inaugurated by Prof. Milne with the enterprise and ardour of their race.

PRINCE PUTIATIN has presented to the Russian Archæological Society a stone slab which was recently found in the course of some excavations at the Bologne station on the St. Petersburg and Moscow Railway, along with some stone weapons and

utensils. A representation of the constellation of the Great Bear was, although rudely, carefully drawn on the slab. It may be remembered that some years ago a similar slab was found near Weimar.

THE additions to the Zoological Society's Gardens during the past week include a White-fronted Capuchin (*Cebus albifrons*) from Central America, presented by Mr. H. A. Blake; a Mississippi Alligator (*Alligator mississippiensis*) from Florida, presented by Miss Janet D. White; a Common Gannet (*Sula bassana*), British, presented by Mr. F. E. Hatfield; two Dominican Kestrels (*Tinnunculus dominicensis*), two Green Bitterns (*Bictorides virescens*), from West India, presented by Dr. A. Boon, F.R.C.S.; a Raven (*Corvus corax*), British, presented by Mr. Robert Galland; a Ring-tailed Coati (*Nasua rufa*), a Globose Curassow (*Crax globicera*), from Central America, a Clouded Iguana (*Cyclura carinata*), from Cuba, deposited; a Black-necked Swan (*Cygnus nigricollis* ♀) from Chili, purchased; a Leopard (*Felis pardus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

NOTES ON VARIABLE STARS.—Mr. T. E. Espin, Observer to the Liverpool Astronomical Society, announces in the Society's *Circular*, No. 6, that the star D.M. + 8°·3780, stated in *Circular* No. 2 (NATURE, vol. xxxiv. p. 110) to be probably variable, passed its maximum about June 4, at which time its magnitude was 6·8. Since this date it has diminished in brightness, and on August 20 it was only 8·2. The star would seem to be a long-period variable. Its place for 1885 is R.A. 18h. 32m. 51s., Decl. + 8° 43′·5. Mr. Espin also states that he has detected variation in the red star D.M. + 47°·3031, which passed a minimum about the end of April. On May 14 it was only 8·9, since which it has increased, and is now 7·7. This star also is a long-period variable. Its place for 1885 is R.A. 20h. 5m. 58s., Decl. + 47° 28′·9. It precedes 32 Cygni by 5m. 57s., and is 9′ north of it.

Circular No. 7 states that—(1) The observations of 10 Sagittæ on sixty nights since 1885 November 28 give: Period = 8·32134d., Epoch of Max. 1886 July 17·56d. A minimum occurs on 1886 September 1·6d., and a maximum on 1886 September 5·5d. (2) The star D.M. + 17°·3940 was observed as 9·5 on April 26 last. From this date it increased, and on June 13 it was 8·3. Latterly it has diminished, and on August 20 it was 8·7. Vogel gives the spectrum as III*b*. ! Dunér as III*a*. !! The star's place for 1885 is R.A. 19h. 16m. 33s., Decl. + 17° 26′·4.

WINNECKE'S COMET.—From the *Dun Echt Circular*, No. 124, we learn that this periodical comet has been found at Cape Town. It is described as circular, less than 1′ in diameter, as bright as a star of the 10th magnitude, and as having some central condensation but no tail. Its observed place was Greenwich M.T. Aug. 20, 5h. 47m. 54s., R.A. 13h. 10m. 21·5s., Decl. 1° 8′ 17″ S. The daily motion, according to Lamp's ephemerides (*Astronomische Nachrichten*, No 2731) is about *plus* 3·3m. and 32′ *south*.

THE OBSERVATORY OF YALE COLLEGE.—The report of the work done at this Observatory during the year ending June 1, 1886, has recently been issued. The chief astronomical work is that done with the heliometer in charge of Dr. W. L. Elkin. With this instrument considerable progress has been made with the triangulation of the Pleiades, completing the series obtained in the previous year. All the stars have now been observed on from ten to twelve nights, and a total of over 1600 measures of distance and 700 of position-angle are available for discussion. The principal observing work accomplished by Dr. Elkin, has, however, been in connection with the scheme for determining the average parallax of the first-magnitude stars as a step towards the more comprehensive plan proposed by Gill and Elkin in concert. It is proposed at present to take the ten brightest stars in the northern hemisphere and observe them each from sixteen to twenty times at epochs of maximum parallactic displacement, using a favourably situated pair of comparison stars—in some cases a double pair, or four stars. Arcturus, with its large proper motion, presents an object of especial interest, and it has been taken up in a more exhaustive manner with six pairs of comparison stars, five of which have been successfully followed up so far. The whole work is progressing satisfactorily, over 200 sets of measures having been made, and is rather more than half completed, the working plan extending until February 1887. Astronomers will await with interest the completion and publication of Dr. Elkin's important researches.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 SEPTEMBER 5-11

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on September 5

Sun rises, 5h. 21m.; souths, 11h. 58m. 34·8s.; sets, 18h. 36m.; decl. on meridian, 6° 45′ N.: Sidereal Time at Sunset, 17h. 35m.

Moon (at First Quarter) rises, 13h. 38m.; souths, 18h. 11m.; sets, 22h. 41m.; decl. on meridian, 17° 44′ S.

Planet	Rises	Souths	Sets	Decl. on meridian
	h. m.	h. m.	h. m.	° ′
Mercury ...	3 38 ...	10 53 ...	18 8 ...	13 44 N.
Venus	3 8 ...	10 34 ...	18 0 ...	15 33 N.
Mars	10 44 ...	15 28 ...	20 12 ...	15 15 S.
Jupiter... ...	7 46 ...	13 37 ...	19 28 ...	2 34 S.
Saturn	0 23 ...	8 27 ...	16 31 ...	21 42 N.

Occultations of Stars by the Moon (visible at Greenwich)

Sept.	Star	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image
			h. m.	h. m.	° °
7 ...	B.A.C. 6536 ...	6½ ...	21 43 ...	22 33 .	65 353
10 ...	B.A.C. 7487 ...	6½ ...	20 11 ...	21 12 ...	129 234

Sept.	h.	
7 ...	5 ...	Mercury at least distance from the Sun.

Variable Stars

Star	R.A.	Decl.			
	h. m.	° ′		h. m.	
U Cephei	0 52·2 ...	81 16 N. ...	Sept. 6,	20 6	*m*
			,, 11,	19 46	*m*
Algol	3 0·8 ...	40 31 N. ...	,, 9,	2 0	*m*
			,, 11,	22 49	*m*
ζ Geminorum ...	6 57·4 ...	20 44 N. ...	,, 8,	19 24	*m*
δ Libræ	14 54·9 ...	8 4 S. ...	,, 7,	3 3	*m*
U Ophiuchi	17 10·8 ...	1 20 N. ...	,, 7,	2 8	*m*
				22 16	*m*
T Herculis	18 4·8 ...	31 0 N. ...	,, 5,		*M*
U Sagittarii	18 25·2 ...	19 12 S. ...	,, 8,	0 0	*m*
R Scuti	18 41·4 ...	5 50 N. ...	,, 6,		*m*
β Lyræ...	18 45·9 ...	33 14 N. ...	,, 6,	0 0	*M*
η Aquilæ	19 46·7 ...	0 43 N. ...	,, 6,	0 0	*M*
			,, 10,	19 0	*m*
R Delphini	20 9·4 ...	8 45 N. ...	,, 7,		*M*
T Aquarii	20 43·9 ...	5 34 S. ...	,, 6,		*m*
S Pegasi	23 14·8 ...	8 18 N. ...	,, 5,		*M*

M signifies maximum; *m* minimum.

Meteor Showers

Meteors have been observed at this season from the following radiants:—Near *o* Eridani, R.A. 55°, Decl. 6° S.; from Camelopardus, R.A. 60°, Decl. 60° N.; from near μ Persei, R.A. 65°, Decl. 46° N.; from near μ Tauri, R.A. 65°, Decl. 6° N.; and near α Pegasi, R.A. 345°, Decl. 13° N.

SCIENTIFIC SERIALS

Journal de Physique, July.—Prof. Mascart, on magnetisation. A study of the secondary effect produced by the reaction of the polar surfaces on the magnetising field. The author calculates also the influence of the earth's magnetism in producing temporary alteration in the magnetisation of a needle during oscillation, an effect which he finds to have been often exaggerated, and not to exceed 1/1000 part of the whole magnetisation.—P. Duhem, on the calorific capacity of dissociable gaseous combinations. Discussion and expansion of the formulæ

of Willard Gibbs to explain the variations of specific heat of such bodies as nitric oxide and acetic acid. The results confirm the idea that such variations are due to the gradual dissociation of polymeric forms.—G. Lippmann, an absolute spherical electrometer. Two hollow metal hemispheres, one fixed, the other held by a trifilar suspension, when similarly electrified, repel one another, with a force actually proportional to the square of the potential. The displacements are read optically.—MM. Bichat and Blondlot, on an absolute electrometer with continuous indication. This is an apparatus of three concentric cylinders, the innermost of which is suspended from a balance. The theory of it is already known.—P. Janet, on the formula of Van der Waals, and its application to capillary phenomena.—F. and W. Kohlrausch, the electro-chemical equivalent of silver (abstracted from *Wiedemann's Annalen*).

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, August 23.—M. Fizeau in the chair.—Elliptical elements of Brooks's Comet III. 1886, by Mr. J. R. Hind. The elliptical orbit, deduced from the observations made at Nice on May 25 and July 1, and at Algiers on June 3, is as under:—

T = 1886 June 6·57145 Greenwich Mean Time

$$\left.\begin{array}{lr} \pi \;\ldots\;\ldots & 229^\circ\ 45'\ 58''\!\cdot\!0 \\ \Omega \;\ldots\;\ldots & 53\ \ 3\ 25\cdot7 \\ \iota \;\ldots\;\ldots & 12\ \ 56\ \ 1\cdot8 \end{array}\right\}\text{Mean Equinox 1886·0}$$

$$\begin{array}{ll} \phi \;\ldots\;\ldots & 37\ \ 27\ \ 10\cdot2 \\ \log a \;\ldots & 0\cdot5329478 \\ \mu \;\ldots\;\ldots & 563''\cdot0992 \\ \text{Period} \;\ldots & 6^{a}\cdot301 \end{array}$$

—On the measurement of very strong pressures and on the compressibility of fluids, by M. E. H. Amagat. For the measurement of very high pressures the author has adopted the principle of the manometer with differential pistons. In order to obtain accurate results, the condition had to be realised of maintaining the pistons in complete action while keeping them perfectly air-tight. The reading of the volumes of compressed fluid was effected by the process already indicated by Prof. Tait, of Edinburgh. Water and ether have been studied at zero and at the two respective temperatures of 20° and 40° C. Respecting the variation with pressure, it is shown that the coefficient diminishes gradually with the increase of pressure, and this takes place throughout the whole scale of pressures, contrary to the opinion of some physicists. At 3000 atmospheres the volume of water was reduced one-tenth, and its coefficient of compressibility one-half. This coefficient between 2590 and 2981 atm. was 0·0000238, and that of ether between 1623 and 2002 atm. 0·000045. The study of ether will be continued and pushed to 3000 atm., and in a future communication will be given the coefficients of compressibility and of dilatation for several other fluids up to 3000 atm. A number of gases will then be examined with the same apparatus and within the same limits of pressure.—On the purple of the solar spectrum, by M. Camille Koechlin. The solar spectrum yields only two simple colours, blue and yellow. The third is blended with yellow and blue to constitute the reds on the one hand, the violet, on the other, purple being red deprived of yellow or violet deprived of blue, or simply the spectrum without yellow or blue. If on the red of one be projected the blue of another spectrums or on the violet of the first the yellow of the second, the result is purple. The red or the violet may again be restored by applying to the purple the yellow or blue of a third spectrum. And if these applications be made with reversed prisms, so that the complementary colours reciprocally cover each other, the spectrum will present at both extremities a purple region with yellowish-white interval. Purple, being a simple colour, will thus never be obtained by mixture, but only by extracting the yellow from a red or the blue from a violet. The solar spectrum contains the elements of all shades, either by mixtures or by diluting with white or extinction with black. In the latter case the colours containing blue preserve their tint, while those on the opposite side of the yellow become changed in character. Thus green, blue, and violet yield the so-called deep greens, blues, and violets, while the yellow, orange, red, and purple cannot be intensified, but pass over to olive, brown, garnet, or amaranth.—On the branchial apparatus and muscular and nervous systems of *Amaræcium torquatum*, by M. Charles Maurice. In this Compound Ascidian, which abounds at Villefranche-sur-Mer, the branchial apparatus presents thirteen rows of stigmata, and is otherwise characterised by three fundamental peculiarities connected with the transverse sinus.—On a larva of *Lampyris noctiluca* surviving the loss of its head, by M. François. This specimen, which had lost the whole of the cephalic region, was found in a perfectly healthy and normal condition, and although destitute of any buccal orifice, it showed on dissection an abundance of adipose tissue. The œsophagus, however, had changed its position, and contained no trace of alimentary matter.—On the cyclone that swept over the Gulf of Aden in June 1885, by Admiral Cloué. As supplementary to the previous statement on this subject, the writer has collected further details from the captains of some English and Dutch vessels overtaken by the storm, and from Obock regarding the caravan which was *en route* for Shoa when the whirlpool swept by.—Remarks on Dr. W. C. Gore's memoir on the "Projectiles of the Future," presented to the Academy, by M. Larrey. In the interests of humanity, which are above those of war, it is argued that the use of explosives should be more and more restricted, and replaced by projectiles calculated rather to wound than to kill the combatants. With this object it is proposed to substitute for the explosive bullets now in use the so-called "Lorenz" projectiles, which are described as "the missiles of the future."

BOOKS AND PAMPHLETS RECEIVED

"Report of the Entomologist C. V. Riley for 1885" (Washington).—"Bulletin of the U.S. National Museum, No. 30," by J. B. Marcou (Washington)—"Géologie de l'Ancienne Colombie, Bolivarienne Vénézuela, Nouvelle-Grenade et Ecuador," by H. Karsten (Friedländer, Berlin).—"Jahrbuch der Meteorologischen Beobachtungen der Wetterwarte der Magdeburgischen Zeitung," Jahrgang iii., 1884, by Dr. R. Assmann (Magdeburg).—"Reports on Insects injurious to Hop Plants, &c.," No. 3 "Insects injurious to Fruit Crops," by C. Whitehead (Eyre and Spottiswoode).—"Lectures to Kindergartners," by E. P. Peabody (Heath and Co., Boston).—"Transactions and Proceedings of the New Zealand Institute, 1885," vol. xviii., by J. Hector (Wellington).—"Philip's Planisphere, showing the Principal Stars visible for every Hour in the Year" (Philip).—"Catalogue of the Blastoidea in the Geological Department of the British Museum (Natural History)," by R. Etheridge, Jun., and P. H. Carpenter (London).—"Choice and Chance," 4th edition, by W. A. Whitworth (Bell and Son).

CONTENTS

THURSDAY, SEPTEMBER 9, 1886

THE ECLIPSE EXPEDITION

NOTHING could have exceeded the magnificent manner in which the authorities of Grenada, and chiefly His Excellency Governor Sendall, and the commanders of the ships detailed to assist the Expedition—Her Majesty's ships *Fantôme*, *Bullfrog*, and *Sparrowhawk*—have met the wishes of, and lent assistance to, the Expedition.

As a consequence, at this time of writing (August 20) all the observers, with the exception of the Chief of the Expedition, are at their posts, with huts and instruments erected, and as much skilled assistance as they can possibly desire. The stations actually occupied so far are as follows:—

(1) Carriacou. Rev. S. J. Perry and Mr. Maunder. This party has the *Bullfrog*, two officers of which will assist, as well as Lieut. Helby of the *Sparrowhawk*.

(2) Boulogne. Prof. Tacchini and Mr. Turner. Lieut. Smith, of the *Sparrowhawk*, and a petty officer and skilled artificer, assist this party.

(3) Hog Island. Prof. Thorpe. The *Fantôme* is anchored near the observing-station, and Prof. Thorpe will have the assistance of the officers.

(4) Prickly Point. Capt. Darwin and Dr. Schuster. One or two officers of the *Fantôme*, and Capt. Maling, the Colonial Secretary, assist this party.

The fifth station, to be eventually occupied by Mr. Lockyer, is at Green Island, at the north-east corner of Grenada. Capt. Oldham, of the *Sparrowhawk*, Mr. Beresford (the Clerk of the Council), the Chief of the Police, Mr. Wright, and Dr. Boyd will assist him.

The parties at Boulogne, Prickly Point, and Green Island occupy houses which have been placed at the disposal of the Expedition by Col. Duncan, Mr. Chadwick (the Treasurer of the Island), and Mr. Belton respectively; nothing can exceed the kindness which the Expedition has received, and the assistance rendered has been so effectual, that so far everything has gone without a hitch. The labours of the Governor in the cause of the Expedition have been unceasing; he planned a hut and sent a model to Barbados, and when it was approved (by telegraph) he had four ready awaiting the arrival of the parties, which were thus enabled to proceed at once to their stations.

The weather chances are doubtful, but certainly they have improved since the arrival of the Expedition. The observations of the local cloud conditions have been so continuous lately, not only by the observers themselves, but by many at the request of the Governor before the arrival of the Expedition, that there is no question that the best stations are occupied, and it is a matter of general satisfaction that Carriacou has been added to the line of stations. The local idea is that the hurricane which passed over St. Vincent—and so nearly over Grenada!—last Monday has cleared the air, as it has been noticed that spells of fine weather generally follow them.

The *Fantôme* comes in on Sunday to convey the Green Island party to their station; although this will leave very little time for the party to establish itself, it has been considered desirable to leave the southern observers undisturbed as long as possible. The Governor and Mr. Lockyer will proceed in her to Carriacou to inspect the station there, while the hut and instruments are being erected at Green Island. The last week has been spent here in erecting and dismounting the instruments and overhauling everything, so that no time will be lost at the station itself.

There are photographic difficulties ahead: with the ordinary plates brought out here, the film simply disappears in the developer in consequence of the usual temperature of the water, about 80°. The Germans and Americans are now supplying plates here which stand this temperature easily, but they do not seem to be known in England. It looks very much as if it will be safer to take some if not all of the photographs obtained—if any *are* obtained—to be developed at home.

The Expedition will arrive in England on September 18.

St. George, Grenada

THE ZOOLOGICAL RESULTS OF THE "CHALLENGER" EXPEDITION

Report on the Scientific Results of the Voyage of H.M.S. "Challenger" during the Years 1873-76 *under the Command of Capt. G. S. Nares, R.N., F.R.S., and Capt. F. T. Thomson, R.N.* Prepared under the Superintendence of the late Sir C. Wyville Thomson, F.R.S., &c, and now of John Murray, one of the Naturalists of the Expedition. Zoology—Vol. XIV. By Prof. W. A. Herdman, and Hjalmar Théel. (Published by Order of Her Majesty's Government, 1886.)

VOLUME XIV. of the Zoological Series of these Reports contains Parts 38 and 39. Part 38 forms the second part of Prof. W. A. Herdman's Report on the Tunicata collected by the Expedition. It will be remembered that the first part was published in 1882, and that it treated of the Simple Ascidians. The Compound Ascidians are described in the present Report, and the free-swimming or pelagic forms will form a third and concluding Report. The Compound Ascidians have always been regarded by biologists as a most difficult group to describe. The impossibility of finding good diagnostic characters in external markings or general contour compels the investigator to search for such in minute internal structure—a laborious proceeding, and one that up to this had had no practical illustration. The large collection of Compound Ascidians made during the Expedition represented 102 species or well-marked varieties, and these are arranged in twenty-five genera. Eighty-eight of the species and ten of the genera are described here for the first time, and two new families have been established.

The families and genera seem to be uniformly distributed, but they are more numerously represented in the southern than in the northern hemisphere; indeed, the Compound Ascidians, like the Simple Ascidians, attain their greatest numerical development in the southern temperate zone. The Botryllidæ appear to be confined to the northern hemisphere, having there a very wide range. The Distomidæ are well represented in both hemispheres. The Polyclinidæ almost exclusively belong to the

southern hemisphere. The Diplosomidæ are from tropical seas. The family of the Cœlocormidæ is only known from the southern hemisphere. In the Didemnidæ the genera are well represented in both hemispheres, while in that of the Polystyelidæ the southern and northern forms belong to different genera.

The Compound Ascidians are not deep-sea forms. While between shore-mark and a depth of 50 fathoms over 60 species and varieties were found, but 12 species were met with at depths between 100 and 250 fathoms; 4 species extended to a depth of 500; 7 species to a depth of 1000 fathoms; and one strange form, *Pharyngodictyon mirabile*, was found at a depth of 1600 fathoms. While as a matter of course the shallow-water forms have been better known from being so much more easily collected than the deep-sea species, still Prof. Herdman seems amply justified in his conclusions that the Compound Ascidians are essentially "a shallow-water group, that they are abundant around coasts in a few fathoms of water, and that they rapidly decrease in numbers as greater and greater depths are reached."

As to the phylogeny of the group, the author has come to the conclusion that the Compound Ascidians are polyphyletic in origin, being made up of several branches, which at differing periods have arisen from the Simple forms.

As introductory to the description of new genera and species, we find a very complete and most instructive chapter devoted to the history, bibliography, and anatomy of the group. The general anatomical details are illustrated by some excellent woodcuts. The details of the anatomy of the various species are given in connection with their description, and are largely illustrated on the forty-nine plates drawn by Prof. Herdman which accompany the memoir. The investigation of the Ascidians, despite the existence of some few brilliant memoirs, is now for the first time done justice to; and, while we congratulate the author on his excellent work, we recognise in it not only an elaborate Report, but in addition a monograph of a, to this, very imperfectly known group.

Part 39 is a Report on the Holothuroideæ, by Hjalmar Théel, Part 2. In the second portion of this Report on these soft-bodied Echinoderms, Théel has not rested satisfied with giving a description only of the new forms of the groups Apoda and Pedata, which were brought home by the Expedition, but he has added a series of short accounts of all the forms known, even quoting the doubtful or little-known forms. Thus we have in this report also a veritable monograph of another most interesting group. Although unable to say much as to the bathymetrical distribution of these forms, still the *Challenger* dredgings have added many facts to our previous knowledge. Up to 1872 very few forms were known from depths exceeding 100 fathoms, and scarcely one from below 200 fathoms. Now we know of a number of forms met with at a depth of 500 fathoms, and these are generally distinct from shallow-water forms though belonging to the same genera. Several species have a vast bathymetrical distribution, some individuals still living near the shore, while others have descended without any notable change to depths of from 5 to 700 fathoms. Some few belong to genera that have no representatives in depths shallower than 500 fathoms. Among the very deep-sea forms we find *Cucumaria abyssorum*, at a depth of from 1500 to 2223 fathoms; *Synapta abyssorum*, at a depth of 2350 fathoms; *Pseudostichopus villosus*, at a depth of 1375 to 2200 fathoms; and the deepest-living of all the forms, *Holothuria thomsoni*, at a depth of from 1875 to 2900 fathoms. Some fifty-three new species or strongly-marked varieties are described and figured. A valuable bibliography is annexed Many imperfectly-described species have been re-described from fresh specimens, thus rendering this Report of immense value to the working zoologist.

OUR BOOK SHELF

Miscellaneous Papers relating to Indo-China. 2 Vols. Trübner's Oriental Series. (London: Trübner and Co., 1886.)

In Oriental matters, more than in any other branch of investigation, the student is beset at every step by the difficulty of knowing what has been done already, for, besides books and papers published in London and other European capitals (which are accessible enough), there are those published in the East itself by numerous Societies as well as private individuals. In addition, many of the *Journals* and *Proceedings* of Societies to which the student would desire to refer are long since out of print, and many of them fetch a very high price indeed. Such are the *Chinese Repository*, the *Oriental Repertory*, Logan's *Journal of the Indian Archipelago*, and many others that could be named. In London these can be consulted at the British Museum, at the libraries of the India Office and the Royal Asiatic Society, and perhaps elsewhere; but this is of little service to the student elsewhere in the British Islands, and still less to one who is working in the very field itself, in the Malay Peninsula, Java, Borneo, Bangkok, or China. Occasionally, an enterprising Society or publisher may republish some of these old papers, but this is not often done, for the number of immediate buyers is necessarily small, and the return therefore slow and doubtful.

Recent events in various parts of Further India, including in this term that part of Asia west of Burmah and south of China, have attracted the public mind to these regions. Accordingly, the Straits Branch of the Royal Asiatic Society, which has its seat at Singapore, decided to publish a first instalment of papers relating to Indo-China, but mainly to the Malay Archipelago, scattered about in various periodicals now beyond the reach of most students, and out of the question for those who are unable to consult large libraries. A selection of papers was made by officers of the Society in Singapore; these were carefully edited by Dr. Rost, the Librarian of the India Office, and the work was fortunate in being placed in Messrs. Trübner's Oriental Series—a series of works which, whether we regard individual excellence or the range of Oriental knowledge which it embraces, stands unrivalled in the world, for in every direction it forms the high-water mark of European study of the East. The present volumes include selections from the papers published in Dalrymple's *Oriental Repertory*, the old *Asiatic Researches*, and the *Journal* of the Asiatic Society of Bengal. It may be hoped that the Society will feel able and willing to continue the issue of similar selections from other sources. The papers commence about 1808, and the latest are dated about 1860, and they embrace almost every subject of interest relating to the East. Some of the earlier reports are now of merely historical interest, such as Topping's account of Keddah, Barton's surveys and description of Balambangan, and the history of the formation of the East India Company's establishment at Penang. But others are of more value. There are numerous descriptions of various economic products, as

the gum vine of Penang, the caoutchouc vine of Sumatra, and metals in the Malay Peninsula. In philology and ethnology we have a paper on traces of the Hindu language amongst Malays (by Marsden); Dr. Leyden's famous paper on the languages and literature of the Indo-Chinese natives, the alphabets of the Philippine Islands, &c. There are several papers on geology and natural history. Two of the latter are catalogues of the Mammalia and reptiles inhabiting the Malay Peninsula, by Dr. Cantor; while a third is a catalogue of the botanical collection brought home by the same naturalist in 1841. Another paper re-published has a peculiar interest, in view of the surveys undertaken by the French two years ago in the Krau isthmus for the purpose of cutting a canal. This is a report by Capts. Fraser and Forlong on a journey from the mouth of the Pakchan River to Krau, and thence across the isthmus to the Gulf of Siam. In the 16th paragraph of that report they urged that the Bay of Bengal could be connected with the China Sea by cutting through the isthmus at comparatively little expense. They enter into calculations showing how easily this could be done, the advantages of the route, &c. These calculations of distance, cost, &c., are exceedingly elaborate, and show that the two officers entered thoroughly into the matter.

It will thus be seen that the volumes offer much of interest to several classes of students, and we repeat the hope that the Singapore Society may shortly be in a position to continue the publication of further selections.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Physiological Selection and the Origin of Species

SEEING that criticisms on the theory of physiological selection are flowing through channels other than the pages of NATURE, and this in a volume larger than could at first have been anticipated, it seems desirable that I should now permit them to exhaust themselves before undertaking a further and a general reply. On the present occasion, therefore, I will only ask you to be good enough to insert the following remarks.

In order to put myself right with my critics, I should like them to remember that the paper published by the Linnean Society is designedly restricted to a preliminary statement of principles, which, it was hoped, might fulfil its avowed object of inducing other naturalists to co-operate with me in verifying the theory by observation and experiment, in the ways suggested. Such being the design, all details as to facts and references were intentionally omitted, and the same has to be said for all objections to the theory which had occurred to my own mind. All these things will require to be gone into with the utmost care, should the course of verifying inquiry eventually prove that the voice of Nature pronounces for the theory. Therefore, while I shall be thankful for all criticisms, I should like my critics to remember that they have not as yet my whole case before them. In particular, I may intimate that I should not have published even the outlines of my theory had I not been prepared for the very obvious exceptions which are taken to it by Mr. Wallace in the current issue of the *Fortnightly Review*.

I am much indebted to Mr. Francis Darwin for his reference to Mr. Belt's anticipation of my theory, for the fact that in its general form this theory has independently occurred to so distinguished a naturalist, appeals to me as an additional pledge of its probability. On the other hand, I am greatly disheartened by his further statement that he has reason to suppose his father was "familiar with the principle of physiological selection," and yet "did not regard it with any great favour." Hitherto I have been under the impression that it was a theory to which the judgment of his father would probably have inclined, and therefore I shall await with no ordinary interest the statement of his reasons for thinking otherwise, whether this be communicated through your columns or privately to myself. It only remains to add that, if Mr. Darwin will be kind enough to turn to p. 380 of my paper, he will find that I have quoted *in extenso*, and with its context on both sides, the passage from the "Origin of Species" which he extracts. But it does not appear to me that this passage furnishes any evidence that the theory of physiological selection was ever present to the mind of the writer—less evidence, for instance, than there is from a passage in one of his earliest writings that the theory of natural selection was present to the mind of Mr. Herbert Spencer.

GEORGE J. ROMANES

Geanies, Ross-shire, September 4

Actinotrocha of the British Coasts

I HAVE been reminded by Mr. T. Bolton, of Birmingham, that about three years ago I sent him living specimens of what Mr. A. G. Bourne afterwards identified as *Phoronis*. At the time I was under an impression, from hasty observation of the arrangement of the tentacular crown, and before I had seen the entire animal, that I had found a new Polyzoan allied to *Lophopus*. *Phoronis* occurs here in company with *Spio seticornis*; a solitary individual or a small group of the former, in the midst of a colony of the latter. A block of stone densely populated with these annelids is a most interesting object in a tank. To me they have proved so interesting that I believe I have spent more time over them than over any other marine organism.

I take the opportunity of calling attention to what I believe is an undescribed species of *Peridinium* that annually recurs in these waters. The form is flattish, and the outline bi-conical, having one end bifurcated, with a flagellum in the fork, and a central ciliary groove. By degrees it loses its present form, and assumes that of a spheroid.

I will gladly send specimens of either or all of these organisms to any naturalists who may wish to study them, if the cost of carriage be defrayed and the applicants not very numerous.

Sheerness-on-Sea W. H. SHRUBSOLE

A New Aerolite

ON May 28 last a farmer of Barntrup, a small town of the Principality of Lippe, in the north-west of Germany, walking in the afternoon, 2h. 30m., on the edge of a neighbouring wood, suddenly heard repeated reports like those of a gun, followed shortly after by an indistinct rumbling as of thunder. At the same time a meteorite came crashing through the leaves of a tree. The rumbling came from a south-westerly direction, the temperature was warm, the sky bright, and almost entirely cloudless.

This is the twelfth case of a meteoric fragment being found in the north-west of Germany. It is a monolith of about the size of a walnut, and weighs 17·3 grammes (specific weight = 3·495). It is covered with a black crust chipped off in places by the fall. Under this crust it is of a light gray colour and granitic substance, dotted in places with small yellow crystals, which are probably troilite or schreibersite. It has been lately presented to the Detmold Museum. L. HAEPKE

Bremen, Germany

DRAPER MEMORIAL PHOTOGRAPHS OF STELLAR SPECTRA EXHIBITING BRIGHT LINES

THE spectra of ordinary stars, whether examined directly by the eye, or indirectly by means of photography, present little variety. The comparatively few cases of deviation from the usual type are therefore particularly interesting, and the occurrence of bright lines in a stellar spectrum constitutes perhaps the most singular exception to the general rule. The brightness of the F line in the spectra of γ Cassiopeiæ and β Lyræ was noticed by Secchi. Rayet afterwards found three rather faint stars in Cygnus, the light of which was largely concentrated in bright lines or bands. The adoption at the Harvard College Observatory of a system of sweeping, with a direct-vision prism attached to the eye-piece of the

equatorial telescope, resulted in the discovery by the present writer of several additional objects of the same class. Still more recently, Dr. Copeland, during a journey to the Andes, has extended the list by the discovery of some similar stars in the southern heavens.

Among the photographic observations which have been undertaken at the Harvard College Observatory, as a memorial to the late Prof. Henry Draper, are included a series of photographs of the spectra of all moderately bright stars visible in the latitude of the Observatory. A recent photograph of the region in Cygnus, previously known to contain four spectra exhibiting bright lines, has served to bring to our knowledge four other spectra of the same kind. One of these is that of the comparatively bright star P Cygni, in which bright lines, apparently due to hydrogen, are distinctly visible. This phenomenon recalls the circumstances of the outburst of light in the star T Coronæ, especially when the former history of P Cygni is considered. According to Schönfeld, it first attracted attention, as an apparently new star, in 1600, and fluctuated greatly during the seventeenth century, finally becoming a star of the fifth magnitude, and so continuing to the present time. It has recently been repeatedly observed at the Harvard College Observatory with the meridian photometer, and does not appear to be undergoing any variation at present.

Another of the stars shown by the photograph to have bright lines is D.M. + 37° 3821, where the lines are unmistakably evident, and can readily be seen by direct observation with the prism. The star has been overlooked, however, in several previous examinations of the region, which illustrates the value of photography in the detection of objects of this kind.

The other two stars first shown by the photograph to have spectra containing bright lines are relatively inconspicuous. The following list contains the designations according to the *Durchmusterung*, of all eight stars, the first four being those previously known:—35° 4001, 35° 4013, 36° 3956, 36° 3987, 37° 3821, 38° 4010, 37° 3871, 35° 3952 or 3953. Of these 37° 3871 is P Cygni, and 37° 3821, as above stated, is the star in the spectrum of which the bright lines are most distinct.

Edward C. Pickering

PEAT FLOODS IN THE FALKLANDS

THE accompanying narratives of a singular visitation which has befallen the town of Stanley in the Falklands may be of some interest to the readers of Nature. Though the causes are so different, the effects of the bursting of a peat-bog in some respects curiously simulate those of a lava-flow. The papers have at different times been sent to Kew from the Colonial Office. It is partly in the hope that their publication may lead to some practical suggestion for dealing with the trouble that I ask for their insertion in your columns.

W. T. Thiselton Dyer

The Acting-Governor Bailey to Governor Callaghan, C.M.G.

Stanley, Falkland Islands, January 1, 1879

Sir,—I regret to have to report to you the circumstances attending an accident which happened early on the morning of November 30 last.

Just after midnight on Friday, November 29, one of the inhabitants was awakened by the continued barking of his dog, and thinking that a cow had strayed into his garden, he went outside, when to his alarm he found that his house was surrounded by a black moving mass of peat several feet in height, and travelling down the hill at about four to five miles an hour. It was not until daylight that the extent of the disaster was manifested.

The sufferers by the calamity were quite shut off from communication with the rest of the settlement, until they had cut a way for themselves through the heap of liquid peat, which everywhere surrounded their dwellings. Fortunately no lives were lost.

Immediately, when the report reached me, I proceeded to the scene of the disaster, and found the town in a worse state than it had been represented, all communication between the east and west end of Stanley being entirely cut off, except by boats. At this time there was no perceptible movement in the mass of peat which covered the ground in confused heaps, except in Philomel Street and the drain on the east side where I perceived the liquid peat moving down at a very slow rate. To get rid of this as quickly as possible, I found it advisable to turn all the water that could be dammed up, and sluice the peat whilst in a liquid state, and by this means I eventually cleared Philomel Street. On following up the course which the slip had taken, the hill presented a curious appearance. From the peat bank, down to the brow of the hill, a distance of about 250 yards, the surface-peat lay in confused heaps direct from the opening of the bog. The moving power (whether water or liquid peat it is impossible to say) travelled over the ground faster than the heavier bodies, which were left standing 3 to 4 feet above the level of the ground.

Proceeding to the top of the bog, I found a depression extending over 9 to 10 acres of ground, the edges cracking and filling up with water, and threatening another accident. I at once saw the necessity of calling upon the inhabitants to assist me in cutting a trench at the back of the hill, so as to draw off this accumulation of water, which seemed likely to float the loose peat left in the depression down into the settlement. I am glad to say that this call was heartily responded to by every man in the settlement, the gentlemen finding substitutes to take their places.

All worked for eight days in the cold and the rain, but nevertheless they were unsuccessful in carrying the trench through the bank into the bottom of the slip, owing to the soft peat welling up from the bottom and filling the trench again. Seeing that the exertions were of little avail in the present state of the bog, I did not press the settlers to continue the work that was so disheartening in its results; and as I now felt satisfied, from the great quantity of water that had been drained off, and the cutting being at a level, that this would prevent any further accumulation of water taking place in the slip, as there was no immediate danger of another accident taking place, the work was stopped, and I published the inclosed notice.

With your Excellency's permission I will, in the course of a few weeks, prepare sections of the bog and the settlement, showing a plan of drainage which will, I hope, prevent a similar accident happening again.—I have, &c.,

(Signed) Arthur Bailey

His Excellency Governor Callaghan, C.M.G.

Lieut.-Governor Barkly to Earl Granville

Government House, Stanley, Falkland Islands, June 3, 1886

My Lord,—I regret to have to report that a slip of the peat-bog at the back of the town of Stanley, similar to that which occurred in November 1878, but about 200 yards to the westward of the scene of that accident, took place last night. A stream of half-liquid peat, over 100 yards in width and 4 or 5 feet deep, flowed suddenly through the town into the harbour, blocking up the streets, wrecking one or two houses in its path, and surrounding others so as completely to imprison the inhabitants. Fortunately, as the night was wet and stormy, almost every one was within doors, and the few who were in the wrecked houses escaped in time. One child was, unfortunately, smothered in the peat, whose body has been recovered, but no other casualties are known to have occurred. An old man is, however, reported to be

missing this morning, and it is feared he may also have perished, as part of his house is almost filled with peat.

(2) The people of Stanley, as on the former occasion, showed great energy and resource in dealing with the danger, and before I myself reached the spot barriers had been erected and lanterns placed to keep the public from dangerous spots, whilst all those who had been driven from their homes had been accommodated by their neighbours.

(3) This morning bodies of volunteers were early at work, clearing the streets, so far as it was safe to do so without risk of disturbing the superincumbent mass of peat and setting it in motion again, and draining the water from it as far as was practicable. I have also employed a strong body of labourers, under experienced supervision, in the same work, and have directed the removal of all persons remaining in dangerously situated houses; and there is now little risk of further accident.

(4) The slip was caused, apparently, by the unusually heavy rains which have fallen during the last few days, and which the drains constructed by Mr. Bailey, the Surveyor, in 1878, proved insufficient to carry off. Deeper and wider cuttings will now be made, and I trust that the recurrence of any similar catastrophe may thus be prevented. The town of Stanley is, however, from its situation and the mass of peat-bog on the high ground behind it, always to some extent exposed to danger of this nature in times of unusually heavy rainfall.—I have, &c.,

(Signed) ARTHUR BARKLY

The Right Hon. Earl Granville, K.G., &c.

THE BRITISH ASSOCIATION

BIRMINGHAM, *Tuesday*

THE Birmingham meeting has been one of unusual excitement, mainly originating in the pre-arranged discussions which have taken place in several of the Sections. It is generally felt that this comparatively new feature has given new life to the Association, and ought to become general in all the Sections. At present the arrangements are somewhat crude, and the discussions are apt to become unmanageable. In some cases each of the speakers has all he means to say already written out, so that the discussion becomes merely the reading of a series of papers on a given subject. In other cases, however, at the present meeting, the discussions have been to a large extent extemporaneous. This was especially so with the joint meeting of Sections A and D to consider the subject of colour-vision, and with the discussion in Section E on Geographical Education. Probably the most lively and generally interesting discussion was that which followed Mr. Seebohm's paper on Dr. Romanes' theory of Physiological Selection. Among those who took part in this were Profs. M. Foster, Newton, and Francis Darwin. On Saturday there was a lively and instructive discussion in Section C on Pre-Glacial Man, in which Prof. McKenny Hughes, Mr. Pengelley, Prof. Boyd Dawkins, Mr. De Rance, and others took part. The address of the President, Sir William Dawson, was a great popular success, so far as he could be heard. Prof. Rücker's lecture on soap-bubbles was universally admired, the experiments being unusually brilliant. Prof. Roberts-Austen's lecture to working men, on Saturday night, on the colour of metals, was greatly appreciated by a crowded audience.

The weather has not been so good as could have been wished, so that the excursions and garden parties have been somewhat damped. The *soirée* in the highly interesting Industrial Exhibition at Bingley Hall on Thursday evening was crowded and successful. Indeed the arrangements throughout made by the Local Committee for the entertainment of visitors have given complete satisfaction; the comfort and convenience of the visitors having been provided for in every conceivable way.

At the meeting of the General Committee yesterday, it was resolved to accept the invitation to Bath for 1888. For the Manchester meeting of next year, Sir Henry Roscoe was chosen President, the meeting to begin on Wednesday, August 31. The fact of an invitation having been sent from New South Wales for 1888, has been already noticed in NATURE. The invitation came up for consideration yesterday, with the result that it was decided to send a deputation of forty or fifty representative members of the Association, to be selected by the Council in co-operation with the Sectional Committees. The New South Wales Government have offered to pay all the expenses of such a deputation, but they insist, in somewhat dictatorial terms, that the deputation shall consist only of the most eminent representatives of British science. This subsidiary meeting will take place in Sydney in January 1888, when it is hoped representatives of science from all the Australasian colonies will assemble, and with the deputation hold a meeting, which will have for its object the promotion of science in Australia, and of more intimate relations between its representatives there and here. On the return of the deputation to this country it will report its proceedings to the Bath meeting; for the Australian meeting will not be regarded as a regular meeting of the British Association. On the whole, the decision come to at the General Committee meeting appears to give satisfaction. Victoria has also sent an invitation, but agreed to retire in favour of its sister colony.

The number attending the Birmingham meeting is about 2500.

Dr. MacAlister read on Thursday last to Section A a communication from the Grenada Eclipse Expedition, announcing that excellent photographs had been taken of the eclipse, and that successful experiments with the spectroscope had been made in the northern part of the expedition by Dr. Schuster, Capt. Darwin, and Prof. Thorpe. Dr. Schuster obtained two good and two fair photographs of the corona. Good spectra of the solar prominences have been obtained, showing the bright lines of highly incandescent vapours. In this respect the result resembles that obtained in the two previous eclipses, though it was thought possible that this year, being one when sunspots are tending to a minimum, would be marked by the more continuous spectrum that bespeaks lower temperature. The bright lines were especially well marked when the slit of the spectroscope was tangential to the sun's disk, less marked when the slit was radial. Capt. Darwin was in charge of the coronagraph, an instrument by which a continuous series of photographs of the corona, before, during, and after totality, can be taken. Before and after the eclipse the photographs are taken by means of Dr. Huggins's device for mechanically shutting off the glare of the sun. The idea of Capt. Darwin's observations is to test the trustworthiness of Dr. Huggins's method. If a complete continuity appears in the series of pictures taken by what may be called the artificial and the natural methods, the confidence of solar observers in the former method will be established. The series has been duly obtained, but until the plates are closely scrutinised in England it is impossible to pronounce on the success of the test. Dr. Thorpe was in charge of an instrument prepared by Capt. Abney for the determination of the intensity of the light sent out from different parts of the corona. He has been very successful, having made no fewer than fifteen determinations.

The following is the list of grants which have been made this year by the Association:—

A—Mathematics and Physics

Solar Radiation	£20
Electrolysis	50
Ben Nevis Observatory	75
Standards of Light	10
Instructions for Tidal Observations	15

Chepstow Meteorological Observatory	£20
Magnetic Observations	40
Electric Standards	50
B—Chemistry	
Silent Discharge of Electricity	20
Absorption Spectra	40
Translation of Foreign Records	5
Nature of Solution	20
Influence of Silicon on Steel	30
C—Geology	
Volcanic Phenomena of Vesuvius	20
Volcanic Phenomena of Japan	50
Exploration of Cae Gwyn Cave	20
Erratic Blocks	10
Fossil Phyllopoda	20
Carboniferous Flora of Halifax	25
Microscopic Structure of the Rocks of Anglesey	10
Eocene Beds, Isle of Wight	20
Circulation of Underground Waters	5
Erosion of Sea Coasts	15
"Manure" Gravels of Wexford	10
Provincial Museum Reports	5
D—Biology	
Lymphatic System	25
Naples Zoological Station	100
Plymouth Biological Station	50
Granton Biological Station	75
Zoological Record	100
Flora of China	75
Flora and Fauna of the Cameroons	75
Migration of Birds	30
British Marine Area	5
E—Geography	
Batho-Hypsographical Map	25
Depth of Permanently Frozen Soil	5
F—Economic Science and Statistics	
Regulation of Wages	10
H—Anthropology	
Prehistoric Races, Greek Islands	20
British Barrows	20
North-Western Tribes of Canada	50
Racial Photographs (Egyptian)	20
Anthropological Notes and Queries	10
Total	£1300

SECTION C

GEOLOGY

OPENING ADDRESS BY PROF. T. G. BONNEY, D.SC., LL.D., F.R.S., F.S.A., F.G.S., PRESIDENT OF THE SECTION

I HAVE felt it a great honour and an especial pleasure to be asked to preside at the meeting of Section C in Birmingham. A great honour, because of the repute of my predecessors; an especial pleasure, because, as born in the Midlands, I am naturally proud of the Midland metropolis, its intellectual activity, and its commercial enterprise. Besides this, there are few towns in England where I number more friends of kindred tastes than in Birmingham. Geology especially seems to thrive in this district, and little wonder when you reckon among your residents, in addition to a host of other workers, such leaders as Crosskey, *malleus erraticorum*, Allport, who taught me how to work with the microscope, and Lapworth, to whose genius my duller mind is under constant obligation.

The addresses delivered at the annual meetings of the British Association afford a convenient opportunity for what may be termed stock-taking in some branch of science which has especially attracted the attention of the author; for a brief review of past progress; for a glance forward over the rich fields which still await exploration. We may compare ourselves to pioneers in a land as yet imperfectly known, the resources of which are only beginning to be developed. Taking our stand upon some vantage-ground at the border of the clearings, we glance forward over plains as yet untrodden, over forests as yet untracked, to consider in what directions and by what methods of investigation new lands can be won through peaceful conquest, new treasures added to the world's intellectual wealth.

I purpose, then, on the present occasion to offer a few remarks upon a branch of geological investigation which appears to me one rich in promise for future workers. The key-note of my address might be conveyed in the following sentence: "The application of microscopic analysis to discovering the physical geography of bygone ages." The ultimate aim of geological researches is obtaining answers, in the widest and fullest sense, to these two problems in the history of our globe—the evolution of life upon it, and the evolution of its physical features. In the former a host of labourers, before and since the epoch of Darwin's great book, have been employed in collecting and co-ordinating facts, and in framing hypotheses by scientific induction. In the latter the workers are fewer, but the results obtained are neither small nor unhopeful. In the past generation, men like Godwin-Austen pointed out the principles of work and gathered no small harvest, but in the present the application of the microscope to the investigation of rock-structure has facilitated research by furnishing us with an instrument of precision; this, by disclosing to us the more minute mineral composition and structural peculiarities of rocks, enables us to recognise fragments, and sometimes even to determine the source of the smaller constituents in a composite clastic rock. The microscope, in short, enables us to declare an identity where formerly only a likeness could be asserted, to augment largely in all cases the probabilities for or against a particular hypothesis, and to substitute in many a demonstration for a conjecture.

Once for all, I ask you to bear in mind that this address is mainly a recital of other men's work, so that I shall not need to interrupt its continuity by remarks as to the original observers. The subject is, indeed, one to which I have paid some attention, but I can only call myself a humble follower of such men as Godwin-Austen, "the physical geographer of bygone periods," and Sorby, who was the first to apply the microscope to similar problems, and to whom in this class of investigation we may apply the well-known saying, *Nihil tetigit quod non ornavit*.

With the deepest gratitude also I acknowlege the loan or the gift of specimens from Drs. Hicks and Callaway, from Messrs. Howard Fox, Somervail, Shipman, Gresley, Houghton, Marr, Teall, and J. A. Phillips, from Profs. Lapworth and Judd. Through their liberality I have had the opportunity of examining for myself the greater part of the materials which have already been described in the principal geological periodicals, and of adding many new slides to my own collection.

The nature of the materials of grits and sandstones has been so admirably treated by Dr. Sorby in his presidential address to the Geological Society for 1881 that I may pass briefly over this part of the subject. I will, however, add a few details in the hope of placing more clearly before you the data of the problems which are presented to us. In order to exemplify the size of the fragments with which we have to deal, I have made rough estimates of the diameters of the constituent grains in a series of quartzose rocks. Sometimes there is much variability, but very commonly the majority of the grains are tolerably uniform, both in size and shape. In a slide from the Lickey quartzite, exposed in the railway cutting at Frankley Beeches, grains, often well rolled, ranging from ·02″ to ·03″ are very common. In a specimen of Hartshill quartzite, they range from ·01″ to ·03″, but the most common size is a little under ·025″. In a quartzite from west of Rushton (Wrekin) a good many grains range from ·03″ to ·05.″ In two speciments of quartzite (white and pale grey) from near Loch Maree, the grains commonly vary a little on either side of ·02″, while in a specimen of the "fucoidal quartzite" (mouth of Glen Logan) much greater variety is exhibited, a good deal of the material being about ·01″ in diameter, but with many scattered grains up to ·03″. The grains in a pale grey quartzite from the Bunter beds at the north side of Cannock Chase range from about ·01″ to ·015″, and are very uniform. In a liver-coloured quartzite from the same locality they are about as long, but narrower and sharply angular in form. These will serve as examples of what we may call an average, moderately fine grit or sandstone. It is my impression that in a very large number of ordinary sandstones most of the grains range from about one to three hundredths of an inch. In rocks, however, to which most persons would apply the epithet "rather coarse-grained," fragments of a tenth of an inch or more in diameter are common.

It is extremely difficult to give, in general terms, an estimate

of the size of the crystalline constituents of ordinary granites, and the more coarsely crystalline gneisses. But speaking of those which enter into the composition of the ground mass, I should say that the individual quartz grains do not often exceed ·03″, and are very frequently between this and ·02″. In the finer-grained granites and more distinctly banded gneisses, and their associated quartziferous schists, about ·01″ is a common size, while in the finer schists (believed by many geologists to be later in date than the aforesaid) they range from ·002″ downwards, and do not generally exceed ·001″. Feldspar crystals, where they occur, probably do not differ very materially in area from the quartz, though they are often, as might be expected, rather longer and narrower; mica crystals, cut transversely, are often longer and usually much narrower. Of other constituents, as being either rarer or more liable to change, I will not speak in detail. The individual quartz grains, in the compact and glassy varieties of the more acid igneous rocks, are about the same size as those in an ordinary granite.

Space does not permit me to enter upon the methods of distinguishing between the materials furnished by the different varieties of crystalline schists, gneisses, and igneous rocks of similar chemical composition. For the most important of these I must refer you to Dr. Sorby's address, but I may add that there are others which it would be almost impossible to describe in words, as they can only be learnt by long-continued work and varied experience. I do not pretend to say that in the case of a grit composed of fragments of about ·02″ diameter we can succeed in identifying the parent rock of each individual, but I believe we can attain to a reasonable certainty as to whether any large number of its constituents have been furnished by granitoid rocks, by banded gneisses and schists, by fine-grained schists or certain phyllites, by older grits or argillites, or by lavas and scoria. There seem to be certain minute differences between the feldspars from a granitoid rock and from a porphyritic lava, and more markedly between the quartz grains from the two rocks. The latter can generally be distinguished from the polysynthetic grains furnished by certain schists or veins, and these not seldom one from another. Obviously the larger the fragments the less, *cæteris paribus*, the difficulty in their identification. When they exceed one-tenth of an inch the risk of important error is, I believe, to a practised observer comparatively small.

Obviously, also, the shape of the grains leads to certain inferences as to the distance which they have travelled from their original source, and as to the means of transport, but into the details of this I must forbear to enter. I will merely remind you that small angular fragments of quartz are so slowly rounded when transported by running water that, if well-rounded grains appear in large numbers in a sandstone, it seems reasonable to suppose that these are, in the main, wind-drifted materials.

Thus every rock in which the constituent particles admit of recognition and of identification may be made to bear its part in the work of deciphering the past history of the globe. Where the constituents have been derived from other rocks, we obtain some clue to the nature of the earth's crust at that epoch; where the locality whence a fragment was broken can be discovered, the nature, strength, and direction of the agents of transport can be inferred. Some idea as to the structure and surface-contour of the earth in that district, and at that time, can be formed; and thus the petrologist, by patient and cautious induction, may, in process of time, build up from these scattered fragments the long-vanished features of the prehistoric earth, with a certainty hardly less than that of the palæontologist, when he bids the dry bones live, and repeoples land and sea with long-vanished races. The latter study is in vigorous maturity, the former is still in its infancy: so much wider then is the field, so much more fascinating, to many minds, is the investigation. There are many districts which are without fruit for the palæontologist—there are few indeed which, to the petrologist, do not offer some hope of reward. The field of research is so wide that not one nor few men can gather all its fruits. It needs many workers, and it is in the hope of enlisting more that I have ventured to bring the subject before you to-day.

Materials of the Coarser Fragmental Rocks of Great Britain[1]

I proceed now to give a brief epitome of the constitution, so far as I know it, of our British grits, sandstones, breccias, and conglomerates. I shall exclude, as involving too many collateral issues, the Post-Pliocene beds, and dwell more on the earlier than on the later deposits, because the latter obviously may be derived from the former by denudation, so that it becomes the more difficult to conjecture the immediate source of the constituent particles. Further, in order to avoid controversy on certain questions of classification, or for brevity, I shall occasionally group together geological formations which I think separable.

It may be convenient, however, to call your attention to the localities at which, at the present day, granitoid rocks (many of which may be of igneous origin, but are of very ancient date), gneisses, and crystalline schists are exposed in Great Britain, as well as those where considerable masses of igneous rock of age not later than Mesozoic occur. The former constitute a large part of the north-western and central highlands of Scotland and of the islands off its west coast; they are exposed in Anglesey and in the west and the north of Carnarvonshire; they form the greater part of the Malvern Chain, and crop out at the Wrekin; they occur on the south coast, at the Lizard, and in the district about Start Point and Bolt Head; they rise above the sea at the Eddystone. It is probable that these last are the relics of a great mass of crystalline rock, which may have extended over the Channel Isles to Brittany; also, that we may link with the *massif* of the Scotch highlands the crystalline rocks of Western Ireland on the one hand, and of Scandinavia on the other. Among the indubitably igneous rocks we have granite, or rocks nearly allied to it, in Scotland, in the Lake district, in Leicestershire, and in Devon and Cornwall. Feldstones, old lavas, and tuffs of a more or less acid type occur in Southern Scotland, to some amount also in the Highlands, in the Lake district, and in various localities of rather limited extent in West-Central England, as well as in the south-west region just mentioned, while in Wales we have, in the northern half, distinct evidence of three great epochs of volcanic outbursts, viz. in the Bala, in the Arenig, and anterior to the Cambrian[1] grits and slates. In South Wales there were great eruptions at the last-named epoch and in Ordovician times. I have passed over sundry smaller outbreaks and all the more basic rocks as less immediately connected with my present purpose. It is, I suppose, needless to observe that a coarsely crystalline rock, whether igneous or of metamorphic origin, must be considerably older than one in which its fragments occur.

Cambrian and later Pre-Cambrian.—That the majority at least of the gneisses and crystalline schists in Britain are much older than the Cambrian period will now, I think, hardly be disputed by any who have studied the subject seriously and without prejudice. There are, however, later than these, numerous deposits, frequently of volcanic origin, whose relation to strata indubitably of Cambrian age is still a matter of some dispute. Therefore, in order to avoid losing time over discussions as to the precise position of certain of these deposits, or the particular bed which in some districts should be adopted as the base of the Cambrian, I will associate for my present purpose all the strata which, if not Cambrian, are somewhat older. The latter, however, exhibit only micro-mineralogical changes, and of their origin, volcanic or clastic of some kind, there can be no reasonable doubt; so that the difference in age does not appear to be enormous; that is to say, I include with the Cambrian the Pebidian of some recent authors.

The utility of microscopic research has nowhere been better exemplified than in the case of the oldest rocks of St. David's. Some authors have supposed that the base of the Cambrian series in this district has been "translated" beyond recognition, others that it has been thrust out of sight by the intrusion of granitic rock. But low down in the series, beneath the earliest beds that have as yet furnished fossils to British palæontologists, there is a well-marked and widespread conglomerate; underlying this, with apparent unconformity, comes a series of beds very different in aspect, chiefly volcanic, and beneath this a granitoid rock. The conglomerate, in places, even without microscopic examination, proves the existence, though probably at some distance, of more ancient rock, as it is full of pebbles of vein-quartz and quartzite; but in other parts it is crowded with pebbles closely resembling the feldstones in the underlying volcanic group, and in some parts becomes a regular *arkose*, made up almost wholly of quartz and feldspar,

[1] I have been obliged to exclude those of Ireland, as I have so little material from that country, and for want of space have not dealt fully with those of Scotland.

[1] I take the base of the Arenig as the commencement of the next formation, the Ordovician, which thus represents one phase of the Lower Silurian in the variable nomenclature of the Geological Survey.

closely resembling those minerals in the granitoid rock, of which also small rounded pebbles occasionally occur. One or two fragments of a quartzose mica-schist, which is not known to occur *in situ* in the district, have also been found. It is therefore evident that not only is the volcanic series somewhat, and the granitoid rock considerably, older than the conglomerate, but also that an important series of rocks, some of which were thoroughly metamorphic, was exposed in the district when the conglomerate was formed. I have very little doubt that a study of the finer-grained sedimentary Cambrian beds overlying the conglomerate will corroborate, were it needed, the conclusion which the latter justifies. Passing on to North Wales, the coarser beds in the Harlech axis, so far as they have been examined, are found to be full of fragmental quartz and feldspar, which is undoubtedly derived from a granitoid rock; some beds being made up of little else. No rock of this character, so far as I am aware, is exposed in this part of Wales, but a ridge of granitoid rock extends from the town of Carnarvon to the neighbourhood of Port Dinorwig. Through this, apparently, the great feldstone masses which occupy considerable tracts on the northern margin of the hills between Carnarvon Bay and the valley of the Ogwen have been erupted, and over this comes a series of grits, slates and conglomeratic or agglomeratic beds, overlain ultimately by the basal conglomerate of the undoubted Cambrian series. It was formerly maintained that these feldstones were only lower beds of the Cambrian metamorphosed—practically fused by some "metapeptic" process. This notion, however, was quickly dispelled by microscopic examination. The overlying conglomerate is often crowded with pebbles, identical in all important respects with the feldstone itself, which also presents many characteristics of a lava flow as opposed to an intrusive mass, and is no doubt an ancient rhyolite now devitrified. There is some difference of opinion among the geologists who have worked in this district as to the exact correlation of various gritty, conglomeratic or agglomeratic beds which succeed the feldstone, as is only natural where disturbances are many, and continuous outcrops generally few. But all agree on the existence of a series, into which volcanic materials enter largely, between the above-named basal Cambrian conglomerate and the feldstone. In this, then, and in the basal conglomerate we have again and again more or less rounded fragments of old rhyolitic lavas. We have numerous and varied *lapilli*, probably of like chemical composition. We have grits which are largely composed of quartz and feldspar, resembling that in the granitoid rock, together with fine-grained quartzose schists and bits of rhyolite, all mingled together. We have also occasionally, as in the Cambrian conglomerate near Llyn-Padarn, pebbles of the granitoid rock. Further, the basal conglomerate, as near Moel Tryfaen, is sometimes crowded with fragments of gritty argillites. Fine-grained schists, as will be noted, seem to be rare in this district, but, as such rocks occur *in situ* in the Lleyn peninsula, they will probably be discovered more abundantly when the Cambrian conglomerate is examined further in that direction.

Fine-grained micaceous, chloritic, and other schists occupy a considerable portion of Anglesey, and in the neighbourhood of Ty Croes there is an important outcrop of granitoid rock. The former were once regarded as metamorphosed Cambrian, the latter as granite which aided in the metamorphism at the end of the Ordovician period. In Anglesey the earlier Palæozoic rocks are not generally rich in fossils, so that it is sometimes difficult to settle their precise position. The oldest beds which have been thus identified have been placed in the Cambrian (Tremadoc), but some experts have doubted whether quite so low a position can be assigned to them. Hence the exact age of the oldest Palæozoic beds in this island is uncertain, as also whether the basal conglomerates near Ty Croes are of the same age as those in Carnarvonshire. This, however, is certain, that some of the Anglesey grits above the basal conglomerate are largely made up of quartz and feldspar derived from a granitoid rock. Others contain numerous fragments of very fine-grained schists, like those so abundant in the island, and the conglomerate contains pebbles sometimes full two inches in diameter, absolutely identical with the rocks in the adjacent granitoid ridge (the foliated structure distinctive of some parts of it having been even then assumed), together with various metamorphic rocks, some green schistose slaty rocks, and some reddish slates. The last two were, I doubt not, cleaved before they became fragments; probably these were derived from the hypometamorphic series, which Dr. Callaway has described, and which also contains pebbles of the granitoid rocks. Fragments of the characteristic fine-grained schists are, so far as I at present know, less common among the Anglesey grits and conglomerates than one would expect, perhaps owing to their comparative destructibility; but I have found them occasionally and suspected their presence more often. Hence there can be no doubt that older crystalline rocks have very largely contributed to the formation of at least the coarser members of the lower Palæozoics of Anglesey.

Passing now to Central England, we come to regions which may be regarded as almost the exclusive property of your local geologists. The Hollybush sandstone on the flanks of the Malvern is, no doubt, largely composed of the finer debris of the older rocks of that chain, but the Malvern hills are only an unburied fragment of a vastly larger area of crystalline Archæan rock. This is just indicated some seven miles further north in the Abberley Hills. It crops up at either end of the Wrekin, and for a little space near Rushton, but in the later fragmental rocks of the district we have abundant proofs of its existence. The central part of the Wrekin is composed of volcanic rocks, rhyolites of varied kinds, with agglomerates; these were once regarded by our highest authorities as greenstones intrusive in beds of Ordovician age, but Mr. S. Allport has taught us their true nature, and Dr. Callaway has proved their far greater antiquity. Similar rocks are to be found elsewhere in the neighbourhood of the Wrekin, and in the district farther west. We cannot affix a precise date to the volcanic outbursts of the Wrekin, but we can prove that they are not newer than the quartzite which fringes the hill, as it contains fragments of the perlitic and other glassy rocks of the apparently underlying series. This quarzite is certainly much older than the newer part of the Cambrian, and pebbles of rhyolites, resembling those of the Wrekin, occur in the indubitable Cambrian beds farther west. For instance, a grit at Haughmond Hill is quite full of fragments of volcanic rock, many of these scoria; another suggests the derivation of some of its materials from a mica-schist, while, according to Dr. Callaway, the conglomerates and grits of the Longmynds (which form the main part of the mass) are largely derived from older rocks, the former being crowded with pebbles of purple rhyolite, quartz, feldspar, mica, and occasional bits of mica-schist. A most interesting conglomerate, apparently older than the quartzite, occurs at Charlton Hill. This contains more or less rolled fragments of grits, quartzites, and argillites, looking in several cases as if they had undergone, before being broken off, the usual micro-mineralogical changes. It contains also fragments of rhyolite and many of coarse granitoid or gneissoid rocks of Malvernian type, besides numerous grains of quartz and feldspar of a like character. Finer-grained gneissoid rocks and schists, micaceous, hornblendic, or chloritic, are present in fair amount. The last bear some resemblance to the Rushton rocks, and remind me strongly of rocks which occur in the Highlands and in the Alps, apparently not in the lowest part of the Archæan series. Some also resemble the Anglesey schists. The quartzite itself is largely made up of grains of quartz which appear to me to have been derived from old granitoid rocks. Occasional grains, however, suggest by their composite structure derivation from a quartzose rock of finer texture, and, as already said, bits of the Wrekin rhyolite sometimes occur. The same is true of the Lickey quartzite, in regard to all three constituents, in which an occasional grain of microcline, very characteristic of old granitoid rocks, has been observed. The quartz grains in this and in the former rock are occasionally very much rounded. The Lickey quartzite has lately been shown by Prof. Lapworth to overlie rhyolitic rocks, and it is much older than the lowest Silurian. Not improbably it is of the same age, and was once connected with that of the Wrekin district. The Hartshill quartzite, near Nuneaton, has a similar composition, is below Cambrian, and overlies some rhyolitic rocks. Thus these insulated areas prove the existence of an old fragmental series, which is largely composed of materials derived from pre-existing and much more ancient Archæan rocks. It is difficult to assign a date to the unfossiliferous rocks forming the rugged hills of Charnwood Forest, but, as they have been affected by very ancient earth-movements, and there is nowhere any valid evidence of volcanic activity in the true Cambrians, they may be assigned with most probability to the antecedent epoch, which seems to have been one of great disturbance. Microscopic examination has shown that materials of volcanic origin enter largely into the composition of these Charnwood rocks, even the most finely grained; but besides occasional fragments of slaty rock in the breccias, for which a high antiquity cannot be asserted, we find

some pebbles of vein-quartz and two or three beds of quartzite. The grains in these appear to have been derived from old granitoid rocks, and not from the porphyritic rhyolites of the district. In one case at the Brande, they are most conspicuously rolled, and this has happened, though less uniformly, in a grit from near the ruins, Bradgate, which also contains grains of compound structure. In conclusion, I must briefly notice the so-called Torridon sandstone of North-Western Scotland, which is in many respects invaluable to the student. That it is not later than the base of the Ordovician is indisputable; that it is underlain by and derived from a mass of Archæan rocks—gneisses, more or less granitoid, with occasional schists—is universally admitted. Its coarser basement beds are crowded with fragments of the underlying gneisses and schists, and since the epoch of their formation no material change has taken place in either the one or the other. The finer beds, though other materials occasionally occur, are largely, sometimes almost exclusively, composed of grains of quartz and of feldspar identical in every respect with those in the underlying series. It may be a fact of some significance, for it agrees with what I have elsewhere noticed in very old fragmental rocks, that the feldspar appears to have been broken off from the parent rock while still undecomposed, and in many cases is even now remarkably well preserved. It would seem, therefore, as if the denudation of the granitoid rock had been accomplished without material decomposition of its feldspar, but I must not allow myself to digress into speculations on this interesting and suggestive fact. While referring to this district I may mention the quartzites, though, strictly speaking, they are Ordovician in age. These in some cases consist all but exclusively of quartz grains derived from the Archæan series, which, however, are generally smaller than those in the Torridon; it would seem as if the feldspar of the parent rock had either decomposed *in situ*, or had been broken up in consequence of the longer distance from the source of supply. This quartzite is sometimes of singular purity, containing little or no earthy material, and only rarely a flake of mica or a grain of feldspar, tourmaline, or epidote (?).

Ordovician-Silurian.—In regard to the earlier of these formations I am better acquainted with the volcanic than with the non-volcanic fragmental beds. Still, so far as I have seen, we find among the latter frequent indications of a supply of materials from regions of crystalline as well as of ordinary sedimentary rocks. The quartzite of the Stiper Stones (possibly earlier than the Arenig) appears to have derived most of its grains from granitoid rocks, and probably the same is true of many of the coarser beds in the Caradoc group of Shropshire and Eastern Wales. The Garth grit of Portmadoc appears to have derived much of its quartz from a like source as the Stiper Stones, but it also contains bits of a fine-grained quartzose schist and of older clastic rocks. A grit from the Borrowdale series of Chapel-le-dale contains, in addition, bits of old andesite and probably diabase, with fragments of a rather granitoid gneiss and quartzose schists. Fragments of crystalline rock, both small and large, abound in the Upper Llandovery beds at Howler's Heath, at Ankerdine Hill, in the Abberley district, on the west flank of the Malverns, and at May Hill, thus indicating that early in Silurian times far greater outcrops of crystalline rock existed than are now visible west of the Severn. Mr. W. Keeping (*Quart. Journ. Geol. Soc.* vol. xxxvii. p. 149, &c.(calls attention to the abundance of fragments of quartz, feldspar, and mica in the "greywackes" of the Aberystwith district, which give the rock sometimes quite a granitoid appearance, and adds that, in his opinion (*ibid.* p. 150), "the abundance of feldspar crystals, so general in the Silurian rocks (Upper Silurian of North Wales, South Wales, and the Lake district), points to the neighbouring presence of a vast mass of early, perhaps primæval, igneous rocks as the great source of sediment supply in Silurian times." What I have seen of the Denbigh grit of North Wales and of the Coniston beds of the Lake district confirms this conclusion. It is true that some of the material may have been supplied by Ordivician volcanic rocks, and that the quartz grains in the specimens which I have examined are not large. But we must remember that the latter can hardly have been furnished by the lavas of the Lake district; and those of North Wales, though richer in silica, do not, so far as I know, generally contain large quartzes. These, indeed, may have been derived from the denudation of Cambrian rocks, but I should doubt the sufficiency of such an explanation. In one specimen, a Denbigh grit from Pen-y-glog, near Corwen, there occurs, besides one of smaller size, a fragment about ·1" in diameter, exhibiting a micrographic arrangement of quartz and feldspar. In Cornwall, among beds which are almost certainly Ordovician or Silurian, we find similar evidence of derivation from much more ancient rocks. The conglomerates of the Meneage district contain, in addition to quartzites, greywackes, and other old sedimentary beds, abundant fragments of a moderately coarse-grained granitoid rock, and occasionally a hornblendic rock similar to the well-known Lizard schist. A series of specimens which I have examined microscopically shows, in addition to compact igneous rocks, apparently volcanic, quartz grains probably derived from granitoid rock, various fine-grained schists and schistose argillites or phyllites, quartzites, grits, and other older clastic rocks. One fragment of schist contains little eyes of feldspar, and in general structure reminds me of some in the so-called "Upper Gneiss" series of North-Western Scotland. Another, a fine-grained mica-schist or a phyllite, exhibits a cleavage transverse to the rumpled foliation.

A rich harvest probably awaits the explorer in the "greywackes" of the southern uplands of Scotland. A "Lower Silurian" conglomerate from Kingside, Peebles-shire, contains numerous fragments of igneous rocks, probably of volcanic origin, and bits of granitoid rock, with some which are either very old quartzites or perhaps vein-quartz. These have been crushed and re-cemented before being detached from the parent rock. The basement conglomerate of the Craig Head limestone group (Llandeilo-Bala) is full of rounded fragments of volcanic rocks. These, as in the last-named case, exhibit considerable variation; the majority, however, are probably andesites, and perhaps in one or two cases even basalts. A Middle Llandovery conglomerate from near Girvan is largely made up of fragments which appear to have been derived from very ancient quartzose rocks. The greywacke of rather later age from near Heriot, Edinburghshire, contains, with numerous volcanic fragments, and a little argillite, a few bits of fine-grained quartz-schist, together with grains of quartz and feldspar, suggestive of derivation from a more coarsely crystalline rock.

Old Red Sandstone and Devonian.—It is, I believe, indisputable that when the Old Red Sandstone of Scotland was formed a great period of mountain-making had ended and one of mountain-sculpture was far advanced. The conglomerates are often full of fragments of the crystalline rocks of the Highlands, and no doubt the sandstones derived their quartz grains from the same source. In the southern half of the country, however, as is well known, volcanic materials, more or less contemporaneous, play an important part. I have not been able to examine closely the Old Red Sandstones of England and Wales, but their frequent near resemblance to the sandstones of Scotland suggests a similar derivation. True, the materials may have been sifted from older clastic rocks, but there is nothing specially to suggest this, and the abundant pebbles of vein-quartz, which I have seen in one or two localities, seem rather more favourable to the other hypothesis. I have only examined microscopically a very few specimens of Devonian grit, all from the south side of the country. These certainly seem to have derived their materials, in part at least, from crystalline rocks, both granitoid and schists of finer grain; one specimen also apparently containing some bits of hypometamorphic rock.

Carboniferous.—In Scotland some of the basement beds of this series so closely resemble the Old Red Sandstone that no further description is needed, and the same remark may be made of the very few overlying sandstones which I have carefully examined. In the North of England the basement conglomerates, so far as I have seen them, are made up of earlier Palæozoic rocks, but for many of the great masses of sandstone which occur in the series a source of supply is not so easily found. Dr. Sorby, who has made a special study of the Millstone grit of South Yorkshire, tells us that it is formed of grains of quartz and feldspar, apparently derived from a granite, and contains pebbles, sometimes an inch or so in diameter, of vein-quartz, of hard grits, of an almost black quartz-rock or quartz-schist, and of a non-micaceous granite. He also notes one fragment of a greenstone, and another either of a fine-grained mica-schist or of a clay-slate. The granite, he states, more resembled those of Scandinavia than any one now visible in Britain, and the bedding indicated a supply of materials from the north-east. In the Millstone grit near Sheffield he says that the grains appear to be but little worn, as if they had not been drifted from far. A few also appear to have been derived from schists. From what I have myself seen, I anticipate that Dr. Sorby's conclusions may be extended to most of the other coarser Carboniferous clastic beds of Northern England, except that, perhaps, as was inferred by Prof. Hull,

another important, if not the principal, source of supply must be sought on the north-west. The materials of the basement conglomerates and grits in North Wales appear to be either Palæozoic rock or vein-quartz and an impure jasper; but a microscopic study of carefully selected specimens, especially from Anglesey, might produce interesting results. In Central England, as the Old Red Sandstone is commonly absent, and, if present, must have been speedily buried, we should naturally look further afield for the materials of the Coal-Measure sandstones and Millstone grit, where it occurs. But probably we shall be right in including this, as indicated by Prof. Hull, with the northern district. He also points out that in the south-western part of England and in South Wales there is good evidence that the materials have been brought by currents from the west. I have only examined one specimen from this region, but it has proved very interesting. It is from a Carboniferous grit near Clevedon, in Somersetshire. About one-third of the rock consists of quartz grains which I should suppose derived from schists or gneisses of moderate coarseness; quite another third of fragments of a very fine-grained micaceous schist, about ·03″ long. It is possible that these may be phyllites, but I think it far more probable that they are true schists. They are very like some of the more minutely crystalline schists of Anglesey, and it is evident in some cases that the rock had been corrugated subsequent to foliation. This grit also contains a few bits of feldspar and flakes of mica. I must not forget to mention some curious boulders which have been discovered occasionally in actual coal-seams. Through the kindness of Mr. Radcliffe I have been able to examine some specimens found at Dukinfield Colliery. They are hard quartzose grits and quartzites, bearing a general resemblance to sundry of the earlier Palæozoic rocks. One of the latter is as compact and clean-looking as the well-known quartzite of the North-Western Highlands. Besides quartz, and perhaps a little feldspar, it contains a small quantity of iron-oxide (?), two or three flakes of white mica, a grain or two of tourmaline, and of a mineral resembling an impure epidote. A similar quartzite has been found by Mr. W. S. Gresley in a coal-seam in Leicestershire, and I have described another from the "thirteenth coal" at the Cannock Chase Colliery. In each of these quartzites the two minerals last named may also be detected.

Before quitting the Carboniferous series I must call attention to some interesting grits which during the last few years have been struck in deep borings. In the London district a red sandstone, in some places conglomeratic, has been found underlying sundry members of the Mesozoic series. Some have thought this of Triassic age, but inasmuch as it is very doubtful, as we shall presently see, whether the coarser beds of the Triassic formations extended so far to the east, and the dip of the red beds in the well at Richmond agrees better with that of the Palæozoic rocks in other parts of the buried ridge, I think these sandstones more probably older than any part of the Mesozoic series, perhaps not very far away from the base of the Carboniferous. In the boring at Gayton, in Northamptonshire, Lower Carboniferous rocks were succeeded by reddish grits and sandstones. The finer beds much resembled the ordinary Old Red Sandstone, and, like it, suggested a derivation from fairly coarse-grained crystalline rocks. But of the origin of one rock, a quartz-feldspar grit, there can be little doubt. I may briefly describe it as very like the Torridon sandstone of Scotland, except that the cement is calcareous. I do not, indeed, claim for it a like antiquity, for I think it far more probably about the age of the lowest part of the Carboniferous series; but it, too, must have been derived from granitoid rocks. While some of the grains are fairly well rounded, others, especially of feldspar, as in the Millstone grit of South Yorkshire, do not seem to have travelled very far.

Permian.—The sandstones of the northern area belonging to this formation do not, as far as I have been able to ascertain, afford us much information. Quartz grains, of course, abound, but as they are rather small, it is not possible to be sure whether they have been primarily derived from a granitoid rock or a schist. The former, however, appears to me the more probable source. They also contain fragments of feldspar still recognisable, flakes of mica, and possibly a little schorl. The frequent occurrence of crystalline quartz as a secondary formation in these sandstones is a point of much interest, but has no relation to my present inquiry. The breccias near Appleby, Kirkby Stephen, &c., which I have not seen, indicate that at this time not distant masses of Carboniferous limestone, and of earlier Palæozoic rocks, were undergoing denudation; but it appears to me improbable that the finer materials of the sandstones were furnished by any rocks in the vicinity.

The Permians of the central area offer a rich field for future work. For the materials of the sandy beds I should conjecture a distant source, but for the pebbles in the conglomerates, and the fragments in the breccias, we need not travel very far afield. The Lower Carboniferous Measures contributed limestone and chert, the former being especially abundant in the conglomerates, but the "vein-quartz, jasper, slates, and hornstone," mentioned by some observers, indicate that yet earlier rocks furnished their contingent, while of the igneous materials I will speak directly. I shall pass very briefly over the breccias, so well displayed, for instance, on the Clent and Lickey hills, at no great distance from this town, because I trust we shall have presented to us, in the course of this meeting, a sample of the rich harvest which is awaiting explorers. Earlier investigators looked towards Wales for the origin of these fragments; we shall, I believe, learn that the majority are more probably derived from rocks, which, though now almost hidden from view, exist at no great distance. Some of the more compact traps may have come from the old rhyolites, which, by the labours of your geologists, have been detected *in situ* beneath the Lickey quartzite, while we may venture to refer the "red syenite" and "red granite" to outcrops of crystalline rocks of Malvernian age. These breccias have been regarded as proving the existence of glaciers in the Lower Permian age. It is, of course, possible that floating ice has been among the agents of transport, but after carefully examining the specimens in the museum of the Geological Survey on which glacial striæ are asserted to occur, I am of opinion that the marks are due to subsequent earth-movements. On only one specimen did I recognise glacial striation, and this pebble is so different from the rest that I think it must have come from drift, and not from the Permian beds.

No less interesting are the Permian breccias of Leicestershire. These have attracted the attention of an indefatigable local geologist, Mr. W. S. Gresley, and to his kindness I am indebted for the opportunity of examining both rock specimens and slices. As might be expected, fragments, which I have no hesitation in referring to the Charnwood series, are not wanting, though hitherto they have not occurred in any abundance; but perhaps the most interesting member is a tolerably hard conglomerate, containing rather abundantly pebbles of a speckled grit and of a compact "trap." Microscopic examination of this conglomerate, which varies from a fairly coarse puddingstone to a grit, shows that the above-named speckled grit is composed of small and rather angular fragments of quartz, associated with grains of brownish and greenish material, which may be in some cases decomposed bits of a rather basic lava, in others possibly a glauconite of uncertain origin. But the "trap" pebbles are yet more interesting. These are the more numerous, and are commonly well rolled. They probably belong, roughly speaking, to one species, but exhibit many varieties. In a single slide I have seen at least six, perfectly distinct. Some are indubitably scoriaceous, others full of microliths of a plagioclastic feldspar, others almost black with opacite, others mottled brown devitrified glasses, more or less fluidal in structure. Probably they belong to the andesite group, with a silica percentage not very far away from sixty. In none have I observed any signs of crushing or cleavage, so that I cannot refer them to the Charnwood series, but conjecture that they are relics of volcanoes later in age than the great earth movements which affected that series, though I cannot connect them with the more basic post-Carboniferous outbreaks of which we have indications at Whitwick and elsewhere. Quartz grains also occur, and some of these exhibit a rather peculiar "network" of cracks which is characteristic of this mineral in the rocks of Peldar Tor, Sharpley, &c., and one such grain is attached to a fragment of minutely devitrified rock. Hence, as shown by larger fragments, the Charnwood series has contributed to the materials of this conglomerate, but the more abundant appear to have been derived from volcanic vents, the locality of which is at present undiscovered.[1]

Trias.—The Bunter beds and the lower part of the Keuper consist of more or less coarse materials, while in the remainder of the latter such deposits are rare and local. Hence it is evident that after the deposition of the Keuper sandstones a very different set of physical conditions prevail. The lower series consists of sandstones and conglomerates; these beds occur in

[1] I pass by the interesting pebbles of hæmatite, which have received special attention from Mr. Gresley.

considerable force on the eastern side of the Pennine Chain, have a great development in Lancashire and Cheshire, and thin away towards the south-east, almost disappearing in eastern Leicestershire and in Warwickshire. As the Trias is followed southwards, along the valley of the Severn, the Bunter in like way dies out, while the Keuper marls may be traced on into Somersetshire and Devon. In that region also there is a grand development of the lower and coarser members. As might be expected, there are considerable differences between the lower Triassic deposits of the northern and southern areas, so that it will be convenient to speak of them separately. The northern group, as is well known, is separable in the Midland and north-western district into the Lower Bunter sandstone, the Pebble-beds, and the Upper Bunter sandstone, over which come, more or less unconformably, the Keuper sandstones. Pebbles are either absent from, or very rare in, every part of the Bunter except the pebble-bed, and are generally small and scarce in the Keuper sandstones, except in the basement breccias. It will be convenient to make a few remarks on them before dealing with the associated sands and sandstones. The pebbles in the Bunter conglomerate are most abundant, and generally attain the largest size in the Midland district. Towards the north-west, though the conglomerate attains a thickness of more than 500 feet, pebbles are rarer and smaller, and this, I believe, is also the case in Yorkshire, though the thickness of the deposit is not so great. I can, however, answer for the occurrence of pebbles of fair size and in considerable abundance for some distance to the north of Retford. In the Midland district they are very frequently from about 2″ to 4″ in diameter, though smaller are intermingled and occasionally some of large size; these attain in certain localities to a diameter of 6″, or even a little more. The majority, as far as I know, are well rounded. In this district many different kinds of rock are found in the conglomerate; the most abundant are quartzose—vein-quartz, quartzites, and hard grits or sandstones. Besides these we find chert and limestone from the Carboniferous series, various fossiliferous rocks of Silurian, Ordovician, and possibly Cambrian age, with mudstones and argillites, more or less flinty, of uncertain date. Feldstones, using the term in a wide sense, are not rare, and granites or granitoid rocks sometimes occur. These, however, together with the scarce fragments of gneiss and schist, are usually very decomposed. A hard quartz-feldspar grit, sometimes very like a binary granite, may be found, and I have noticed a peculiar black quartzose rock of rather schistose structure. As the lithology of the Bunter conglomerate has already attracted the notice of more than one author, I shall restrict myself to a brief mention of its more salient features. The most abundant rock is a quartzite, frequently so compact as to give a rather lustrous sub-conchoidal fracture, in which the individual grains can be with difficulty distinguished. In colour it varies mostly from white to some tint of grey, but is occasionally "liver-coloured." Rather obscurely marked annelid-tubes are the only organic indications which I have observed in these quartzites, and these are very rare. Under the microscope the rock consists chiefly of quartz fragments, of various forms in different specimens, with an occasional fragment of feldspar (sometimes, I think, silicified), a flake of white mica, a grain of tourmaline, and of an impure epidote (?). As a rule it is easy to distinguish this quartzite from the other indurated arenaceous rocks which occur in the conglomerate, especially from those containing fossils.

The above-described quartzites differ in appearance both macroscopically and microscopically from those of Hartshill, the Lickey, and the Wrekin district, but they closely resemble the most compact variety, which I have already described as occurring in boulders in coal. They have also an extraordinary likeness to quartzite pebbles in Old Red Sandstone and Lower Carboniferous conglomerates of Southern Scotland and to the quartzites of the Northern and Western Highlands, already described, a liver-coloured variety of which, as I have been informed, occurs in the island of Jura. These quartzite pebbles, to my knowledge, may be traced into Lancashire on the one side of the Pennine Chain and to beyond Retford on the other. The quartz-feldspar grit consists mainly of quartz and feldspar, obviously the debris of granitoid rock. I have found it at various localities on the northern margin of Cannock Chase, and have received specimens from the Bunter beds near the Lickey and near Nottingham. The rock, macroscopically and microscopically, presents an extraordinary resemblance to the Torridon sandstone of North-West Scotland, and differs from every other rock which I have seen *in situ* in any other part of Britain. The nearest approach to it is the quartz-feldspar grit, already mentioned as having been struck in the Gayton boring, Northamptonshire, but this has a calcareous cement. The feldstones vary from micro-crystalline to glassy rocks more or less devitrified, some being slightly scoriaceous. They may be classified lithologically as quartz-felsites, rhyolites (more or less devitrified), quartz-prophyrites, porphyrites, and old andesites. Some specimens contain a considerable amount of tourmaline, and I have seen this mineral in the vein-quartz pebbles. It also occurs rather abundantly in a very hard, black quartzose grit. I have received varieties of feldstone, which I have found on Cannock Chase, from the Bunter beds of the Lickey and from Nottingham. In Staffordshire pebbles of granitoid rock, gneiss, and schist are not only rare, but also generally too rotten to admit of examination; but I found a few months since, in the conglomerate at Style Cop, near Rugeley, two pebbles of a whitish gneiss, which appeared to me to indicate a secondary cleavage-foliation, such as may be observed in many parts of the Scotch highlands. The black quartz-schist already mentioned exhibits a peculiar "squeezed-out" structure, which ordinarily indicates that the rock has undergone great pressure.

The sandy matrix and associated sandstones of the conglomerate beds, together with those of the Upper and Lower Bunter, and of the Lower Keuper, consist mainly of quartz grains, most of which appear to have been derived originally from granitoid rocks. They are often more or less angular, but at certain horizons, as described by Dr. Sorby, Mr. Phillips, Mr. G. H. Morton,[1] and others, well-rounded grains are so abundant as to suggest an exposure to the action of the wind. They are often stained red with iron peroxide, and mixed with more or less earthy matter. In Cheshire and Lancashire recognisable grains of felds ar have been noticed by Mr. Morton and others, and probably this mineral is, in most cases, the source of the argillaceous constituents which are often intermingled with the quartz grains. Flakes also of white mica are sometimes rather common. So far as I have been able to judge, distinct grains of rolled feldspar are commoner in the north-western district than in Staffordshire, where, however, mica-flakes are sometimes rather abundant.

The Keuper sandstone seems to me to differ from the above only in the general absence of the red colour, and in a more even bedding, especially towards the upper part (the waterstones), where they are interbedded with the marls. The appearance of these last suggests that the currents were gradually losing strength, and only capable of transporting the finer feldspathic detritus with occasional tiny plates of mica.

The lithology of the lower part of the Trias in the southern area is as yet imperfectly worked out, and a rich harvest awaits the student. My own knowledge of it is but superficial, so that I must pass it by with a brief notice. The great beds of breccia, so finely exposed on the South Devon coast, are crowded with fragments, sometimes of large size; these have clearly been derived from the older rocks which are still in part exposed to the west and south-west, and probably had once a much greater extension in the latter direction. Fragments of Devonian limestone, grits, and slate, together probably with other Palæozoic rocks, earlier and later, are mingled with granites, resembling those of Cornwall and Devon, and many varieties of more compact igneous rock. The fossiliferous quartzite pebbles which occur mingled with others in the Trias at Budleigh Salterton, have been discussed by the late Dr. Davidson in an exhaustive memoir ("British Fossil Brachiopoda," *Mem. Palæont. Soc.* vol. iv. p. 317). He refers the majority of the fossils obtained from them to the Lower Devonian age, but a few are Caradoc, and four occur in France in beds which are either Llandeilo or perhaps a little older. As the first two formations are represented, lithologically and palæontologically, on the opposite side of the Channel in France, and the third is at present only known to occur in the *Grès Armoricain* of that country, he thinks it probable that these pebbles have been derived from rocks which are now concealed beneath the waters of the Channel. It may then, I think, be taken for granted that land to the west and south-west has supplied the materials of the Lower Trias of the southern district of England, and I may add that there is every reason to believe that outliers of the formation itself still exist beneath the sea.

The so-called dolomitic conglomerates, which occur chiefly in

[1] In an excellent paper published in the *Proceedings* of the Liverpool Geological Society, vol. v. p. 52.

Somersetshire, have been so fully worked out by Mr. Etheridge and Mr. Ussher as to require but a passing notice. It is evident that they differ somewhat in date, though probably all may be referred to the age of the Keuper, and that they are local breccias or conglomerates formed around the margin of islands or on a continental coast-line during a gradual subsidence and in comparatively quiet waters.

Jurassic.—Coarse detrital material is not very common in the Jurassic series. The limited Rhætic beds indicate a transition from the peculiar physical conditions of the Keuper to the marine conditions of the Lias, and the sediment in them was probably derived from the same source as the Keuper marls. Great clay beds also occur, as is well known, throughout the Jurassic series; and the sandstones, so far as I have been able to examine them, do not enable me to offer any suggestions as to their origin. Probably some of the grains were originally derived from granitoid rocks, but they may have been directly obtained from other sandstones. A grit, however, in the estuarine series of the lower oolites of Yorkshire (Mr. Phillips's collection) looks as if it might have been partly derived from a schist, but as this is the only rock from the northern area which I have had the opportunity of minutely examining, it would be imprudent to speculate.

Neocomian-Cretaceous.—I have examined very few specimens from the fresh-water Neocomians of the south of England, but, so far as I have seen, I should think it probable that the quartz had been derived from old crystalline rocks, though perhaps not immediately. The same remark applies to the sands of the upper and marine series, which, in one instance at least, exhibit exceptionally rounded contours.[1] Among these, however, conglomeratic beds occur which have already attracted some attention. It is obvious that no small part of the materials, as at Farringdon, Potton, and Upware, has been derived from fossiliferous secondary rocks of earlier date. There are also pebbles of vein-quartz and quartzite which, however, may have been obtained by the denudation of Triassic rocks. The "Lydian stone," which is abundant in angular or subangular fragments at Potton and Upware, is for the most part chert from the Carboniferous Limestone, or in some cases from Jurassic rocks, but a few specimens may be flinty argillites, and thus of greater antiquity. One or two pebbles of older Palæozoic rock have been found, and a hard quartz grit has occurred, containing among its grains minute acicular crystals, probably of tourmaline. Potton has furnished one or two pebbles which appear to be a devitrified pitchstone, and a large pebble of porphyritic quartzfelsite has been sent to me by Mr. Willet from Henfield (Sussex). These conglomerates, together with others in the Upper Neocomian of England, have been so fully described by Mr. Walter Keeping (*Geol. Mag.* Dec. 2, vol. vii. p. 414), that I need not enter into further details, though I am well aware that the subject is by no means exhausted.

For a like reason I may pass briefly over the remarkable erratics found in the Cambridge greensand (Sollas and Jukes-Browne, *Q. J. G. S.* vol. xxix. p. 11). They occasionally slightly exceed a cubic foot in volume, but are generally smaller. Among them are diverse sandstones and grits, probably Palæozoic, granite, gneiss, various schists, quartzites, and slates, besides greenstone, a very coarse gabbro or hypersthenite, and a compact feldstone. I think it highly probable that many of these erratics came from the north, in some cases almost certainly from Scotland, and were transported by ice, though I am not satisfied that any exhibit true glacial striæ. In the south of England a boulder of old quartzose rock, perhaps a piece of a coarse quartz-vein, crushed and re-cemented, has been found by Mr. J. S. Gardner in the gault, and in the chalk we have the well-known cases of the granitic rock and other boulders at Penley, near Croydon, and of coal (Wealden or Jurassic) in Kent (Godwin-Austen, *Q. J. G. S.* vol. xvi. p. 327). Mr. Godwin-Austen describes other instances of pebbles in chalk, and I have received two or three small specimens from Mr. W. Hill, from about the horizon of the Melbourne rock, which, however, have not yet thrown any light on the subject.

Eocene.—Previous writers have called attention to the fact that the sand of the Thanet, Oldhaven, and Bagshot beds is mainly composed of quartz. This is abundantly confirmed by my own observations. So far as I have seen, in all these the grains are not, as a rule, conspicuously rounded. It can hardly be doubted that older sandstones or granitoid rocks lying to the west have furnished the materials of the Bagshot series, which still has so wide an extension in that direction; their lithological similarity would lead us to look towards the same quarter for the materials of the more limited Oldhaven and Thanet beds. The well-rolled flint pebbles in the Oldhaven series, and in occasional layers in the Bagshot, suggest the proximity of a shore-line of Upper Cretaceous rocks.

I have had no opportunity of adding to what has been written on the lithology of the limited Pliocene deposits in England, and, as stated at the outset, have excluded from the scope of this address all beds of later date, which have been so ably discussed by Mr. Mackintosh, Dr. Crosskey, and many other geologists.

Principles of Interpretation

In attempting to interpret the facts which I have enumerated we must bear in mind the following principles:—

(1) Pebbles indicate the action either of waves of the sea,[1] or of strong currents, marine or fluviatile.

(2) The zone in the sea over which the manufacture of pebbles can be carried on is generally a very narrow one. It extends from the high-tide line to the depth usually of a few feet below low-water mark. It is estimated that as a rule there is no disturbance of shingle at a greater depth than twenty feet below the latter. It is therefore probable that a thick and very widely extended pebble bed is not the result of wave action.

(3) The movement of the deeper waters of the sea as a rule is so slight that only the very finest sediment can be affected by it. Now and then great currents like the Gulf Stream, or more locally "races," may have sufficient power to transfer pebbles and sand, but instances of this will be exceptional, and confined to rather shallow water. The larger coast currents, however, may transport mud to considerable distances, but in directions parallel with the main trend of the shores.

(4) Except where very large rivers discharge their water into the ocean, or in some special case of (3), sediment is deposited comparatively near the shores of continents. Even in the case of very large rivers only the finer sediment is carried far from land. The *Challenger* soundings have shown that 150 miles is about the maximum distance from land within which any important quantity of detrital materials is deposited.[2] As a rule (so far as I can ascertain), the coarser sediments are generally deposited within a few miles of the coast. Hence this is fringed by a zone of sediment, which, after passing a maximum thickness within a short distance from the shore, gradually thins away. I doubt whether this detrital fringe is often more than seventy or eighty miles wide; probably the coarser sands do not usually extend for so much as a quarter of this distance. The inertia of the mass of the ocean water quickly arrests the flow of even the mightiest river or reduces it to a mere superficial current. Hence the great ocean basins are regions where rock-building is carried on slowly and chiefly by organic agency. Their borders bear the burden, and the load taken off the continent is laid down on the bed of the adjacent sea.

(5) Thus rain and rivers are generally more important agents of denudation and transportation than the sea, because unless the land be rising or sinking the zone over which the latter can operate is limited both vertically and horizontally.

(6) The coarser materials of rock are capable of being transported by streams to considerable distances without serious diminution of volume. Prof. Daubrée has proved experimentally that a stream flowing at the rate of about two miles per hour would roll angular fragments of quartz or hard granite into perfectly smooth pebbles after a transit of 25 kilometres ($15\frac{5}{8}$ miles). During this process the fragments lost about four-tenths of their weight. Further transport reduced the volume of the pebbles very slowly. The loss afterwards varied from 1/1000 to 4/1000 per kilometre. To reduce a pebble of 2 inches diameter to 1 inch diameter—that is, to diminish its volume by seven-eighths—would require a journey of from about 219 to 875 kilometres (approximately from 137 to 548 miles). This approximation, rough as it is, becomes still less exact as the pebbles decrease in size; the rate of diminution in volume (*cæteris paribus*) bearing a relation to the area of the surface. Thus the smaller the pebble the further it will travel without material diminution of size. Sand grains are even rounded with extreme

[1] Prof. Rupert Jones has called attention to sand-worn pebbles in the Upper Tunbridge Wells sandstone of the Weald (*Geol. Mag.* Dec. 2, vol. v. p. 287).

[1] The waves of lakes also have some rounding effect, but this—except in the case of very large lakes, such as Lake Superior—is not important; and such cases are, of course, not of common occurrence.

[2] I except floating pumice, cosmic dust, &c., as comparatively unimportant.

slowness. According to the same author a quartz grain 1/50 inch in diameter requires to be transported by water action some 3000 miles before losing its angles. On this account the presence in a sandstone of numerous well-rounded grains is taken to indicate the action of wind, for, as is well known, blown sands are much more quickly rounded.[1]

(7) Thus deposits of gravel and coarse sand, of considerable vertical thickness and great extension, are more likely to indicate the immediate action of a river than of a marine current. If limited in extent they may have been formed at the embouchure of a river into a lake or sea. If, however, they can be traced for long distances, they are more probably in the main sub-aërial deposits from rivers.

The following examples may convey some idea of the kind of river which would be required to transport the more important deposits of grits and stones mentioned in the first section of this address:—

The old river-gravels of the Sierra Nevada are "in some places 300 or 400 feet thick and almost homogeneous from top to bottom," sometimes they even obtain a thickness of 600 feet. Mr. Whitney is of opinion that the fall in these old river channels was probably from 100 to 130 feet per mile. Apparently, however, we need not invoke so large a fall as this. The total fall of the Danube is 3600 feet, and its length 1750 miles. From Passau to Vienna the fall is 1 in 2200, from Vienna to Old Moldava 1 in 10,000. Yet the velocity of the current from Vienna to Basias (fifteen miles above Old Moldava) is "from two to three knots an hour," depending on the amount of water. This would suffice to transport pebbles of the average size of the English Bunter. Below the Iron Gates the fall is still less rapid, but sand is carried down for a very considerable distance. If then the rivers of the old continental land resembled the larger streams of Europe, they would suffice for the transport of the materials with which we have dealt, especially if aided by coast currents after the debris had reached the sea.

(8) If boulders occur in a matrix consisting of fine mud, or mainly of organic material, they must (unless they are volcanic bombs) have floated thither either attached to large seaweeds or entangled in the roots of trees, or supported by ice. If they are rather numerous and a foot or more in diameter, in a marine deposit, the last is the most probable mode of transport. A cubic yard of ice will more than suffice to float a cubic foot of average rock.

Conclusion

The facts already mentioned, regarded in the light of the above principles, justify, in my opinion, the following inferences as to the past physical geography of our country. At the commencement of the Cambrian period great masses of Archæan rock, granites, gneisses, and schists, must have existed, not only on the western side of Britain, but also over a considerable tract of land now covered by the sea. Detritus from this continent became an important constituent in the Cambrian rocks, and in many cases, as at St. David's, in Anglesey, Carnarvonshire, &c., the shore-line must have been very near at hand. With the Cambrian period commences a long continued subsidence, so that its basement beds at different places are very probably not all of quite the same age. The land surface was from the first irregular, and it is very probable that waves of the sea were fretting away some parts, while rain and river, heat and cold, were still sculpturing others. But among the materials of the ancient land were not only granitoid rocks, gneisses, and schists, but also newer strata more distinctly of clastic origin, memorials of past denudation—quartzites and grits, phyllites and slates, not to mention others—and these, by their intimate structure, sometimes indicate that great earth-movements must have already occurred.[2] In many localities, perhaps as a result of these disturbances, there occurred, towards the conclusion of the Archæan period, great volcanic outbursts—by which, no doubt, numerous cones were built up, and many of the materials of the so-called Pebidian group were supplied. It is, I think, at present hardly safe to attempt to trace the exact land boundaries of the Cambrian ocean, but the enormous masses of Archæan material which are entombed in the earlier Palæozoic strata of Wales and of North-West Scotland can, I think, only be explained by the proximity of a great continental land—an extension of the present Scandinavian peninsula—which not improbably had a general slope towards the south-east, the main watershed of which may have lain some distance to the west of the Outer Hebrides.[1] But even over the more central parts of Britain there cannot have been deep or open ocean. We are constantly coming upon the traces of pre-Cambrian and early Cambrian land; some of our Mid-England Archæan masses, like the Malverns, appear to have risen above the water, and to have undergone denudation after the great earth-movements which ushered in the Silurian period. Prior to this, after a time of repose in the Cambrian, at more than one epoch, and in more than one place, there were great volcanic outbursts, which appear to have studded the sea with volcanic islands, and to have added to the heterogeneous materials from which the sediments were now formed. It is evident that in Silurian times the coast-line had extended southward and eastward. The coarse deposits of this age, in Wales, the Lake district, and Southern Scotland, compared with the finer mudstones and limestones of the Welsh border and of England, seem fully to bear out this assumption, which is in accordance with a well-known law of mountain-making. The Old Red Sandstone of Scotland and of Wales indicates a yet further continental extension towards the south east. A great epoch of mountain-making in the Scotch highlands, which had perhaps been going on at intervals from the beginning to the end of the Silurian period, had now come to an end; the southern uplands had risen up, like a Jura to the Alps. But probably their elevation did not terminate the earth-movements, for the post-Silurian cleavage of the Lake district, and the absence of Old Red Sandstone both here and in Central England indicate that the Palæozoic land mass continued to extend on its south-eastern flank. The Devonian period introduces us in the greater part of Great Britain to an epoch of limited and exceptional deposits, and of widely prevalent terrestrial conditions. It seems almost certain that the Old Red Sandstones of Scotland and Wales are of fresh-water origin—the deltas of rivers, formed either in lakes or possibly in part as sub-aërial deposits. Streams of considerable volume and of some strength are indicated by the materials. In one case, the Old Red Sandstone of North-East Scotland, we may perhaps discern in the Great Glen some indication of the old river course. It is easy to ascertain the source of the materials of the Scottish Old Red Sandstones. They are as obviously the detritus of the Highland mountains—then probably a far grander and loftier chain—as the nagelflue and the molasse of Switzerland are of the Alps.

At this time marine conditions prevailed in the south of England. The sea appears to have deepened towards the south, but I suspect that a region of crystalline rock still existed at no great distance in that direction and in the west. Probably the Brito-Scandinavian peninsula curved round to the east so as to include some part of Brittany.[2] Another great epoch of subsidence now commenced, commemorated by the formation of the Carboniferous limestone. At this I need hardly glance, as it has been so fully discussed by Prof. Hull and other writers. The land sank both in the north and in the south of England. There was deep sea over Derbyshire and Southern Wales, but the ground beneath our feet probably remained above water, forming either a continental promontory or a large island.

There were other well-known interruptions to the sea, which also overflowed a considerable part of Ireland and districts far to the east of England. The Scotch highlands, however, probably remained above water, for, as is well known, the Carboniferous limestone of Central Scotland overlies a fresh-water formation, and is itself not wholly marine, since it contains coal, and like conditions prevailed in Northumberland.

Gradually, however, the sea shallowed, and terrestrial conditions returned. In the later part of the Carboniferous series we have clear indications of two, or perhaps three, important currents, almost certainly those of rivers, bringing materials, in the southern district from the west; in the northern, from the north-west and probably the north-east. These materials may have been in part derived from older Palæozoic rocks, but the facts when fairly considered seem to indicate that there was also an extensive denudation of crystalline and not improbably Archæan rocks, unless we suppose that great areas of eruptive Palæozoic granite have now disappeared beneath the waters. At any rate, we may perhaps regard the open water between Ireland

[1] See on the subject of this paragraph Daubrée, "Géol. Expériment.," vol. i. sec. 2, ch. i., and J. A. Phillips, *Q. J. G. S.*, vol. xxxvii. p. 21, &c.

[2] It is evident, for instance, that the north-west strike, and other effects of folding, had been produced in the Hebridean series of North-West Scotland before the Torridon sandstone was deposited.

[1] Possibly the comparatively rapid deepening of the Atlantic beyond the 100-fathom line may have some relation to the western outline of this primæval Atlantis.

[2] Compare, as an illustration, the curving round of the Alpine chain on the western side of Italy.

and Scotland on the one hand, and to the east of the latter country on the other, as significant of a denudation earlier than that of the sea which has in later times divided the British Isles. Another epoch of earth-movements closed—as was to be expected—the Carboniferous subsidence and deposition. We trace one line of flexures and of intense compression along a broad zone, including the south of England, from Germany to Ireland; another less intense over the northern part of our country; the axes of the former flexure striking a little north of west, of the latter about west-south-west. The one appears to me to indicate a thrust from a great mass of hard, more or less crystalline rock in the south, which probably led to the formation of a mountain-chain extending from North-Central Europe over the Channel to the southern margin of England. The latter may be explained by the presence of the above-named north-western continent.

In the Permian time terrestrial conditions probably prevailed over a large part of Britain. It is extremely difficult to ascertain the exact circumstances under which the Permian beds of Central England were deposited, but I should think they imply a return to physical conditions not unlike those of the Old Red Sandstone, though perhaps the marine fossils which have been found in Warwickshire may indicate that the water there had some imperfect connection with the sea. I must not discuss the vexed question of the age of the Pennine Chain, but must content myself with expressing my opinion that, at most, it can only, as yet, have very partially interrupted the continuity of the water in Northern England. The beds there appear to indicate a supply of materials from the north and north-west, as if the old rivers had not been wholly diverted by the great earth-movements which closed the Carboniferous period. Sir A. Ramsay's view, that the water in which the dolomitic limestone was deposited was more or less cut off from the open sea, seems to me by no means improbable; in any case, it is a rather exceptional formation, and over the greater part of Britain, probably, land sculpture continued, and deposition was on the whole local.

With the Trias a new era commences; physical features had been now produced, which in all probability endured through a considerable part of Mesozoic times. The facts which I have laid before you, regarded in the light of the general principles indicated above, compel us to look away from the immediate vicinity for the bulk of the materials, coarse and fine, of which the northern Trias is composed, though neighbouring hills may have furnished occasional contributions, especially to the earlier deposits. The analogy of the Old Red Sandstone, the Calciferous Sandstone of Scotland, and the Nagelflue and Molasse of Switzerland, together with other peculiarities too well known to need repetition, make it in the highest degree probable that the Bunter beds were not deposited in the ocean.[1] Hence they must be either deltas formed in an inland sea or in a lake, or true fluviatile deposits. Neither lake nor inland sea appears likely to have been sufficiently large to admit of waves or currents capable of either rounding the pebbles or transporting the materials. We are therefore compelled to fall back upon the action of rivers. The sandy beds of the Bunter indicate a stream flowing from one-third to half a mile an hour, the pebbles one from two to three miles; that is to say, the Upper and Lower Bunter sandstones would require the former rate of movement, the Pebble Beds the latter. Now, we must remember that, in the West-Central district, the Lower Trias consists of three wedge-like masses, about a hundred miles in length, of which the coarser is probably the more extensive. The comparative uniformity of the deposits in each case indicates a uniformity of flow, and suggests either a large and broad stream, not liable to much variation, or one which, when flooded, quickly made a channel of its valley, and deposited mainly at such season. I have the greatest difficulty in understanding how a current of the requisite velocity could be maintained by the water of a river or rivers flowing into a lake or an inland sea, or in explaining the tripartite arrangement of the beds on the hypothesis that a basin was gradually filled up from the northward by a stream which, like the Rhone at the upper end of the Lake of Geneva, gradually advanced its delta by flowing over the materials which it had previously deposited in the basin. Hence I believe that we must regard the Bunter beds as subaerial deltas, analogous to the conglomerates in the Siwalik deposits of India,[2] and to the sandstone and nagelflue on the outer zone of the Alps, deposits in all respects very similar to the English Bunter. We may suppose, then, that rivers emerging on each side of the Pennine Chain from a mountain land first formed the Lower Bunter sandstones, then, owing to increasing upheaval in the mountain district, and corresponding depression in the lowlands, flowed more swiftly so as to cover this deposit with the Pebble Bed, and lastly, as its former conditions returned, laid upon this the Upper sandstones. I have spoken, for the sake of clearness, as if these were perfectly distinct formations, but it would by no means follow that some part of the finer beds to the south-east might not be contemporaneous with a portion of the coarser beds to the north-west, as the velocity first increased, and then diminished. As I have already said, the materials of the pebbles and of the sand make it impossible to refer the main constituents to local sources. Many of the rocks do not exist in the Midland; there is no reason to suppose that at that time there were in this region masses of land of sufficient area and height to feed important rivers.[1] From currents of any other kind we are precluded, so that I believe we may safely turn our eyes northward and look for the ultimate source of the Triassic sandstones and conglomerates among the older rocks of the Scotch highlands, and their extension to east and to west, though very probably the materials may have been more directly supplied from Old Red Sandstone and early Carboniferous strata, in remnants of which identical fragments may still be seen. In like way we may regard the Trias of the south of England as the detritus of at least one great river, which flowed from the west or south-west. The materials of the Keuper came from the same directions in each case, but here, I think, we have indications of deposition in an inland sea. Breccias formed on its coasts, and sands were at first deposited in it; but presently the area of water increased, and the coarser materials must have been arrested in the uplands, while the fine sediment which forms the marls may have been carried out into the salt lake and slowly settled down in its calm waters.[2] Its shores may have been hardly more favourable to a vigorous development of life than were its salt-saturated waters; during this period and the preceding Bunter the lowland border of the mountains, like some of the northern districts of India, may have been arid and barren regions of shifting sands.

The Trias of Northern Scotland very probably indicates a repetition on a more restricted scale of the physical conditions of the Old Red Sandstone, but after this we observe signs of an encroachment of the Atlantic on the above-named old area of continental land.

The Jurassic series is represented in Northern Scotland on both the western and eastern coasts by marine or estuarine beds. This probably indicates important modifications in the river channels, subsidence on the west altering the slopes, reducing the length, and cutting away some of the feeding-ground. Traces may still be discerned in England of the two northern rivers, but that which in Triassic times was the larger contributor, appears in Jurassic to have been gradually enfeebled; the other one and the south-western stream seem to have still flowed with some strength. Sands, however, now become comparatively local. Probably the coarser materials, as a rule, did not reach the sea. This appears at all times to have been comparatively shallow and inclosed by land on every side but the south-east. The recent discovery of Oxford Clay beneath the Cretaceous beds at Chatham suggests that a narrow strait running in a northerly direction may have insulated the Palæozoic rocks

[1] Compare also the Bunter and Keuper in the region traversed by the German Rhine.

[2] The analogy of the Indian conglomerates was suggested to me by Dr. Blanford. See *Geol. Mag.* Dec. 2, vol. x. p. 514.

[1] It may be useful to give a rough idea of the quantity of rock which must have been denuded in order to obtain materials for the Bunter beds. Suppose, for purposes of calculation, we consider the Bunter beds, which cover the district from the Cheshire coast to the Midland counties, as forming the section of a cone contained by two planes drawn through the axis so as to include an angle of 30°. If h be the height of this axis, and r the radius of the base, the volume of this figure is $\frac{\pi r^2 h}{36}$. Take, for purposes of rough calculation, $h = \frac{1}{4}$ mile, $r = 80$ miles, $\pi = 3$; the volume is about 133 cubic miles. Conceive this piled up to form a long mound, in section an isosceles triangle 1 mile high, with a base of 4 miles. The length would be over 65 miles. Thus the materials buried in the Bunter beds of the above-named district represent a chain of hills unfurrowed by valleys 5000 feet high, 4 miles wide, and 65 miles long. Suppose the Pebble Bed, a like slice of a cone, axis one-tenth of a mile, base 70 miles; the volume is more than 40 cubic miles. Suppose the quartz and quartzite pebbles one-tenth of its volume; these represent a mass of 4 cubic miles, or a line of hills like the above 1000 feet high, 2 miles wide, and 20 long.

[2] The lake may have gradually become salt, or possibly the Muschelkalk sea may have for a short space invaded Britain, and then have been insulated like the Caspian.

beneath the London district. The clays of the Lias, Oxfordian, and Kimmeridgian probably indicate a direct discharge of sediment into the sea,[1] the limestones, depression sufficing to convert valleys into fjords, in the upper parts of which sediment was deposited so that the waters of the sea were clear. The deposits of the Purbeck and Weald indicate that the western river still drained an extensive area, and a gradual rise of land in later Jurassic times, especially towards the south, appears to have advanced the river delta eastwards, and to have limited the area of the Jurassic sea on the north.

Towards the end of the Neocomian, owing to a widespread subsidence, the sea once more returned to South-Eastern England, and a communication appears to have been opened between it and the Speeton basin. This comparatively narrow strait was a region of considerable denudation and of strong and shifting coast currents.[2] The Cretaceous subsidence at first brought back physical conditions not very different from those prevalent in Oxfordian and Kimmeridgian times, but later on a very considerable depression must have so far submerged the northern continental land as either to break up the parts adjacent to Britain into groups of islands, or at least to flood the valleys so completely as to prevent any discharge of sediment into the sea. The erratics of the Cambridge Greensand suggest that a free communication into the northern ocean was established, anterior to the formation of the Chalk marl, through some part of the present interval between Scotland and Scandinavia, so as to set up a coast current with a southerly drift of shore ice near the eastern part of England; to this also may be due the erosion of the Cambridgeshire Gault.

The larger part of Britain was dry land during the Eocene, though the sea after retreating appears to have again encroached over the southern and eastern districts of England. The sands may indicate that the western river again resumed its course;[3] the extension of the London Clay up our eastern coast suggests that the northern river still flowed. But with the important disturbances which closed the Eocene and ushered in the continental conditions of the Miocene—new flexures along the old east and west lines—the earlier physical features appear to have been finally obliterated, and the sculpture of the English lowlands began. The tale of the volcanic outbursts of Western Scotland has been so well told by my friend and predecessor Prof. Judd that I need do no more than recall it to your minds. The Pliocene deposits of Eastern England indicate a new encroachment of the Franco-Belgian Tertiary sea.

Thus ends my sketch, too lengthy, I fear, for your patience, yet too brief to allow of a complete treatment of the subject. It may, however, suffice to indicate that in geology the "task of the least" is by no means despicable, and that great results may be hoped from apparently small means; that in this search for "Atlantis through the micro-cope" we may find it very near at hand, and may discover analogies, as has been indicated in our President's address, between the two borders of the ocean which severs Europe from America. An enlarged study of the materials of our Palæozoic and later detrital rocks may indicate that from very early times there has been a recurrence of similar physical conditions, and that in geology also a recurrence of effects indicates a recurrence of the same causes. The facts which I have brought before you have justified, I trust, my opening remarks as to the rich harvest which yet awaits investigations into the structure of the fragmental rocks. To resume the simile then used, I see the land of promise, stretching far away from beneath my feet, till it seems to melt into the dim and as yet unknown distance. Not speedily will its riches be exhausted. Our hands will long have vanished, our voices will long have been still, before the work of discovery is ended, and men have reached the shore of that circumfluent ocean which, at least in this life, limits their finite powers.

[1] The considerable distance to which the clays extend in a southerly direction may possibly indicate that, to the east of Scotland, a communication had now been opened with the northern ocean, which had set up a current along the coast east of the Pennine Chain.

[2] As the Speeton beds continue to be clays, one would infer a drift from the south, but a current to the opposite direction would be more probable, and it is the opinion of Dr. Sorby that this was the case. His papers "On the Direction of the Currents indicated by the Coarse Sediments in our British Rocks" are most valuable (*Yorks. Geol. Pol. Soc.* v. 220, &c.).

[3] The occasional beds of flint pebbles indicate a neighbouring shore line of Cretaceous rocks rather than the denudation of beds of Cretaceous age, which had been deposited on parts of the western land during the period of depression.

SECTION D

BIOLOGY

Opening Address by William Carruthers, Pres.L.S., F.R.S., F.G.S., President of the Section

In detaining you a few minutes from the proper work of the Section, I propose to ask your attention to what is known of the past history of the species of plants which still form a portion of the existing flora. The relation of our existing vegetation to preceding floras is beyond the scope of our present inquiry: it has been frequently made the subject of exposition, but to handle it requires a more lively imagination than I can lay claim to, or, perhaps, than it is desirable to employ in any strictly scientific investigation.

The literature of science is of little, if any, value in tracing the history of species, and in determining the modification or the persistency of characters which may be essential or accidental to them. If help could be obtained from this quarter, botanical inquiry would be specially favoured, for the literature of botany is earlier, and its terms have all along been more exact than in any of her sister sciences. But even the latest descriptions, incorporating as they do the most advanced observations of science, and expressed in the most exact terminology, fail to supply the data on which a minute comparison of plants can be instituted. Any attempt to compare the descriptions of Linnæus and the earlier systematists who, under his influence, introduced greater precision into their language, with the standard authors of our own day, would be of no value. The short, vague, and insufficient descriptions of the still earlier botanists cannot even be taken into consideration.

Greater precision might be expected from the illustrations that have been in use in botanical literature from the earliest times; but these really supply no better help in the minute study of species than the descriptions which they are intended to aid. The earliest illustrations are extremely rude: many of them are misplaced; some are made to do duty for several species, and not a few are purely fictitious. The careful and minutely exact illustrations which are to be found in many modern systematic works are too recent to supply materials for detecting any changes that may have taken place in the elements of a flora.

But the means of comparison which we look for in vain in the published literature of science may be found in the collections of dried plants which botanists have formed for several generations. The local herbaria of our own day represent not only the different species found in a country, but the various forms which occur, together with their distribution. They must supply the most certain materials for the minute comparison at any future epoch of the then existing vegetation with that of our own day.

The preservation of dried plants as a help in the study of systematic botany was first employed in the middle of the sixteenth century. The earliest herbarium of which we have any record is that of John Falconer, an Englishman who travelled in Italy between 1540 and 1547, and who brought with him to England a collection of dried plants fastened in a book. This was seen by William Turner, our first British botanist, who refers to it in his "Herbal," published in 1551. Turner may have been already acquainted with this method of preserving plants, for in his enforced absence from England he studied at Bologna under Luca Ghini, the first Professor of Botany in Europe, who, there is reason to believe, originated the practice of making herbaria. Ghini's pupils, Aldrovandus and Cæsalpinus, formed extensive collections. Caspar Bauhin, whose "Prodromus" was the first attempt to digest the literature of botany, left a considerable herbarium, still preserved at Basle. No collection of English plants is known to exist older than the middle of the seventeenth century; a volume containing some British and many exotic plants collected in the year 1647 was some years ago acquired by the British Museum. Towards the end of that century great activity was manifested in the collection of plants, not only in our own country, but in every district of the globe visited by travellers. The labours of Ray and Sloane, of Petiver and Plukenet are manifest not only in the works which they published, but in the collections that they made, which were purchased by the country in 1759 when the museum of Sir Hans Sloane became the nucleus of the now extensive collections of the British Museum. The most important of these collections in regard to British plants is the herbarium of Adam Buddle, collected nearly 200 years ago, and containing an extensive series, which formed the basis of a

British flora, that unhappily for science was never published, though it still exists in manuscript. Other collections of British plants of the same age, but less complete, supplement those of Buddle: these various materials are in such a state of preservation as to permit of the most careful comparison with living plants, and they show that the two centuries which have elapsed since their collection have not modified in any particular the species contained in them. The early collectors contemplated merely the preservation of a single specimen of each species; consequently the data for an exhaustive comparison of the indigenous flora of Britain at the beginning of last century with that of the present are very imperfect as compared with those which we shall hand down to our successors for their use.

The collections made in other regions of the world in the seventeenth century, and included in the extensive herbarium of Sir Hans Sloane, are frequently being examined side by side with plants of our own day, but they do not show any peculiarities that distinguish them from recent collections. If any changes are taking place in plants, it is certain that the 300 years during which their dried remains have been preserved in herbaria have been too short to exhibit them.

Beyond the time of those early herbaria the materials which we owe in any way to the intervention of man have been preserved without any regard to their scientific interest. They consist mainly of materials used in building or for sepulture. The woods employed in mediæval buildings present no peculiarities by which they can be distinguished from existing woods; neither do the woods met with in Roman and British villages and burying-places. From a large series collected by General Pitt-Rivers in extensive explorations carried on by him on the site of a village which had been occupied by the British before and after the appearance of the Romans, we find that the woods chiefly used by them were oak, birch, hazel, and willow, and at the latter period of occupation of the village the wood of the Spanish chestnut (*Castanea vulgaris*, Lamk.) was so extensively employed that it must have been introduced and grown in the district. The gravel beds in the north of London, explored by Mr. W. G. Smith for the palæolithic implements in them, contained also fragments of willow and birch, and the rhizomes of *Osmunda regalis*, L.

The most important materials, however, for the comparison of former vegetation of a known age with that of our own day have been supplied by the specimens which have been obtained from the tombs of the ancient Egyptians. Until recently these consisted mainly of fruits and seeds. These were all more or less carbonised, because the former rifling of the tombs had exposed them to the air. Ehrenberg, who accompanied Von Minutoli in his Egyptian expedition, determined the seeds which he had collected; but as he himself doubted the antiquity of some of the materials on which he reported, the scientific value of his enumeration is destroyed. Passalacqua in 1823 made considerable collections from tombs at Thebes, and these were carefully examined and described by the distinguished botanist Kunth. He pointed out, in a paper published sixty years ago, that these ancient seeds possessed the minute and apparently accidental peculiarities of their existing representatives. Unger, who visited Egypt, published in several papers identifications of the plant remains from the tombs; and one of the latest labours of Alexander Braun was an examination of the vegetable remains in the Egyptian Museum at Berlin, which was published, afer his death, from his manuscript, under the careful editorship of Ascherson and Magnus. In this, twenty-four species were determined, some from imperfect materials, and necessarily with some hesitation as to the accuracy of their determination.

The recent exploration of unopened tombs belonging to an early period in the history of the Egyptian people has permitted the examination of the plants in a condition which could not have been anticipated. And happily, the examination of these materials has been made by a botanist who is thoroughly acquainted with the existing flora of Egypt, for Dr. Schweinfurth has for a quarter of a century been exploring the plants of the Nile valley. The plant-remains were included within the mummy-wrappings, and, being thus hermetically sealed, have been preserved with scarcely any change. By placing the plants in warm water, Dr. Schweinfurth has succeeded in preparing a series of specimens gathered 4000 years ago, which are as satisfactory for the purposes of science as any collected at the present day. These specimens consequently supply means for the closest examination and comparison with their living representatives. The colours of the flowers are still present, even the most evanescent, such as the violet of the larkspur and knapweed, and the scarlet of the poppy; the chlorophyll remains in the leaves, and the sugar in the pulp of the raisins. Dr. Schweinfurth has determined no less than fifty-nine species, some of which are represented by the fruits employed as offerings to the dead, others by the flowers and leaves made into garlands, and the remainder by branches on which the body was placed, and which were inclosed within the wrappings.

[The following is a list of the species of ancient Egyptian plants determined by Dr. Schweinfurth; I am indebted to Dr. Schweinfurth for some species in this list, the discovery of which he has not yet published:—*Delphinium orientale*, Gay; *Cocculus Leaba*, DC.; *Nymphæa cærulea*, Sav.; *Nymphæa Lotus*, Hook.; *Papaver Rhœas*, L.; *Sinapis arvensis*, L., var. *Allionii*, Jacq.; *Mærua uniflora*, Vahl.; *Oncoba spinosa*, Forsk.; *Tamarix nilotica*, Ehrb.; *Alcea ficifolia*, L.; *Linum humile*, Mill.; *Balanites ægyptiaca*, Del.; *Vitis vinifera*, L.; *Moringa aptera*, Gærtn.; *Medicago denticulata*, Willd.; *Sesbania ægyptiaca*, Pers.; *Faba vulgaris*, Mœnch; *Lens esculenta*, Mœnch; *Lathyrus sativus*, L.; *Cajanus indicus*, L.; *Acacia nilotica*, Del.; *Lawsonia inermis*, Lamk.; *Punica Granatum*, L.; *Epilobium hirsutum*, L.; *Lagenaria vulgaris*, Ser.; *Citrullus vulgaris*, Schrad., var. *colocynthoides*, Schweinf.; *Apium graveolens*, L.; *Coriandrum sativum*, L.; *Cenuana pratensis*, Forsk.; *Sphæranthus suaveolens*, DC.; *Chrysanthemum coronarium*, L.; *Centaurea depressa*, M. Bieb.; *Carthamus tinctorius*, L.; *Picris coronopifolia*, Asch.; *Mimusops Schimperi*, Hochst.; *Jasminum Sambac*, L.; *Olea europæa*, L.; *Mentha piperita*, L.; *Rumex dentatus*, L.; *Ficus Sycomorus*, L.; *Ficus Carica*, L.; *Salix Safsaf*, Forsk.; *Juniperus phœnicea*, L.; *Pinus Pinea*, L.; *Allium sativum*, L.; *Allium Cepa*, L.; *Phœnix dactylifera*, L.; *Calamus fasciculatus*, Roxb.; *Hyphæne thebaica*, Mart.; *Medemia Argun*, P. G. von Würtemb,; *Cyperus Papyrus*, L.; *Cyperus esculentus*, L.; *Andropogon laniger*, Desf.; *Leptochloa bipinnata*, Retz.; *Triticum vulgare*, L.; *Hordeum vulgare*, L.; *Parmelia furfuracea*, Ach.; *Usnea plicata*, Hoffm.]

The votive offerings consist of the fruits, seeds, or stems, of twenty-nine species of plants. Three palm fruits are common: the *Medemia Argun*, Würt., of the Nubian Desert, and the *Hyphæne thebaica*, Mart., of Upper Egypt, agreeing exactly with the fruits of these plants in our own day; also dates of different forms resembling exactly the varieties of dried dates found now in the markets of Egypt. Two figs are met with, *Ficus Carica*, L., and *Ficus Sycomorus*, L., the latter exhibiting the incisions still employed by the inhabitants for the destruction of the Neuropterous insects which feed on them. The sycamore was one of the sacred trees of Egypt, and the branches used for the bier of a mummy found at Abd-el-Qurna, of the twentieth dynasty (a thousand years before the Christian era), were moistened and laid out by Dr. Schweinfurth, equalling, he says, the best specimen of this plant in our herbaria, and consequently permitting the most exact comparison with living sycamores, from which they differ in no respect. The fruit of the vine is common, and presents, besides some forms familiar to the modern grower, others which have been lost to cultivation. The leaves which have been obtained entire exactly agree in form with those cultivated at the present day, but the under surface is clothed with white hairs, a peculiarity Dr. Schweinfurth has not observed in any Egyptian vines of our time. A very large quantity of linseed was found in a tomb at Thebes of the twentieth dynasty, now 3000 years old, and a smaller quantity in a vase in another tomb of the twelfth dynasty, that is, 1000 years older. This belongs certainly to *Linum humile*, Mill., the species still cultivated in Egypt, from which the capsules do not differ in any respect. Braun had already determined this species preserved thus in the tombs, though he was not aware of its continued cultivation in Egypt. The berries of *Juniperus phœnicea*, L., are found in a perfect state of preservation, and present a somewhat larger average size than those obtained from this juniper at the present day. Grains of barley and wheat are of frequent occurrence in the tombs; M. Mariette has found barley in a grave at Sakhara of the fifth dynasty, 5400 years old.

The impurities found with the seeds of these cultivated plants show that the weeds which trouble the tillers of the soil at the present day in Egypt were equally the pests of their ancestors in those early ages. The barley-fields were infested with the same spiny medick (*Medicago denticulata*, Willd.) which is still found in the grain crops of Egypt. The presence of the pods

of *Sinapis arvensis*, L., among the flax seed testifies to the presence of this weed in the flax crops of the days of Pharaoh, as of our own time. There is not a single field of flax in Egypt where this charlock does not abound, and often in such quantity that its yellow flowers, just before the flax comes into bloom, present the appearance of a crop of mustard. The charlock is *Sinapis arvensis*, L., var. *Allionii*, Jacq., and is distinguished from the ordinary form by its globular and inflated silicules, which are as characteristically present in the ancient specimens from the tombs as in the living plants. *Rumex dentatus*, L., the dock of the Egyptian fields of to-day, has been found in graves of the Greek period at Dra-Abu-Negga.

It is difficult without the actual inspection of the specimens of plants employed as garlands, which have been prepared by Dr. Schweinfurth, to realise the wonderful condition of preservation in which they are. The colour of the petals of *Papaver Rhœas*, L., and the occasional presence of the dark patch at their bases present the same peculiarities as are still found in this species growing in Egyptian fields. The petals of the larkspur (*Delphinium orientale*, Gay) not only retain their reddish-violet colour, but present the peculiar markings which are still found in the living plant. A garland composed of wild celery (*Apium graveolens*, L.) and small flowers of the blue lotus (*Nymphæa cærulea*, Sav.), fastened together by fibres of papyrus, was found on a mummy of the twentieth dynasty, about three thousand years old. The leaves, flowers, and fruits of the wild celery have been examined with the greatest care by Dr. Schweinfurth, who has demonstrated in the clearest manner their absolute identity with the indigenous form of this species now abundant in moist places in Egypt. The same may be said of the other plants used for garlands, including two species of lichens.

It appears to have been a practice to lay out the dead bodies on a bier of fresh branches, and these were inclosed within the linen wrappings which enveloped the mummy. In this way there have been preserved branches of considerable size of *Ficus Sycomorus*, L., *Olea europæa*, L., *Mimusops Schimperi*, H., and *Tamarix nilotica*, Ehrb. The Mimusops is of frequent occurrence in the mural decorations of the ancient temples; its fruit had been detected amongst the offerings to the dead, and detached leaves had been found made up into garlands, but the discovery of branches with their leaves still attached, and in one case with the fruit adhering, has established that this plant is the Abyssinian species to which Schimper's name has been given, and which is characterised by the long and slender petiole of the leaf.

In none of the species, except the vine to which I have referred, which Dr. Schweinfurth has discovered, and of which he has made a careful study, has he been able to detect any peculiarities in the living plants which are absent in those obtained from the tombs.

Before passing from these Egyptian plants I would draw attention to the quality of the cereals. They are good specimens of the cereals still cultivated. This observation is true also of the cultivated grains which I have examined, belonging to prehistoric times. The wheat found in the purely British portion of the ancient village explored by General Pitt-Rivers is equal to the average of wheat cultivated at the present day. This is the more remarkable, because the two samples from the later Romano-British period obtained by General Pitt-Rivers are very much smaller, though they are not unlike the small hard grains of wheat still cultivated on thin chalk soils. The wheat from lake-dwellings in Switzerland, for which I am indebted to Mr. J. T. Lee, F.G.S., are fair samples. My colleague, Mr. W. Fawcett, has recently brought me, from America, grains of maize from the prehistoric mounds in the valley of the Mississippi, and from the tombs of the Incas of Peru, which represent also fair samples of this great food substance of the New World. The early peoples of both worlds had then under cultivation productive varieties of these important food-plants, and it is remarkable that in our own country, with all the appliances of scientific cultivation and intelligent farming, we have not been able to appreciably surpass the grains which were harvested by our rude ancestors of 2000 years ago.

In taking a further step into the past, and tracing the remains of existing species of plants preserved in the strata of the earth's crust, we must necessarily leave behind all certain chronology. Without an intelligent observer and recorder there can be no definite determination of time. We can only speculate as to the period required for effecting the changes represented by the various deposits.

The peat-bogs are composed entirely of plant-remains belonging to the floras existing in the regions where they occur. They are mainly surface-accumulations still being formed and going back to an unknown antiquity. They are subsequent to the last changes in the surface of the country, and represent the physical conditions still prevailing.

The period of great cold during which Arctic ice extended far into temperate regions was not favourable to vegetable life. But in some localities we have stratified clays with plant-remains later than the Glacial epoch, yet indicating that the great cold had not then entirely disappeared. In the lacustrine beds at Holderness is found a small birch (*Betula nana*, L.), now limited in Great Britain to some of the mountains of Scotland, but found in the Arctic regions of the Old and New World and in Alpine districts in Europe, and with it *Prunus Padus*, L., *Quercus Robur*, L., *Corylus Avellana*, L., *Alnus glutinosa*, L., and *Pinus sylvestris*, L. In the white clay beds at Bovey Tracey of the same age there occur the leaves of *Arctostaphylos Uva-Ursi*, L., three species of willow, viz. *Salix cinerea*, L., *S. myrtilloides*, L., and *S. polaris*, Wahl., and in addition to our alpine *Betula nana*, L., the more familiar *B. alba*, L. In beds of the same age in Sweden, Nathorst has found the leaves of *Dryas octopetala*, L., and *Salix herbacea*, L., this being associated with *S. polaris*, Wahl. Two of these plants have been lost to our flora from the change of climate that has taken place, viz. *Salix myrtilloides*, L., and *S. polaris*, Wahl.; and *Betula nana*, L., has retreated to the mountains of Scotland. Three others (*Dryas octopetala*, L., *Arctostaphylos Uva-Ursi*, L., and *Salix herbacea*, L.), have withdrawn to the mountains of Northern England, Wales, and Scotland, while the remainder are still found scattered over the country. Notwithstanding the diverse physical conditions to which these plants have been subjected, the remains preserved in these beds present no characters by which they can be distinguished from the living representatives of the species.

We meet with no further materials for careful comparison with existing species until we get beyond the great period of intense cold which immediately preceded the present order of things. The Glacial epoch includes four periods during which the cold was intense, separated by intervals of somewhat higher temperature, which are represented by the intervening sedimentary deposits. During these alterations of temperature, extensive changes in the configuration of the land were taking place. The first great upheaval occurred in the early Glacial period, and was followed by a considerable subsidence. A second upheaval took place late in the Glacial epoch. Various estimates have been formed of the time required for this succession of climatic conditions and earth-movements. The moderate computation of Ramsay and Lyell gives to the boulder-clay of the first Glacial period an age of 250,000 years, estimating the time of the first upheaval as 200,000 years ago, while the subsidence took place 50,000 years later, and the second upheaval 92,000 years ago.

The sedimentary deposits later than the Pliocene strata, but older than the Glacial drift, indicate an increasing severity in the climate, which reached its height in the first Glacial period.

At Cromer, on the Norfolk coast, the newest of these deposits has supplied the remains of *Salix polaris*, Wahl., *S. cinerea*, L., and *Hypnum turgescens*, Schimp. This small group of plants is of great interest in connection with the history of existing species; their remains are preserved in such a manner as to permit the closest comparison with living plants. Such an examination shows that they differ from each other in no particular. In the post-Glacial deposits in Sweden, *Salix herbacea*, L., is associated with *S. polaris*, Wahl., as I have already stated. These two willows are very closely related, having indeed been treated as the same species until Wahlenberg pointed out the characters which separated them when he established *Salix polaris* as a distinct species in 1812. One of the most obvious of the specific distinctions is the form and venation of the leaf, a character which is, however, easily overlooked, but when once detected is found to be so constant that it enables one to distinguish without hesitation the one species from the other. The leaves of the two willows in the Swedish bed present all the peculiarities which they possess at the present day, and the venation and form of the leaves of *S. polaris*, Wahl., from the pre-Glacial beds of Cromer, present no approach towards the peculiarities of its ally *S. herbacea*, L., but exhibit them exactly as they appear in the living plant. This is the more noteworthy as the vegetative organs supply, as a rule, the least stable of the

characters employed in the diagnosis of species. The single moss (*Hypnum turgescens*, Schimp.) is no longer included in the British flora, but is still found as an Arctic and Alpine species in Europe, and the pre-Glacial specimens of this cellular plant differ in no respect from their living representatives.

The older beds containing the remains of existing species, which are found also at Cromer, have recently been explored with unwearied diligence and great success by Mr. Clement Reid, F.G.S., an officer of the Geological Survey of England. To him I am indebted for the opportunity of examining the specimens which he has found, and I have been able to assist him in some of his determinations, and to accept all of them. His collections contain sixty-one species of plants belonging to forty-six different genera, and of these forty-seven species have been identified. Slabs of clay-ironstone from the beach at Happisburgh contain leaves of beech, elm, oak, and willow. The materials, however, which have enabled Mr. Reid to record so large a number of species are the fruits or seeds which occur chiefly in mud or clay, or in the peat of the forest bed itself. The species consist mainly of water or marsh plants, and represent a somewhat colder temperature than we have in our own day, belonging as they do to the Arctic facies of our existing flora.

Only one species (*Trapa natans*, L.), has disappeared from our islands; its fruits, which Mr. Reid found abundantly in one locality, agree with those of the plants found until recently in the lakes of Sweden. Four species (*Prunus spinosa*, L., *Œnanthe Lachenalii*, Gmel., *Potamogeton heterophyllus*, Schreb., and *Pinus Abies*, L.) are found at present only in Europe, and a fifth (*Potamogeton trichoides*, Cham.) extends also to North America; two species (*Peucedanum palustre*, Mœnch, and *Pinus sylvestris*, L.) are found also in Siberia, whilst six more (*Sanguisorba officinalis*, L., *Rubus fruticosus*, L., *Cornus sanguinea*, L., *Euphorbia amygdaloides*, L., *Quercus Robur*, L., and *Potamogeton crispus*, L.) extend into Western Asia, and two (*Fagus sylvatica*, L., and *Alnus glutinosa*, L.) are included in the Japanese flora. Seven species, while found with the others, enter also into the Mediterranean flora, extending to North Africa: these are *Thalictrum minus*, L., *Thalictrum flavum*, L., *Ranunculus repens*, L., *Stellaria aquatica*, Scop., *Corylus Avellana*, L., *Zannichellia palustris*, L., and *Cladium Mariscu.*, Br. With a similar distribution in the Old World, eight species (*Bidens tripartita*, L., *Myosotis cæspitosa*, Schultz, *Suæda maritima*, Dum., *Ceratophyllum demersum*, L., *Sparganium ramosum*, Huds., *Potamogeton pectinatus*, L., *Carex paludosa*, Good., and *Osmunda regalis*, L., are found also in North America. Of the remainder, ten species (*Nuphar luteum*, Sm., *Menyanthes trifoliata*, L., *Stachys palustris*, L., *Rumex maritimus*, L., *Rumex Acetosella*, L., *Betula alba*, L., *Scirpus pauciflorus*, Lightf., *Taxus baccata*, L., and *Isoetes lacustris*, L.-extend round the north temperate zone, while three (*Lycopus europæus*, L., *Alisma Plantago*, L., and *Phragmites communi*,) Trin.), having the same distribution in the north, are found also in Australia, and one (*Hippuris vulgaris*, L.) in the south of South America. The list is completed by *Ranunculus aquatilis*, L., distributed over all the temperate regions of the globe, and *Scirpus lacustris*, L., which is found in many tropical regions as well.

The various physical conditions which necessarily affected these species in their diffusion over such large areas of the earth's surface in the course of, say, 250,000 years, should have led to the production of many varieties, but the uniform testimony of the remains of this considerable pre-Glacial flora, as far as the materials admit of a comparison, is that no appreciable change has taken place.

I am unable to carry the history of any existing species of plant beyond the Cromer deposits. Some of the plant-remains from Tertiary strata have been referred to still living species, but the examination of the materials, as far as they have come before me, convinces me that this has been done without sufficient evidence. The physical conditions existing during even the older of the Tertiary periods were not suitable to a flora fitted to persist in these lands in our day, even if the period of great cold had not intervened to destroy them. And in no warmer region of the earth do these Tertiary species now exist, though floras of the same facies occur, containing closely allied species. The sedimentary beds at the base of the Glacial epoch contain, as far as we at present know, the earliest remains of any existing species of plant.

It is not my purpose to point out the bearing of these facts on any theoretical views entertained at the present day: I wish merely to place them before the members of this Section as data which must be taken into account in constructing such theories, and as confirming the long-established axiom that by us, at least, as workers, species must be dealt with as fixed quantities.

SECTION H

ANTHROPOLOGY

OPENING ADDRESS BY SIR GEORGE CAMPBELL, K.C.S.I., M.P., D.C.L., F.R.G.S., PRESIDENT OF THE SECTION.

I FEEL much diffidence in taking this chair, for, though in former days I used to pay a good deal of attention to what was then called ethnology, I have been for many years immersed in perhaps more exciting but, I am afraid, less satisfactory occupations; and I feel that I am very far behind in scientific knowledge and scientific methods. I only venture to address you because I take for my subject practical, rather than scientific, anthropology; the study and cultivation of the creature man as he exists, rather than that branch of the subject which seeks to inquire into his origin and development. Intensely interesting as are inquiries into the origin of man, it must be admitted that our knowledge on the subject is still very limited and our progress slow; that we have not yet got hold of the missing link, and scarcely know whether the flint implements are the work of man or of some earlier intelligent creature. We are hardly on firm ground till we come to man very much in the form in which we now have him, and even already divided into the principal varieties which exist to this day. I now then invite you to approach the subject rather as practical agriculturists deal with the subject of horses and cattle than as scientists who trace these animals to very ancient prehistoric types; and in dealing with man from this point of view I am emboldened by the consideration that here also science has not yet completely conquered the field, and that very much is open to those who bring to it only a quick eye and careful observation. I think it can hardly be doubted that, in distinguishing well-marked types of humanity, the eye is after all the easiest and perhaps the safest guide. With that alone one can recognise the unmistakable differences of colour, size, facial features, set of the eye, character of the hair, and one or two other features by which the physical characters of different types of humanity are varied. On the other hand, when we come to nicer and more subtile distinctions, especially among the mixed races which occupy most of the world, we must confess that anthropometric science as applied to craniology, &c., gives us results only partially conclusive. I have an unusually narrow head. I can hardly be fitted with a hat without making the hatter elongate it; my next brother has so remarkably broad a head that he cannot be fitted without altering a large hat the other way: and so I think it is in many families and races, as any one who tries to puzzle out craniological results will find.

So again as regards other guides to race. It is admitted that language is not always a safe guide, but still it is a very important element in ethnological inquiries, especially among primitive races. I have paid some attention to that, and my impression is strong that language tests of race are to be found in the few simple elementary words and forms which any observer can easily master and examine, and not in the higher developments of the language, which are generally much intermixed with and influenced by foreign elements. I roughly put together a few dozen test words, &c., which we found very efficacious in India. Take English, too: the origin of the race is found in the lower and monosyllabic words, though the majority of the English words in a dictionary are Latin and French.

There is another race-guide which requires much care and some scientific accuracy, though not of what we should call a properly anthropometric character—I mean laws, customs, and habits. Like language these too may be varied by foreign influences, but I incline to think that they are more important for our purposes than has always been recognised, and are at least as persistent as, perhaps more persistent than, language. At any rate, they are certainly most important as affecting the modern history and cultivation of man; and while some laws and customs require scientific study, many habits and practices are on the surface, and open to the observation of every intelligent observer. I might class food and drink among such habits, as being those which bear most directly of all on physical

development. For instance, one scarcely realises till one goes to China how important is the cow as pre-eminently an Aryan animal, the early sacredness of which was founded upon uses almost ignored by other great races, such as the Chinese. The Chinese, again, who will not touch milk, and reject some other food which we think among the best (pheasants, for instance), make constant and large use of food which we reject, such as puppies and rats. It is most interesting to inquire whether there is any foundation for either class of prejudices.

Among other habits and institutions well worthy of observation I might cite marriage and the family descent, through the female or through the male, the forms of small self-governing communities, and the tenure of land. Animals of nearly allied species seem to be divided by curiously sharp lines into polygamous and monogamous races. It is hard to understand why hares should be strictly monogamous, rabbits polygamous, partridges monogamous, pheasants polygamous, geese monogamous, ducks polygamous. We have yet to discover to which class man belonged before laws divided the race into two opposite camps in this matter. When we come to institutions and land tenure we approach the region of politics, but for my part I must at once say that, if we avoid mere party in politics, we anthropologists are called on to perform most important functions in the social politics of the day. What can be more important than to ascertain the effect on the race of modern urban life, of the increased use of meat, of the diminished use of milk, of the enormously increased consumption of tea, of the more constant use of the eyes and the brain, viewing these subjects in their broad general results, rather than from a merely medical point of view?

My view of the good work that may be done by the more popular methods in anthropology may be somewhat consoling to our countrymen generally, for they seem as a whole to be too busy for much science, and to be deficient in it. I see it was stated that we have to get anthropometric instruments from abroad. But on the other hand our opportunies for observation far outrival all others. In our vast Empire we have every race, and every shade, every stage of progress, from the lowest to the highest; every institution and every method of living. As rulers, as explorers, as merchants, as employers of labour, as colonists, we come into the nearest contact, and have the most intimate relations with almost every people and every tribe on the face of the earth. We are indeed a people who, if we make but the most moderate use of opportunities, may bring together such a mass of knowledge of mankind as to leave nothing wanting. Surely then in this country anthropology is no mean subject.

Both in regard to the greatness of our dominion, the vastness of the population, and its infinite variety, India is by far the greatest of our fields, as it is that in which we have the most complete and effective official machinery. India is remarkable not only for its many countries, climates, and races, but also for the division of the populations into what one may call horizontal strata. There, under the caste system, every rank, occupation, and profession represents in some sort a race, and that in enormous variety. Whatever infiltration of blood there may be, every caste in India is at least as much a peculiar and separate race as were till lately Jews or gipsies in our own country, and more so. Every one of them has, too, its own institutions, its own rules of marriage and inheritance, its own laws and customs; and I need scarcely add that outside this Hindoo agglomeration of many races there are the various aboriginal races—also in great variety, and in a state of excellent preservation—tribes not of one family of the human race, but of almost every great family, from the purest Aryans of the north-west to what I may call extreme Mongolians in the east, and primitive blacks in the centre and south.

In truth, my experience of that great anthropological field India is my excuse for sitting here to-day. It has been my fortune to serve in very many parts of that great country, and, so far as my scanty acquirements permit, I have always taken great interest in, and inquired much about, the races and varieties of the peoples; and I think I may claim this, too, that ever since I have been a good deal absorbed in politics, in all the travels I have made in several parts of the world, in Eastern Europe, in America, and elsewhere, I have never wholly forgotten my ethnological proclivities, and have pried about a good deal to pick up information regarding the various races and tribes.

As India is in some sense an epitome of the world, so I may also say that the last provinces I administered, those forming the Government of Bengal, are or were an epitome of India. At that time the whole of Assam and the eastern frontiers were under Bengal, and we certainly had a very much greater variety of races than any other province in India—perhaps I may say than any other country in the world. Among the more advanced races, besides the whole of the well-marked Bengalee nationality we had some twenty millions of Hindustanis on the north, the Ooryahs on the west, and the Assamese on the east; then of the Indian aboriginal races, while in other provinces they have but scanty hill tribes, counted by thousands, we have in the western districts of Bengal may millions of these aborigines, settled, comparatively civilised, a fecund, colonising, and migratory people; we have them in endless variety of both the great aboriginal families, the Dravidian and that now generally known as the Kolarian. Partly in the Central Provinces and partly in Bengal, it has indeed been my lot to administer the whole of what I may call "aboriginal India." I may here mention that the several aboriginal Dravidian tribes of this tract speak languages clearly Dravidian in their roots, and yet for the rest so distant from the cultivated Dravidian languages that the common origin must be very ancient indeed. But no one who sees these people can doubt their non-Caucasian character; and that may go far to settle the question whether the Dravidians of the Peninsula are of Caucasian origin, or non-Caucasians overlaid by an Aryan over-crust.

Again, on the north and east we have some forest tribes which may or may not be related to the aborigines of the interior of India. But as soon as we get into the hill country we meet with every form of what may be called the Indo-Chinese type—all the way from the frontiers of Nepal on the north, along the Eastern Himalayas, round both sides of Assam, and then on to Maneepore, the Chittagong hills, and the Burmese country. Here and there in this great extent of country we have many unclassed races with peculiar languages and institutions of their own—some very savage, others far advanced in civilisation. Among the latter I might mention, for instance, the Khassyahs, a very peculiar people with highly developed constitutional and elective forms of government, and also specially interesting as exhibiting far the best specimen of which I have anywhere heard of the matriarchal, or perhaps I should rather say matri-herital, system fully carried out under recognised and well-defined law among a civilised people. The result of observation of the Khassyahs has been to separate in my mind the two ideas of matri-heritage and polyandry, and to suggest that polyandry is really only a local accident, the result of scarcity of women; as, for instance, in some parts of the Himalayas, where the hill women are in great demand in the adjoining plains, and the hill men are obliged to be content with a reduced number of women. Among the Khassyahs, on the other hand, there is no polyandry (so far as I have been able to learn) though there is great facility for divorce; and heritage through the female becomes quite intelligible, I may say natural, when we see that the females do not leave the maternal home and family and join any other family, as do the Aryans. They are the stock-in-trade of the family, the queen bees as it were; they take to themselves husbands—only one at a time, and if he is divorced they may take another—but the husband is a mere outsider belonging to another family. The property of the woman goes in the woman's family, the property of the man in his own maternal family. It should be added, however, that in these maternal families, though the heritage comes through the female, the males rule, as they ought to in all well-ordered communities.

When I administered the Government of Bengal I did the best I could to obtain a classification of our many races, and a comparison of the languages brought together under my system of test words, and officially published in a large volume. We owe to the unrivalled experience of the late General Dalton a mass of information regarding the western aboriginal tribes, comprised in his great ethnological volume and many other publications; and more recently that very distinguished Indian officer, Mr. A. Mackenzie, partly a Scotchman and partly a Birmingham man, has brought together in his "North-East Frontier of Bengal" a full and most interesting account of the eastern tribes. Now I am happy to say that one of my old fellow-workers in Bengal, who at present most worthily and well administers the government of that province, has undertaken, through Mr. Risley, a much greater work than any of us have yet attempted, viz. a general survey of the whole people, not only as regards their physical characteristics and languages, &c., but also (and this is the newest and most important part of the undertaking) as regards their institutions, laws, and social rules.

It is hoped that, by obtaining accurate information of this kind regarding the many races, tribes, and castes of these great provinces, a flood of light may be thrown on the social history of the human race. It is a very great undertaking, but successfully carried out must have very great results. I can conceive nothing more important and interesting, and only hope that something of the kind may be attempted for India as a whole. Some of the most important castes, the Brahmins for instance, are so widely spread that we can hardly realise their position without extending the survey over India. In Bengal I think they are little agricultural, while in some provinces they are among the best of the agriculturists.

I could well wish that we had systematic inquiries of this kind nearer home. Europe is almost as good an anthropological field as India, and in our islands there is still very much room for investigation. In my own country of Scotland, after much asking, I have never been able to get any information who the Aberdonians are, and what is the language they speak, so different in its forms and intonations from the rest of Scotland. In England some most interesting maps might be made if it were only to trace the letter *h*, showing where it begins and where it ends. I have a belief that though languages may be changed and cease to indicate races, there is a great racial persistency in the letter *h* or the absence of it. The Scotch and the Irish have adopted the English language, but no Scotchman or Irishman was ever in the smallest degree wanting in aspirates—an Englishman might perhaps call them hyper-aspirators. The greater part of England, on the contrary, is equally persistent in the dropping of *h*'s. The whole subject is most interesting, not only in regard to the use or omission of the *h* by various races, but also on account of the very singular—I may say phenomenal—tendency of so many of the English neither to maintain nor to abandon the *h*, but simply to reverse the written language, omitting the *h* where it is written, and putting it in where it is not, in a peculiarly aggressive manner. It has been noticed, with truth, that we seem legitimately to drop the *h* in almost all words that come direct from the Latin, as "hour," "heir," "honour," yet in the Latin we pronounce the *h* fully. Is the spoken language the true tradition? Can it be that, while the Greeks spoke in aspirates which they did not write, the Romans clipped those which they did write, and that the modern Englishman combines the practice of these two famous races? Or is there any foundation for what I can call no more than a conjecture, viz. that the real English is that spoken by the Scotch, and that the corruption of the *h*'s is French brought in by the Normans? If a language map showed the clipping of *h*'s to be coincident with large Norman settlements, that might be so. Perhaps a few hundred years ago it was the aristocratic thing to clip the *h*'s, and the fashion may have gradually gone to the lower classes like the swallow-tailed coat worn by the typical Irish peasant, while the upper classes have been partially reformed back to true English by contact with the Scotch—only partially though, for they still say "wēn" and "wāle" instead of "when" and "whale," to say nothing of "idear" and "Indiar."

This, however, is a digression. I am afraid I have been long in coming to the main object of this address, viz. to recommend the systematic and scientific cultivation of man—what I may call "homi-culture," in the same sense as "oyster-culture," "bee-culture," or "cattle-culture"—and that with a view both to physical and mental qualities. It seems very sad indeed, that, when so much has been done to improve and develop dogs, cattle, oysters, cabbages, nothing whatever has been done for man, and he is left very much where he was when we have the first authentic records of him. Knowledge, education, arts, he has no doubt acquired; but there seems to be no reason to suppose that the individual man is physically or mentally a superior creature to what he was five thousand years ago. We are not sure that under very modern influences he may not retrograde. No one doubts that, by careful selection and cultivation, cattle, vegetables, and many other things have been immensely improved. In regard to animals and plants we have very largely mastered the principles of heredity and culture, and the modes by which good qualities may be maximised, bad qualities minimised. Why should not man be similarly improved? It is true that the mind has a larger share in that which constitutes a man; but after all this is only a question of degree—the cultivation of the mind *does* enter very largely into animal-culture. I apprehend there is no doubt that the superiority for our purposes of shorthorns, black-polled, and other famous breeds of cattle is very largely due to placid and well-regulated minds, which enable them to take calmly a short and happy life, and to assimilate their food, differing in this very much from their restless and often vicious ancestors. Surely, then, if we only had the requisite knowledge, and, taking a practical view of life, could regulate our domestic arrangements with some degree of reason, rather than by habit, prejudice, and the foolish ideas cultivated by foolish novelists, man too might be greatly improved.

It may be admitted that we are not in a position to begin confident man-culture at once. Much study is first required and much knowledge must be accumulated before we can be confident in practice. The first thing that most strikes us in man, as compared with all domesticated and even most widely-spread wild animals, is the extremely small variation in man all over the globe. There are differences which seem large to us, but are extremely small from a more enlarged point of view. How enormous are the differences between different breeds of dogs, horses, and cattle! When we come to man the difference of which we make most is that of colour—a feature which we think quite trivial in animals. Who thinks very much more highly of a white than of a black cow, of a grey horse than of a black one? Our skilled eyes recognise variations of human feature, but they are so slight that the inhabitant of another planet would see no more difference than in the countenances of a flock of sheep. In size, compared to other animals, the differences are but slight. Probably there is no race whose average height really approaches six feet, and I doubt if any are on the average so small as five feet. In other physical features there are no considerable differences of formation whatever. Then as regards the mind we have yet to learn that there are very wide differences of mental capacity between different races. Very likely—probably, I may say—there are considerable variations, but they are not so wide as to be apparent without careful and accurate study. With the superficial knowledge we have, no one can say that Europeans, Hindus, Chinese, are born with brains superior or inferior to the other; and even in regard to the negro I do not know that it is yet shown that with equal advantages Negro babies might not grow up nearly or quite as intelligent as Europeans. I do not say that it is so, but only that the question has not yet been sufficiently worked out. The difference is not so radical as to be self-evident from the first. Still, such experience as we have and the analogies derived from domesticated animals both tend to the belief that there *are* considerable, if not excessive, variations in the qualities and capacities of different races of men.

It seems to me, then, that the first object to which observation and experiment should be directed is to ascertain how far the qualities which distinguish different races, peoples, castes, and families are congenital and hereditary, and how far the result of education and surroundings. The distinguished President of the Anthropological Institute, Mr. F. Galton, has done much to make a beginning of the study of hereditary qualities in man, but there is still much to be done. To begin with very rudimentary facts, we hardly know whether courage in man and absence of courage in women are natural or artificial qualities; whether right-handedness is natural or a very ancient fashion. Coming nearer to modern variations we do not know how far energy, enterprise, constructive power, and all the rest of it are qualities appertaining to particular breeds, like the qualities of pointers or greyhounds; or whether they are more the result of education and surroundings. What is the effect on mind or body of vegetable and animal food respectively, and of the use of one stimulant and another? Why do particular races affect particular stimulants? Why is the Northern European more especially given to spirits, and the Chinese and Indo-Chinese races to opium? Is there anything in the breed that enables Britishers to rule over Hindus, or is it only education? Why has a Chinaman some virtues which an Irishman has not, and *vice versâ*? All through, the most important inquiry is to sift out those qualities in regard to which we must look to improvement in the breed, and those which more depend on education, so that power may not be wasted by efforts in the wrong direction—by breeding for qualities which already exist, or educating where the breed renders a particular education hopeless.

We must try to learn the direction in which we are to work first, and then the methods by which we may effect improvements in the ascertained direction—whether it be in the direction of breed or in that of education.

Now to come to the practical modes by which effect might be given to some such ideas as I have ventured to suggest.

To begin at the beginning, I think that, while so much effort and so much science have been expended, perhaps not very fructuously, in inquiries into the origin of man, too little systematic attention has been given to the radical differences between the modern man and modern animals. For instance, in the matter of speech no one can doubt that dogs and elephants and seals understand a great deal of language. One cannot see the individuals of a pack of hounds answer to their names without being satisfied that they not only attach a meaning to a few rude sounds, but can distinguish niceties and refinements of language. Again, we know that parrots and other creatures can speak our language; but I have never seen the question whether any one creature can both speak and understand thoroughly worked out. Has it been carefully and thoroughly ascertained whether any animals really cry or laugh? Sir John Lubbock and others have given attention to the question whether, in habitation-building, and the like, bees and ants exercise an intelligent discretion or follow one unvarying hereditary instinct; but I do not think any distinct conclusion has been arrived at. Can any monkey or other creature be educated up to the point of putting sticks on a fire and cooking chestnuts? I am afraid that on all these subjects there has been nothing but very desultory individual effort.

Then as regards man-breeding. Probably we have enough physiological knowledge to effect a vast improvement in the pairing of individuals of the same or allied races if we could only apply that knowledge to make fitting marriages, instead of giving way to foolish ideas about love and the tastes of young people, whom we can hardly trust to choose their own bonnets, much less to choose in a graver matter in which they are most likely to be influenced by frivolous prejudices. As I am not preaching I need say no more on that—all that I could say is self-evident. But when we come to the very important question of the crossing of races there is very great need of scientific observation and experiment. Both the general knowledge that we have of humans and the analogy of animals tends to show the great benefit of the crossing of breeds. Anglo-Saxon is an awkward term. I do not stop to inquire whether it represents two races; whether the peasant of the Lothians is an Englishman and the peasant of the south of England a Saxon, or why one is superior to the other; but using the word English for the Teutonic inhabitants of these islands I think one can hardly doubt that the English breed crossed with a dash of Celtic blood produces a better animal than either of the parent races. Witness the people of many parts of Scotland, of Ulster, and, I believe I may also say, of Cornwall. It is the use of the Celtic blood as an alloy that makes me specially unwilling to see Highlanders, and even wild Irishmen, exterminated from these islands. It may be worse for all of us if that comes to pass.

There is a popular belief that the cross between an Englishman and a Hindu produces a race inferior to either. I very much doubt the fact. Owing to the caste system (and it prevails with us almost as much as with the Hindus) half-castes are placed at a very great disadvantage, but I doubt if they are naturally inferior; at any rate, the question requires to be worked out. I think we have the means of doing so if we systematically went about it. So again as regards the cross-breeds between whites and Negroes. There is so much prejudice on the subject in the United States that it is very difficult to arrive at the truth. Some people think that the stimulating climate tends to make the white race in America wear itself out, and that (apart from the present great immigration from Europe) it would be a real improvement to the American race if the whites were crossed with the more phlegmatic blacks, say, in the porportion of six or eight of white to one of black, which now exists in the States. However, that is their affair, but a very important question for them.

And this brings me to the effect of climate. Is it the fact that in course of generations settled in America the climate alters the British race—or perhaps I should say European races? What is the tendency of the very peculiar Australian climate? It has passed into a popular proverb that the European race cannot survive in India beyond the second or third generation; and the result of that belief has been of enormous practical importance, for no sort of colonisation has been attempted. Yet I wholly doubt if the belief can be supported by any facts whatever; it is one of those things that are universally believed because they have never been tried, and therefore cannot be contradicted. Till little more than fifty years ago Europeans were not allowed to settle in India. To this day opportunities for education and good up-bringing are very much wanting—the surroundings are most unfavourable to European children; yet a good many instances could now be quoted of Europeans brought up in India who are physically just as good as their parents. The mortality in the European orphan asylums is extraordinarily low. It is not at all certain that the race might not be adapted to the climate, especially as the cool hill regions are those least occupied by the natives, and most fit for many lucrative industries introduced by Europeans.

Coming to physical and mental education, I have already alluded to some of the subjects which urgently require attention, the most important of which is, I think, the effect of what we call civilised life, and especially urban life. It is impossible to see the crowded and inferior dwellings in which so vast a population live in towns, without room for the gardens which their fathers had, and without the space and recreations natural to man, and not to fear for the result on the race. I might also say more on the question of physical education and on that of a mental education so general as to leave no mere primitive jungle plants as a stock on which to graft improved varieties; a subject which is already engaging anxious attention. On many other questions to which I have briefly alluded I might enlarge, but I have detained you so long that I think you would prefer to get to business; and so I will conclude by recommending practical anthropology to your earnest attention.

NOTES

We have received the programme of the Finsbury Technical College for the session 1886-87. There is no change in the curriculum calling for remark.

On September 3 a banquet took place in Bologna to celebrate the 100th anniversary of the discovery of animal electricity in that city by Galvani.

The *Bund* announces that Prof. Forel, of Morges, in the Canton of Vaud, has discovered a natural gallery which goes right across the lower portion of the glacier of Arolla, in the Eringerthal, in the Valais. It constitutes a natural grotto in the heart of the glacier, and was explored to a distance of 250 metres (273 yards) by the Professor and some fellow-members of the Swiss Alpine Club from Geneva, Neuchatel, and the Canton of Vaud. The average width was from 6 to 10 metres, broadening out here and there to fully 25 metres; the height varied from 2 to 3 metres. At the spot where the party stopped, the cavern divided into two galleries, the exploration of which they reserved for another time. The glacier was found to rest direct on the ground.

According to a Kimberley journal, Dr. Holub's exploration party is making but little progress. The whole of the party had been down with the fever, which has been severe this year in the Zambezi region.

The captain of the steamer *Ardangorm* officially reports at Malta that at 1 p.m. on August 30, in clear, calm weather, when about 14 miles north of Galita, a small island between Sardinia and Tunis, he noticed that the eastern and highest peak of the island appeared to be in eruption, while smoke resembling that ascending from Mount Etna was ejected at intervals from the crater.

The *Pioneer* of Allahabad states that news dated last April has been received from Mr. Carey, who is travelling from Leh towards China. He was then at Lob Nor. His course from Leh was south-eastward into Western Tibet, and then due north to Khotan, whence he made the Tarim River. After an excursion northwards towards the Baba Kul Lake he returned to the Tarim River, and followed it to Lob Nor. He is said to have probably entered Northern China before now.

One of the projects formed by M. Paul Bert before leaving France as Resident-General in Tonquin and Annam, was the

formation of an Academy similar to that planned by Napoleon I. for Egypt. M. Bert has now issued a decree establishing the "Tonquinese Academy." The preamble sets out that it is desirable to revive in the country, which has been disturbed for so long a time, the taste for literature and science, and to preserve to the people the vestiges of its glorious past; as well as to collect the scattered evidences of its ancient splendour. The decree then goes on to provide that the seat of the Academy shall be at Hanoi, and that its functions shall be to investigate and collect everything of interest, from any point of view, relating to Tonquin, to preserve ancient monuments, to initiate the people into the knowledge of modern sciences and civilisation, by translating and publishing in the Annamite language *résumés* of European works, to translate into French extracts from the more important dynastic annals of Tonquin, as well as other works to be selected by a Commission, to aid in forming public libraries in the principal towns, and a national library at Hanoi, to publish a monthly bulletin in which scientific and other questions shall be treated, and to put itself in relations with other Oriental Societies in Europe and Asia. The Academy is to consist of forty members and an unlimited number of correspondents. The dignity of *Ham-lam* is to be conferred by the Resident-General. Various degrees are to be given to Tonquinese, and these are to be marked by a medal or emblem to be worn on the dress. Political questions are not to be discussed.

WE regret to learn that difficulties arising out of the reorganisation of the Imperial College of Engineering, Tokio, have resulted in the loss to the new University of Japan of the services of Prof. T. Alexander.

MR. R. JASPER MORE, writing to the *Times*, from the House of Commons, on the subject of "Science for the Masses," inquires why some of the 600 papers which are not read before the British Association should not be delivered as lectures at the schools or public rooms of the West Midland district, where "large and appreciative audiences would have been found, and a foundation laid for the objects of the British Association." "A few words spoken," he says, "in a familiar manner to working-men and others by members of the British Association would tend to make that taste for natural science the absence of which Sir John Lubbock deplores."

AT the last meeting of the Entomological Society of London, on the 1st inst., Mr. C. O. Waterhouse called attention to the various reports which had lately appeared in the newspapers of the discovery of the Hessian Fly (*Cecidomyia destructor*) in Britain, and inquired whether any communication on the subject had reached the Society. The Rev. W. W. Fowler stated that he had been in communication with Miss Ormerod on the subject, and that she had informed him that neither the imago nor larva of the species had been seen, and that the identity of the species rested on the supposed discovery of the pupa.

PROF. BRUN has published in the *Archives de Genève* an interesting study on the so-called lightning holes to be found in the High Alps. He and other investigators have found them at heights of from 3348 to 4000 metres, or between 11,000 and 13,000 feet above the sea-level. Usually they are found on summits. Sometimes the rocky mass, which has been vitrified in the passage of the electric fluid, presents the appearance of small scattered pearls, sometimes of a series of semi-spherical cavities only a few millimetres in diameter. Sometimes there are vitrified rays going out from a central point to a distance of 4 or 5 inches. Sometimes a block detached from the mass appears as if bored through by a cannon-ball, the hollowed passage being quite vitrified. The thickness of this vitrified coating or stratum never exceeds a millimetre, and is sometimes not more than the quarter of that depth. The varying colours which it presents depend on the qualities and composition of the rock. The same may be said as to its transparency. On the Rungfischhorn the glass thus formed by the lightning is black, owing to the quantity of actinolite which the rock contains. It is brown on La Ruinette, the rock consisting of feldspar mixed with gneiss containing chloride of iron. Under the microscope these lightning holes display many interior cavities, which must be attributed to the presence of water in the rock at the moment of melting by the electric discharge. This vitrified material has no influence on polarised light.

THE captain of the steamer *Thessaly*, belonging to the Houston Line, writing to the owners of that vessel, notes a strange experience on his last voyage from Liverpool to Monte Video. On Thursday, July 1, at 11.35 p.m., the ship, which at the time was in lat. 0° 55′, long. 29° 34′ W., was suddenly and violently shaken and bumped, the shaking being accompanied by a loud, rumbling, metallic kind of noise. The first impression was that the ship was tearing the bottom out over hard rock, but knowing there was nothing in the neighbourhood she could touch, save St. Paul's rocks, and as they could not see land, the captain concluded the machinery was going to pieces. A report received from the engine-room, however, stated that there was nothing wrong there. The engineer had slowed down instantly, under the impression that something had gone wrong. The carpenter reported the wells all free. The shock lasted about a minute; no disturbance was visible on the water. About 8 minutes after the first shock, a second, not quite so severe, stopped the ship, which in the meantime had been going slowly. Subsequently they experienced a third shock—a slight one. The lead indicated 60 fathoms with no bottom. Being now satisfied that the shocks were caused by some submarine disturbance, the captain proceeded on his course. After steaming about 15 minutes, he experienced a fourth shock, only inferior to the first in severity and duration. After this all was quiet. During the shocks the compass cards were much agitated.

WE have received a pamphlet by Prince Albert of Monaco describing the investigations which he has made during the past year in his yacht, the *Hirondelle*, into the Gulf Stream, and its relations with the coast of France. After referring to the interest which the Gulf Stream possesses for various branches of science, the Prince describes the knowledge of it possessed by the ancients, and the various investigations of modern times. The stream has been carefully studied by the Americans along their coasts, but our knowledge of it farther out in the Atlantic is more doubtful. Its influence on the French coasts has never been experimentally studied, and to this particular point the work on the *Hirondelle* was directed. 179 floats were thrown out at various places to the north-west of the Azores. These were of three classes—hollow copper balls, oak barrels, and ordinary bottles, there being ten balls, twenty casks, and 149 bottles. The various places of immersion formed a line about 170 miles in length. The conclusion which the Prince draws from the results so far is that as far as 300 miles to the north-north-west of the Azores, the Gulf Stream shows no tendency to flow towards the north-east, and even its tendency towards the east is scarcely pronounced. The pamphlet contains two charts, —one of the places of immersion of the floats, with the dates and hours, the other of the voyage of the *Hirondelle* from the time it left Lorient until its return.

THE session 1886 of the University College, Bristol, will begin on October 5. Lectures and classes are held every day and evening throughout the session. In the Chemical Department lectures and classes are given in all branches of theoretical chemistry, and instruction in practical chemistry is given daily in the chemical laboratory. Excursions to some of the

mines, manufactories, and chemical works of the neighbourhood are occasionally made. The Department of Experimental Physics includes courses of lectures arranged progressively, and practical instruction in the physical and electrical laboratory. Those students who attend the mechanical engineering course enter engineering works during the six summer months, and, in accordance with this scheme, various manufacturing engineers in the neighbourhood have consented to receive students of the College into their offices and workshops as articled pupils; the engineering laboratory is provided with a powerful testing-machine, and instruction in the use of tools is given in the workshop. Special courses in surveying are given, and excursions for field practice are frequently made. The Department for Geology, Biology, and Zoology includes various courses of lectures in all branches of those subjects, together with laboratory instruction. In the Botanical Department practical instruction is given by means of the Botanical Gardens, which contain upwards of 1000 specimens.

THE additions to the Zoological Society's Gardens during the past week include a Common Mole (*Talpa europæa*), British, presented by Mr. J. Scatcherd; two Hawfinches (*Coccothraustes vulgaris*), British, presented by Mr. W. Strutt; a Lanner Falcon (*Falco lanarius*), European, received; two Common Vipers (*Vipera berus*), British, presented by Mr. W. Robertson; a Common Viper (*Vipera berus*), British, presented by Mr. W. H. B. Pain; two Common Marmosets (*Hapale jacchus*) from Brazil, three Indian Crocodiles (*Crocodilus palustris*) from India, deposited; a Mesopotamian Fallow Deer (*Dama mesopotamica*), four Long-fronted Gerbilles (*Gerbillus longifrons*), five American Milk-Snakes (*Coluber eximus*), an Argus Pheasant (*Argus giganteus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

CHANGES OBSERVED ON THE SURFACE OF MARS.—In the July number of the *Bulletin Astronomique* M. Perrotin gives a detailed account of his observations of Schiaparelli's "Canals" made during the months of April and May of the present year (NATURE, June 3, p. 110), remarking that their appearance differs little from that observed in the Milan astronomer's chart drawn in 1882, and that these markings appear to indicate the existence of a state of things, in the equatorial regions of the planet, which, if not absolutely permanent, at all events give evidence of considerable stability. But during the progress of the Nice observations of the "canals," some changes were noticed in the neighbourhood of the Kaiser Sea (Schiaparelli's Syrtis Major), which M. Perrotin has thought it worth while to put on record. During the earlier observations this part of the planet's surface was dark, like all the Martial "seas," and as it is represented in the chart. On May 21, however, the part of Syrtis Major extending from 10° to 55° of north latitude was seen to be covered with a luminous cloud forming regular and parallel bands, stretching from north-west to south-east on the surface, in colour somewhat similar to that of the continents, but not quite so bright. On the 22nd these cloud-like structures were more uniformly distributed than on the previous day; they were also seen on the three following days, but were noted to be of considerably diminished intensity. On May 25 the Nice observers noted the visibility of the isthmus which is placed in Schiaparelli's chart on the prolongation of Syrtis Major, below its junction with Nilus, in longitude 300° and north latitude 52°, and which had not hitherto been seen by them. M. Perrotin thinks it probable that these appearances are really due to clouds circulating in the atmosphere of Mars; at all events he concludes they arise from something connected with the atmosphere or with the surface of the planet capable of motion and of change in a comparatively short space of time.

A SUSPECTED NEW VARIABLE STAR.—In *Circular* No. 8 of the Liverpool Astronomical Society, Rev. T. E. Espin states that the star D.M. + 35° 4002 was observed by him on the night of June 26 last as a very red 8·5 mag. star. On August 29 it was again observed with the same comparison stars, and was found to be barely 9·5. There seems, therefore, reason for suspecting it of variation. Dunér calls this star "rouge-jaune foncé," spectrum III*b*. ! !, and identifies it with Pickering No. 36 (*Astronomische Nachrichten*, No. 2376), which seems improbable, as Pickering's place is 1m. 20s. *preceding* and 0° 7′ *south*. The place of D.M. + 35° 4002 for 1885 is R.A. 20h. 6m. 3s., Decl. + 35° 36′·1.

THE BINARY STAR OΣ 234.—In the *Astronomische Nachrichten*, No. 2743, Mr. J. E. Gore publishes elliptic elements of the orbit of this binary. The components are of magnitudes 7 and 7·4, and the star has always been a close and difficult object to measure even with large telescopes. Mr. Gore considers his orbit as provisional only, on account of the discordance of some of the recorded measures. The following are the elements:—

$P = 63{\cdot}45$ years	$\Omega = 124° \; 11'$ (1880·0)
$T = 1881{\cdot}15$	$\lambda = 71° \; 58'$
$e = 0{\cdot}3629$	$a = 0''{\cdot}339$
$\gamma = 47° \; 21'$	$\mu = +\, 5°{\cdot}67$

Mr. Gore states that these elements satisfy the observations fairly well from 1844 to 1853, and from 1870 to 1880, but in the years 1858–66 the discordances are considerable. The position of the star for 1880·0 is R.A. 11h. 24m. 20s., Decl. + 41° 58′.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *SEPTEMBER* 12–18

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on September 12

Sun rises, 5h. 31m.; souths, 11h. 56m. 11·0s.; sets, 18h. 21m.; decl. on meridian, 4° 7′ N.: Sidereal Time at Sunset, 17h. 48m.

Moon (Full on September 13) rises, 18h. 8m.; souths, 23h. 38m.; sets, 5h. 16m.*; decl. on meridian, 6° 34′ S.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury ...	4 10 ...	11 9 ...	18 8 ...	10 46 N.
Venus	3 30 ...	10 40 ...	17 50 ...	12 53 N.
Mars	10 44 ...	15 19 ...	19 55 ...	16 46 S.
Jupiter... ...	17 27 ...	13 15 ...	19 3 ...	3 9 S.
Saturn... ...	23 58* ...	8 2 ...	16 6 ...	21 37 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (not actually occulted at Greenwich)

Sept.	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
13 ...	φ Aquarii ...	4½ ...	1 50	near approach	227 —
13 ...	24 Piscium ...	6½ ...	20 33	near approach	168 —
16 ...	ν Piscium ...	4½ ...	7 12	near approach	56 —

Sept.	h.	
18 ...	21 ...	Venus at least distance from the Sun.

Variable Stars

Star	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·2 ...	81 16 N. ...	Sept. 16,	19 25	*m*
Algol	3 0·8 ...	40 31 N. ...	,, 14,	19 38	*m*
ζ Geminorum ...	6 57·4 ...	20 44 N. ...	,, 13,	21 15	*M*
			18,	23 6	*m*
δ Libræ	14 54·9 ...	8 4 S. ...	,, 14,	2 37	*m*
U Coronæ	15 13·6 ...	32 4 N. ...	,, 15,	4 2	*m*
U Ophiuchi... ...	17 10·8 ...	1 20 N. ...	,, 12,	2 55	*m*
		and at intervals of		20 8	
β Lyræ...	18 45·9 ...	33 14 N. ...	Sept. 18,	21 0	*M*
η Aquilæ	19 46·7 ...	0 43 N. ...	,, 13,	5 0	*M*
			,, 18,	0 0	*m*
δ Cephei	22 24·9 ...	57 50 N. ...	,, 12,	5 0	*M*
			,, 15,	22 0	*m*

M signifies maximum; *m* minimum.

THE RECENT EARTHQUAKES

THE earthquake record of the past week is a long and disastrous one. An earthquake of wide area and extraordinary intensity took place soon after 10 o'clock on the night of August 31, throughout nearly the entire portion of the United States east of the Mississippi, shocks being felt from the Gulf of Mexico northwards and from the Mississippi eastward to the Atlantic. The shocks were especially severe at Montgomery (Alabama), Cleveland (Ohio), Meadville (Pennsylvania), Raleigh (North Carolina), and Indianapolis (Indiana). In New York, Washington, Detroit, Milwaukee, Cincinnati, Louisville, Chattanooga, and other places severe undulations were felt. The shock was light at Chicago, and west of the Mississippi at Omaha, Ogden, or San Francisco no disturbance was felt. The bounds of the disturbed area are thus roughly defined to be the Mississippi, the Atlantic, the Gulf of Mexico, the Lakes and the St. Lawrence. Georgia and South Carolina appear to have been the most severely visited of all the States. At Augusta, in the former State, there were ten distinct shocks between 9.15 and 10.45 p.m., and the streets were filled with the terrified population. At Savannah five shocks were felt. Sharp shocks were also felt in New Jersey, and vibrations as far north as Philadelphia. Prof. Newcomb, of the *Nautical Almanac*, Washington, reports that the first shock occurred at 9.53.20, and the second at 9.54 30, lasting until 9.59. The Signal Service Bureau at Washington reports that four distinct shocks were felt there. The first began at 9.54, and lasted 40 seconds, the second at 10.0.4, the third at 10.10, and another at 10.30. Charleston, in South Carolina, suffered most severely,—the streets were blocked with fallen buildings, telegraph poles, and tangled wires. The population spent the night in the streets. As usual after violent earthquakes, fires broke out. The principal business quarter and two-thirds of the dwelling-houses have been destroyed, and the town was isolated from the outside world, the bridges and railroads being all destroyed. Sullivan's Island, a watering-place near Charleston, was submerged by a tidal wave. At Columbia, in the same State, ten distinct shocks of earthquake were felt, the last at 10.20 on the morning of the 1st inst. Fresh shocks were felt in the afternoon of the 1st at Augusta, Charleston, and Columbus, and during the same day throughout North and South Carolina and Georgia, and many fissures opened, emitting fresh water, white sand, and blue mud from a great depth. At 11.55 on the night of the 1st another violent shock in Charleston brought down several houses. Since that date, shocks of more or less violence have continued up to the present in the States above-mentioned.

This earthquake is believed to have disturbed a greater extent of territory than any earthquake on record. Twenty-two States, covering an area of a million square miles, were affected. Toronto and London in Ontario are said to have felt some symptoms of disturbance. The natural phenomena accompanying the earthquake are curious. The fissures in the earth, whence the sulphurous fumes arise, are not confined to Charleston itself, but are found for miles round the town. From these fissures is exuded sand, white in some places, and red in others. From other openings brackish tepid water has been spouted from 15 to 20 feet high. These fissures are not made by the sinking of the ground, but by the tearing apart of the earth's crust. Sometimes they are 20 yards long, and they are of uncertain depth. They could be seen to widen and contract during the shocks, and sand, water, and an unfamiliar substance of an oily paste character was expelled. After these ejections mound-like cones remained. The water in the wells was observed to rise and fall. At Summerville, the holiday resort of the people of Charleston, detonations were heard about once in ten minutes in all directions, but they appeared to have no relation with the earthquake shocks. There are some doubtful reports of flames being seen proceeding from the ground. The atmosphere during the earlier phenomena was oppressive, and so still was the air that the lamps burned out of doors for hours without flickering. A violent shock which visited part of South Carolina at 11 o'clock on the night of the 5th, was followed after an interval of five minutes by two brilliant meteors which shot across the sky from north to south. A curious occurrence is reported by the Correspondent of the *Times*. He says that on the 2nd inst. at Charleston "two showers of morsels of flints, abraded by mechanical action, were noticed, some having been recently fractured. The first shower fell at 7.30 a.m., and the second at 11." At the Signal Office at Washington the self-registering wind-vane shows a horizontal mark preceding and subsequent to the shaking, denoting a mild, steady, and almost invariable breeze. But during the 30 or 40 seconds of the most violent shaking, the marks indicate that the pencil point moved up and down the paper many times with great rapidity. The effect of the earthquake at sea is described by the captain of the steamer *Palatka*. He had just left Charleston, and was twelve miles off the harbour of Port Royal, in eight fathoms and a half, when he heard a terrible rumbling, lasting a minute and a half. There had been a heavy sea from the south-east, but when the rumbling began the wave-motion ceased, and the waters remained in a perfect calm until the rumbling ended, when the swell was again manifest. The wind was south-east and light, the weather cloudy, the barometer 30·01, and the thermometer at 80°. The ship's timbers vibrated strongly. No unusual meteorological conditions prevailed at Charleston before, during, or after the earthquake on the first day, according to the officer of the Signal Service. Profs. Mendenhall and M'Gee are investigating the effects of the earthquake at Charleston and Summerville.

Earthquakes are also reported from Malaga, where a severe shock occurred, on the 1st and morning of the 2nd; from Santa Cruz, in California, at 11.45 on the morning of the 2nd, where it is described as not violent, but long-continued; and from Smyrna, where several sharp shocks were felt between 10 and 12 on the night of the 31st, that is almost simultaneously with the American earthquake. A telegram dated the 5th inst., from Athens, reports that the shocks at Pyrgos have been renewed.

We have received several letters relating to the earthquake in the Eastern Mediterranean of the 27th ult. Prof. Forel, of Morges, writes that, as it extended at least from Alexandria to Berne, it covered an area of about 25° diameter. At Berne the seismograph of the Observatory marked the shock at 10h. 36m. 16s. local time, corresponding to 22h. 6m. 30s. Greenwich time. If, continues Prof. Forel, we admit, in accordance with the times recorded at Benevento, Fermo, Pesaro, and Zante, that the time of the shock at the centre was 22h. 1m. 20s. ± 70s. Greenwich time, then the seismic wave would have taken 5m. 10s. to reach Berne. It is said that the shock was felt at Alexandria 15 minutes after midnight, in which case progress in that direction was a little slower; put into Greenwich time, this would be 22h. 15m., or about 14m. for transmission from Zante to Alexandria. In Switzerland a small preparatory shock was felt in the Alps of Vaud, about 11 minutes before the Berne shock. Mr. Henry Simon, writing from Engelberg, in Obwalden, states that the shock was distinctly felt there as a swaying motion about 10.20 by several of the visitors and in different houses.

THE SCOTTISH METEOROLOGICAL SOCIETY

THE following is an abstract of the Report of the Council read at the meeting of the Scottish Meteorological Society held on July 22:—

Since the general meeting of the Society in March, the number of the Society's stations has remained the same.

The membership of the Society now numbers 719, being 7 more than at the meeting in March.

The preparation of a fourth paper on the climate of the British Islands, dealing with the monthly rainfall for the twenty years 1866–85, is now far advanced, and the results are practically and scientifically of great interest.

Much time has been spent in preparing for the press the whole of the observations of the Ben Nevis Observatory and those of the station at Fort William for the two years and a half from December 1883 to May 1886. The volume will shortly be in the hands of scientific men in all parts of the world. In connection with these valuable observations, the investigation of the important question of the bearing of the results on the weather of these islands is steadily advancing.

The position of the Ben Nevis Observatory on an elevated isolated peak, and the adjoining low-level station at Fort William being close to the sea, and on a bank sloping down to it, renders this pair of stations second to none anywhere yet established for the investigation of some of the fundamental facts of meteorology. Among the more important of these questions is the determination of the rate of decrease of temperature with height, and the rate of diminution of atmospheric pressure with height, for different atmospheric temperatures and sea-level pressures.

In these aspects the observations for the two years and a half from December 1883 to May 1886 have now been discussed. As regards decrease of temperature with height, it is shown to be at the rate of 1° F. for every 270 feet of ascent—a rate which closely agrees with the results of the most carefully conducted balloon ascents, and of those other pairs of stations over the world which are so situated as to yield trustworthy results for the inquiry. Ben Nevis Observatory and the Fort William station are among the very few such groups of stations that have yet been anywhere established.

In researches into weather phenomena and weather prognostics the most important point to determine is the normal difference between atmospheric pressure at the top of the Ben and at Fort William for the different atmospheric temperatures and sea-level pressures. This was empirically calculated from the observations, and thereafter the departures from the normals were ascertained for the five observations of each day since the Observatory was opened. The results showed a diminution of pressure from the calculated normals during the occurrence of high winds at the Observatory. The difference not unfrequently amounts to the tenth of an inch, and on one day the five consecutive observations showed differences of about a tenth-and-a-half inch. This diminution of pressure is doubtless occasioned by the winds, as they brush past the Observatory buildings, partially sucking out the air from the interior, thus lowering the pressure. This does not occur till the velocity rises to or exceeds the rate of 30 miles an hour.

It thus became necessary to recalculate the normals for pressure, using in the computations only those observations which were made when the velocity of the wind fell short of 30 miles an hour. This recalculation has recently been completed, and the inquiry as to the bearing of the Ben Nevis observations on the weather of the British Islands is being pushed forward.

The work at the Ben Nevis Observatory continues to be discharged by Mr. Omond and his assistants in a way that leaves nothing to be desired. Since the last meeting of the Society Mr. Omond has contributed a valuable paper to the Royal Society of Edinburgh, on the observations of wind force recorded at the Observatory. From a comparison of the results obtained from the registrations of Prof. Chrystal's anemometer, and the estimations of the force of the wind made by the observers on scale 0 to 12, he has determined the velocity in miles per hour for each figure of the scale, 1, 2, 3, &c. The highest figure for which the double observations were sufficiently numerous, so as to give a good average, was 8, which was found to be equivalent to a rate of 73 miles an hour. This velocity is of frequent occurrence; and as regards the higher force 11, which occasionally occurs, Mr. Omond estimates its equivalent at 120 miles an hour. Observations on the rain band have been undertaken by Mr. Rankin, the first assistant.

The hygrometric observations made by Mr. H. N. Dickson at the Scottish Marine Station and the Observatory have now been partially discussed by him, and the results submitted to a recent meeting of the Royal Society of Edinburgh. These results are of considerable value in determining how far Glaisher's factors, so largely used by meteorologists in hygrometric inquiries, may be used satisfactorily. As regards the remarkably dry states of the atmosphere, which are so prominent a feature in the climate of the Ben, Glaisher's factors are altogether inapplicable, and hence the hygrometric observations of the Observatory, therefore, will require a specially constructed set of tables.

The zoological work at the Marine Station has been carried on regularly. The principal work since March last has been the examination of the ova and larvæ of fishes. Endeavours have been made to obtain the early stages of as many species as possible, and as only a few species breed at one season, considerable success has been achieved. The results of this work have been communicated to the Royal Society.

Since the last meeting the Physical Department of the Scottish Marine Station has been actively engaged in carrying on observations of temperature and water-density in the Firth of Forth and the Clyde districts. Meteorological observations are continued at Granton, and the temperature of a number of rivers (the Thurso at three points, the Tummel, Forth at two points, Teith, Tweed, and Derwent) is being observed daily. Mr. Morrison has continued his monthly trips to Loch Lomond and Loch Katrine, where he has observed the vertical distribution of temperature. The work on the Firth of Forth, usually carried on solely by serial temperature observations made on the *Medusa* inside the Isle of May, has been supplemented by the captain of one of the Granton steam-trawlers, who was supplied with a deep-sea thermometer, and has been using it to good purpose in the North Sea from 30 to 60 miles off shore. The tidal variations of salinity in the estuary of the Forth were investigated by Dr. Mill and Mr. Morrison during a week's stay at Kincardine, and the result embodied in a paper read to the Royal Society of Edinburgh. Two trips of the *Medusa* on the Clyde supplied data for a paper which is presented to this meeting, as well as a quantity of observations not yet fully worked up.

The work of collecting and discussing the sea temperatures around the coast of Scotland is being carried on by Dr. Mill and Mr. Morrison. Since last meeting the Meteorological Council of London has lent all the sea observations made at the Scottish Coast-Guard Stations from 1879 to 1885; and observations of river temperature made by direction of the Duke of Sutherland and Mr. Boyd, Peterhead, have also been received. The Government Grant Committee of the Royal Society of London has given a grant of 50*l.* towards the completion of this work. Thermometers have been lent for use in the deep water off Shetland to Mr. F. Coulson, who is at present on a dredging trip in his yacht in that locality. The National Fish Culture Association of England, which has been in correspondence with the station for some time as to physical observations, has now commenced operations at several light-ships and on board the mission-smacks in the North Sea.

Mr. John Murray, of the *Challenger* Expedition, made a communication on the extent of the areas of the different mean annual rainfalls over the globe. He had been led to undertake this inquiry so as to find out the amount of material carried down from the land to the ocean, and which went to form ocean deposits. The amount of rain that fell upon the surface of the globe annually was estimated at about 34,000 to 35,000 cubic miles. Taking the inland drainage areas disconnected with the sea, such as the Sahara Desert, it is found that 77 cubic miles of rain fell upon these surfaces, which must be regarded as equivalent to the amount of evaporation. The Americans had calculated that 99 million millions of cubic feet of rain fell annually over the Mississippi drainage area. Calculating the outflow of the river, they estimated that only one-fourth of that water reached the ocean. By extending their inquiry over large areas, it was hoped that it would be made of some practical importance.

Dr. H. R. Mill, of the Scottish Marine Station, read a paper on the temperature of the water in the Firth of Clyde and its connected lochs. The configuration of the water system was explained by means of a bathymetrical chart. Roughly speaking, the Firth of Clyde contained two tracts of deep water—one in which the water is over 70 fathoms deep, running up Kilbrannan Sound, and the other rather deeper between Arran and Ayrshire—uniting with the first at the north of Arran, and continuing up Loch Fyne to near Ardrishaig, attaining its greatest depth off Skate Island, near Tarbert, where it is 107 fathoms. These tracts of deep water are separated from the Atlantic by a broad plateau, which extends between the Mull of Cantyre and Girvan, and rises to within 25 fathoms of the surface. There are also three shallow lochs—Gareloch, Holy Loch, and Loch Ridden; and four deep lochs—Upper Loch Fyne, Loch Goil, Loch Strivan, and Loch Long—which are shut off from communication with the outside waters by barriers rising in some cases to within a few fathoms of the surface. Dr. Mill then gave an account of the temperature in each of the regions as were ascertained during trips of the *Medusa* in April and June. He omitted, he said, discussing in detail the individual observations until fuller investigation gave the data for a general theory. In April, surface temperature over the whole Clyde district varied only from 42° to 45°, and the temperature at considerable depths had a range of not more than half a degree—41°·3 to 41°·8. The warmest water was found on the barrier plateau at the south end of the firth, and outside it, the deep lochs came next, and then the deep open basins. In all cases the temperature fell gradually, proceeding downwards for about 10 or 15 fathoms, and then remained constant to the bottom. By June a considerable heating of the surface-layers had taken place, and the different regions had undergone changes to a very different extent. The shallow lochs had been heated apparently from the surface and from the bottom; the greatest rise in temperature was found beyond the plateau, then in the deep open basins, and the least in the deep lochs, one of

which, Loch Goil, was only half a degree warmer than in April. The range of surface temperature in June was from 45° to 53°, and of bottom temperature from 42° to 47°·3, according to locality. Constant temperature to the bottom commenced at a much lower depth than in April. In the upper basin of Loch Long, which was discussed with more fulness, the surface-temperature was 48°·4, at 10 fathoms it was 44°·2, and from 55 fathoms to the bottom at 70 fathoms it was 44°. But between 10 fathoms and 55 fathoms the water was colder than at either of these points, reaching its lowest temperature of 42°·8 at 20 fathoms. It thus appeared that a lenticular mass of water floated between the warmer strata, the opinion as to the cause of which was meantime reserved until further light can be thrown on the phenomenon. In the Clyde district, Dr. Mill said, physical configuration is the determining cause of differences of temperature, and it appears that as the season advances, warmth descends from the surface everywhere by conduction, and travels inward from the sea by conduction and convection. The study of water climate, he said, was likely to lead to important results, but it must be carried on by a large number of observers, who would note the temperature of rivers and of falling rain, before any degree of completeness could be obtained. The paper was illustrated by a series of admirable charts.

SCIENTIFIC SERIALS

THE *Journal of the Franklin Institute*, August.—Capt. O. E. Michaelis, the applications of electricity to the development of marksmanship. This is the conclusion of an interesting paper on chronoscopic and chronographic methods, illustrated by cuts of recent instruments.—W. Lewis, experiments on transmission of power by gearing (conclusion of the discussion).—F. Lynnwood Garrison, the microscopic structure of car-wheel iron.—G. Richmond, the refrigeration-machine as a heater.—C. Hoele, a method of designing screw propellers.—F. E. Ives, correct colour-tone photography with ordinary gelatine bromide plates. A proposal to reduce the sensitiveness of the bromide films to the blue and green rays, by introducing into a plate-glass tank mixtures of aniline colour solutions, chiefly yellow and red, in certain proportions, thereby equalising the sensitiveness throughout the range of the visible spectrum.—Joshua Pusey, suggestions towards a simplified system of weather signals, termed the index weather-signal system.—P. E. Chase, Herschel and Jevon on density of the ether.

Annalen der Physik und Chemie, vol. xxviii. No. 8, August 1886.—Prof. G. Quincke, electrical researches, No. xii., on the properties of dielectric fluids under strong electric forces. The dielectric constant of a number of liquids is examined by two methods, by attraction between two plane parallel plates immersed in the liquid, and by discharge of their charges through a galvanometer. High potentials were obtained by a Holtz machine, and measured by a long-range electrometer up to 30,000 volts. The results show that with high electric forces the dielectric constant is less than with lower electric forces; in other words, there exists an apparent tendency to saturation in inductive capacity. Measurements of the dielectric constant are always from 10 to 50 per cent. higher when made by the balance-method than those made by the condenser discharge method. In different dielectric fluids the spark-distance for the same difference of potentials is different, and always much shorter than in air. The potential requisite to produce a spark within a dielectric liquid increases with the spark-length, but at a slower rate. The strength of a steady electric current in a dielectric fluid increases more rapidly than the electromotive force which produces it; an exception, apparently, to Ohm's law.—L. Sohncke, electrification of ice by water-friction. Experimental proof that water becomes negatively electrified and ice positively electrified by mutual friction. The author thinks thereby to explain the origin of thunderstorms by friction of cumulus and cirrus clouds.—E. Edlund, researches on the electromotive force of the electric spark. He finds the counter-electromotive force of the electric spark to be divisible into two parts, one at each pole, that at the positive pole decreasing, and that at the negative pole steadily increasing, as the air-pressure is diminished. He regards this as explaining the anomalies of unequal heating of the electrodes.—W. Donle, contributions to knowledge of the thermo-electric properties of electrolytes. According to these experiments the thermo-electromotive force between two electrolytes, such as solution of sulphate of copper and sulphuric acid is approximately proportional to the differences of temperature of the points of contact; the proportionality varying in some way with the concentration of the solutions. The electromotive force is usually less with more concentrated solutions. Through the heated junction of a chloride and a sulphate the current flows from chloride to sulphate.—F. Auerbach, on the electric conductivity of metal powders. Precipitated silver was used. The author finds an enormous reduction when the density is increased by mechanical force.—R. Krüger, on a new method of determining the vertical intensity of a magnetic field. This method consists in sending an electric current radially through a horizontal copper disk suspended by a thin wire, and observing the rotation of the disk.—R. Maurer, on the ratio of the sectional contraction to the longitudinal elongation produced in rods of glue-jelly. The rods were made of gelatine and water, and of gelatine and glycerine. One of the methods was an electrical one, consisting in observing the change of electrical resistance on stretching. These jelly rods exhibit the phenomena of residual strains very markedly.—M. Hamburger, researches on the duration of the impact of cylinders and spheres.—Dr. K. Noack, on the fluidity of absolute and diluted acetic acids. Curious minima of fluidity are observed by the author, varying with concentration and with temperature.—W. Müller-Erzbach, the law of decrease of absorbing power with increasing distance.

SOCIETIES AND ACADEMIES

EDINBURGH

Royal Society, July 19.—Mr. Robert Gray, Vice-President, in the chair.—The Right Hon. Lord Rayleigh communicated a paper on the colours of thin plates. He has laid down on Maxwell's triangle of colours a curve representing the variation of the colours of thin plates as the thickness of the plates increases.—Prof. Dr. Fr. Meyer communicated a paper on algebraic knots.—Prof. Tait described Amagat's "manomètre à pistons libres."—Prof. C. G. Knott communicated a paper on the electrical properties of hydrogenised palladium. This paper contains the results of experiments on the resistance and thermo-electric properties of hydrogenium or hydrogenised palladium. Up to a temperature of about 200° C. no special peculiarity is noticeable; but at that temperature, or a little higher, hydrogen begins to escape from the wire, and this causes the particular specimen of hydrogenium to recover partially, if not wholly, its pure palladium characteristics. It is known that the resistance of a palladium wire charged with hydrogen at ordinary atmospheric temperatures increases at a rate almost strictly proportional to the amount of charge. The same law seems to hold at all temperatures up to 150° C., and in such a way that the *total* increase of resistance of a given palladium wire for a given rise of temperature is nearly the same at all charges; or the temperature-coefficient for any particular specimen of hydrogenised wire is practically inversely proportional to the resistance as compared with the resistance of the wire in its pure uncharged state. Just before the hydrogen begins to escape, the resistance begins to increase somewhat more rapidly than at lower temperatures; and this peculiarity is more marked in the specimens of higher charge. When once the hydrogen begins to escape, the resistance begins to fall off rapidly as the temperature rises to 300° C. At this temperature the wire cannot be distinguished from pure palladium. In the thermo-electric experiments, peculiar irregularities appear at the higher temperatures, which seem to be due to the fact that the hydrogenium wire is unequally heated, and that the hydrogen, which is almost completely driven out of the heated portion of the wire, returns partially as the wire is cooled down again. In all cases at temperatures below 150° C., the current is from pure palladium to hydrogenium through the hot junction, is probably proportional to the difference of temperature in each case, and is greater for the greater charge. Thermo-electrically, fully saturated hydrogenium lies between iron and copper at ordinary atmospheric temperatures. On the thermo-electric diagram the hydrogeniums of different charge are represented (up to a temperature of 150° C.) by a series of straight lines parallel to palladium, whose thermo-electric powers at 0° C. range roughly from − 600 (pure palladium) to + 1400 (saturated hydrogenium) expressed in C.G.S. units. (Compare Everett's "Units and Physical Constants," p. 151.)

In other words, the electromotive force in a circuit of palladium and saturated hydrogenium, the temperatures of the junctions being 0° C. and 100° C., is 20×10^4 C.G.S. units, or ·002 volts. The thermo-electric peculiarities of hydrogenium may be prettily shown by the following simple experiment. Let a palladium wire, by immersion to half its length in the electrolytic cell, be hydrogenised throughout that half length. Attach the ends of this seeming single uniform wire to the terminals of a galvanometer, and let a flame be allowed to play gently at the central point of the wire. A large current is at once obtained, which grows to a maximum, and then diminishes to zero as the temperature rises to a red heat. There is no such current during cooling. This spurious neutral point is due to the hydrogen being driven out of the heated portion, partly, no doubt, into the contiguous colder portions. By following up with the flame the ever-shifting point of separation between the charged and uncharged portions, we may repeat the experiment indefinitely until the hydrogen is all driven out of the wire, or until the distribution of hydrogen has become fairly uniform.—Mr. Thomas Andrews communicated a paper on the electro-chemical reactions between metals and fused salts.—Mr. H. N. Dickson communicated a paper on the hygrometry of Ben Nevis and the Scottish Marine Station.—Mr. J. T. Morrison read a paper on the temperature of Loch Lomond and Loch Katrine during winter and spring; also, a note on the surface temperature near a tidal race.—Mr. John Aitken gave further remarks on dew.—Prof. J. B. Haycraft gave a communication on the nature of the objective cause of sensation.

Sydney

Royal Society of New South Wales, July 7.—H. C. Russell, F.R.A.S., in the chair.—The following papers were read:—Further additions to the census of the genera of plants hitherto known as indigenous to Australia, by Baron Ferd. von Müller, K.C.M.G., F.R.S. The author gives the number of Australian plant-genera recorded hitherto as 2248.—Notes on improvements in the construction of reflecting telescopes by hand, and experiments with flat surfaces, by Mr. H. F. Madsen. The author showed an 18-inch speculum, and the glass tool with which it was worked. The latter was composed of three plates of 1-inch rough glass cemented to form a solid block, and worked to about one-quarter more convexity than the required concavity of the speculum, which was partly hollowed out at first by a leaden weight and emery. The speculum-glass was then ground by hand over the block, the two forming themselves into perfectly spherical surfaces having a high reflective power, and free from irregularities of less than 1/50,000 inch. The speculum, having now an absolutely true surface, was polished with emery upon pitch, it being uppermost, and moved round without pressure. The pitch-polisher had an improved graduation, the result being that, without side motion, the speculum was polished by hand for hours without producing the trace of a ring. Both polisher and glass having been regularly raised in temperature, were left together until cool, when ten minutes was required to give the true parabolic curve, the glass being simply revolved on the polisher, great care being taken to avoid the slightest inequality in temperature. Without the aid of machinery, it is doubtful if larger specula than 18-inch could be produced by hand. Mr. Madsen investigated the thickness of the silver film of a speculum by a novel optical method, and confirmed the late Dr. Draper's "chemical" estimate, viz. 1/200,000 inch. Two perfectly flat surfaces 5 inches diameter were taken and illuminated by a homogeneous yellow light of 1/44,000 wave-length, falling at an angle of 30° incidence (Brashear's colour-test), whereby a series of straight dark and coloured bands were visible. By silvering half the upper surface of one of these glasses the bands were displaced or broken at the edge, a distance of about $2x$ (x being the distance between two succeeding bands). The thickness of silver, δ, would be expressed by

$$\frac{\lambda\,(\text{wave-length})}{5} \sec 30 = 0{\cdot}00000525.$$

Several measurements gave less than 1/300,000 inch. Under the same optical methods the effects of heat and cold were rendered plainly visible and measurable. The true surfaces were placed on a 2-inch diameter wooden chuck; the light falling at 65° gave a uniform colour. On applying the finger without pressure upon the centre of the top glass, the colour changed to regular concentric rings, causing the glass to become concave by a measurable quantity. Placing the glass upon an iron support produced convexity (1/30,000 inch) in a regular curve. With a pressure of 8 lbs. on the centre, two wide bands of colour appeared, crossing in the centre, straining the glass in two directions, and destroying its figure. These experiments show how the defining power of specula and lenses is injured by temperature.

Paris

Academy of Sciences, August 30.—M. Émile Blanchard in the chair.—In the name of the Academy the President felicitated M. Chevreul on his hundredth anniversary, remarking that the case was unique in the annals of the Academy; even Fontenelle, although spoken of as a centenarian, having died shortly before reaching that venerable age. M. Chevreul replied with a few touching words of gratitude for the sympathy of his *confrères*, after which a telegram was read from the University of Kasan complimenting the patriarch of the scientific world on his long and laborious life, so fruitful in valuable contributions to the progress of the technical arts.—On a remarkable case in the problem of planetary perturbations, by M. F. Tisserand. In the case of two planets revolving round the sun, or of two satellites round their planet, in orbits slightly inclined towards each other, it is shown that even if the proper excentricity be null there may be a very sensible apparent excentricity. In other words, if the movement of one orb was originally circular and uniform, the perturbations caused by the other would transform this movement into one approximating to a Keplerian elliptical orbit with a uniform rotation of the long axis. These results are compared with those obtained by A. Hall and S. Newcomb for the Saturnian satellite Hyperion, in so far as its movement results from the perturbations caused by the larger satellite Titan.—On the atomic weight of germanium, by M. Lecoq de Boisbaudran. The atomic weight of this body, provisionally determined by M. Winkler at 72·75, and by the author theoretically at 72·28, is now found by M. Winkler to be 72·32. The law of proportionality between the variations of the atomic weight and those of the wave-lengths, a law already applied to gallium, here receives a fresh and important confirmation. It becomes at the same time highly probable that no appreciable error now exists regarding the atomic weights of cæsium, rubidium, potassium, indium, gallium, aluminium, tin, and silicium. In fact the wave-lengths and atomic weights of Cs, Rb, K, In, and Al have served to calculate spectrally the atomic weight of gallium (afterwards verified analytically), while the λ and atomic weights of In, Ga, Al, Sn, and Si have helped to determine spectrally the atomic weight of germanium.—Note on a reptile of the Permian formation, by M. Albert Gaudry. To this reptile, which was found by M. Bayle in the Permian beds of Télots, near Autun, the author proposes to give the name of *Haptodus baylei* (from ἅπτω and ὀδούς), the teeth adhering so closely to the maxillaries as at first sight to be scarcely distinguishable from them. In these rocks, where no animals higher than fishes were for a long time known to occur, there are now found four distinct types of Reptilia: Actinodon, Protriton, Stercorachis, and Haptodus.—Phosphorography applied to the photography of the invisible, by M. Ch. V. Zenger. Observing Mont Blanc after sunset in September 1883, the author noticed that the blue-greenish glow remained perceptible till 10.30 p.m.; hence he concluded that the ice on the summit mingled with carbonate of lime emitted a light like that of Lake Geneva, and that it might be possible to fix the image of the mountain at night by means of the phosphorescent light of the ice, which is highly actinic. On his return he projected the images given by the photographic lenses in the dark chamber on a glass plate covered with a layer of Balmain's phosphorus, just as such plates are prepared with collodion. After exposing it for a few seconds, he removed it in the dark from the chamber in order to place it in contact with a not very sensitive dry photographic plate. After an hour of contact in the dark, the image of the object appeared in all its details as in an ordinary case of photographic impression. Subsequent experiments tended to show that light may be absorbed, and afterwards slowly given back, and that images of objects invisible in the dark may be fixed by simple contact, or by the photographic apparatus. He found it useful to cover the plates with chlorophyll, as when thus prepared they become sensitive to all the radiations of the solar spectrum from the ultra-red to the ultra-violet.—Observations of Winnecke's comet made at the Obser-

vatory of Algiers with the 0.50 m. telescope, by M. Ch. Trépied. On August 23 the apparent position of this comet was :—

Algiers mean time.	Apparent Right Ascension.	Log. fact. parall.	Apparent Declination.	Log. fact. parall.
h. m. s.	h. m. s.		° ′ ″	
8 4 29 ...	13 21 11·65 ...	1·656 ...	− 3 2 31·8 ...	0·731

—On some non-linear differential equations, by M. Roger Liouville.—On the algebraic integrals of the problems of dynamics, by M. G. Kœnigs.—Notes were submitted by M. Martin on an apparatus reproducing the motions of the heavenly bodies, and by M. L. Hugo on the geometrical forms of the hailstones which fell in Paris on August 23.

Berlin

Chemical Society, July 26.—C. Liebermann, President, in the chair.—S. Gabriel has further examined isoquinoline obtained by the reduction of monochlorisoquinoline; it melts at 20°. He has also prepared some new derivatives of dichlorisoquinoline.—Biedermann has prepared some derivatives of parahydroxybenzylalcohol.—Raschig communicated a very interesting research on the nature of gold chloride. He has prepared nitrogen compounds corresponding to the three oxidation stages of gold, and these he has analysed by a new method; he points out the analogy between the iodides of nitrogen and gold fulminate and the analogous compounds obtained from gold chlorides and methylamine.—Prof. Pinner reported on the following communications received by the Society:—Clève, on naphthalenesulphonic acids and on the value of orientation determined with the help of phosphorus pentachloride.—P. Bradley, on thienylglyoxylic acid and its derivatives.—R. H. Mertens, on the nitration of di- and mono-methylaniline with dilute nitric acid.—R. Leuckart and E. Bach, on the action of ammonium formate on benzaldehyde and benzophenone; bases are produced, that from benzophenone having the composition $\left.\begin{matrix}C_6H_5\\C_6H_5\end{matrix}\right\rangle CH.NH_2$. Camphor also reacts with ammonium formate with production of crystalline compounds which, however, have not yet been further examined.—T. H. van't Hoff and Ch. M. von Deventer have studied the question of the temperature at which reaction takes place in chemical decomposition and the accompanying phenomena: first in the case of double salts, *e.g.* sodium ammonium racemate or copper calcium acetate; and secondly in the case of double decomposition, *e.g.* the decomposition of magnesium sulphate and sodium chloride with formation of astracamite and magnesium chloride, the reaction temperature in this case being 31°.—B. Tollens describes what he considers the best method for preparing formaldehyde.—Werner Kelbe and H. Stein have a paper on the products of the action of bromine on aqueous solutions of xylenesulphonic acids.—H. von Perger gives a preliminary account of the results obtained from the action of ethyl acetoacetate and ethyl acetonedicarboxylate on hydrazo-compounds.

Stockholm

Geological Society, May 6.—Baron Nordenskjöld gave an account of his researches on the atomic weights of certain rare terrestrial metals, pointing out the peculiar conditions under which they combine in some minerals. He further described the analyses of the dust which had fallen in 1883 in the Cordilleras, believed to be of cosmic origin, being connected with the much-discussed red glows in the autumn of that year. Baron De Geer expressed the opinion that the glow was a natural meteorological phenomenon, though very pronounced in 1883, whilst Prof. Brögger sided with the usual view of its being caused by the Krakatão eruption.—Dr. E. Svedmark exhibited a map of the district of Roslagen, near Stockholm, showing the lakes and valleys which were considered to be caused by the cracking of the earth's crust. He also corrected the reported discovery of basalt at Tolånga, in the province of Scania, which on closer examination had been found to be diabase accompanied by the formation of tophus.—Dr. F. Svenonius read a paper forwarded by Dr. H. Sjögren, on the mud volcanoes in the neighbourhood of Baku, in which locality he has for a long time sojourned, in order to prosecute geological researches. The volcanoes occur in a line along the Caspian Sea some 120 miles in length. One of the greatest mud cones as 1000 feet high, and the crater 2100 feet in diameter, viz. almost equal to that of Etna. Three violent eruptions have taken place this and last year. They were accompanied by severe emissions of fire, as, for instance, once by a column of are 50 feet in height, visible at a distance of 80 versts. There are also violent discharges of gas, which on one occasion, on being fired, produced a fire-column 20 feet in height. The discharge was so violent that the current could only be fired at a height of 7 feet from the opening. The changes which the surrounding rocks and mountains had suffered through the influence of these volcanoes were of the greatest interest.

BOOKS AND PAMPHLETS RECEIVED

"Journal of Society of Telegraph Engineers," Nos. 62 and 63: List of Members (Spon).—"Pictorial Arts of Japan," part 3, by W. Anderson (S. Low and Co.).—"The Mulberry Silkworm," by C. V. Riley (Washington).—"Record of North American Invertebrate Palæontology for 1885," by J. B. Marcou (Washington).—"A List of the Mesozoic and Cenozoic Types in the Collections of the U.S. National Museum," by J. B. Marcou (Washington).—"Proceedings of the American Academy of Arts and Sciences," October 1885 to May 1886 (Boston).—"Memoirs of the Geological Survey of India," vol. i. 3. "The Fossil Echinoidea." Fasc. vi. "The Fossil Echinoidea from the Makran Series (Pliocene) of the Coast of Bilūckistān and of the Persian Gulf," by P. M. Duncan, and W. P. Sladen (Trübner).—"University College, Bristol: Calendar for the Session 1886-87" (Arrowsmith, Bristol).—"Durham College of Science, Newcastle-on-Tyne: Calendar for the Session 1886-87" (Reid, Newcastle).—"University College, Dundee: Calendar for the Fourth Session 1886-87" (Lang and Co., Dundee).—"Analysis Tables for Chemical Students," by R. S. Taylor (S. Low and Co.).—"Exercises on Mensuration for Junior Students," by T. W. K. Start (S. Low and Co.).—"The Methods of Glass-Blowing," by W. A. Shenstone (Rivingtons).—"First Lessons in Zoology," by A. S. Packard (Holt and Co., New York).—"Fancy Pigeons," parts 11, 12, 13, by J. C. Lyell (U. Gill).—"British Cage Birds," parts 11, 12, 13, by R. L. Wallace (U. Gill).—"Loggio di Igiene Antimicrobica," by I. Giglioli (Napoli).—"Journal of the Chemical Society," September (Van Voorst).—"Thèses à la Faculté des Sciences de Paris," 1 and 2, by J. Deniker (Poitiers).—"Goolden and Trotter's Dynamos," 4th edition.

CONTENTS

THURSDAY, SEPTEMBER 16, 1886

EARTHQUAKE DISTRIBUTION

Alphabetical Catalogue of the Earthquakes recorded as having occurred in Europe and adjacent Countries arranged to serve as a basis for an Earthquake Map of Europe. By Joseph P. O'Reilly, C.E., Professor of Mining and Mineralogy, Royal College of Science, Dublin. *Transactions* of the Royal Irish Academy, Vol. XXVIII. "*Science.*" (Dublin, 1886.)

THE distribution of earthquakes may be studied either geographically or topographically—either along its broad lines in connection with the general physics of the globe, or in its smaller details with immediate and minute reference to local peculiarities. The field of inquiry is in both directions vast and comparatively unexplored. As regards what we may call circumstantial seismography, little has been done in the particular application of general principles to explain the apparent caprices of disturbances. These are innumerable and fantastic; yet, by patient observation, they can to a certain extent be brought within the range of strict physical reasoning. The laws of wave-motion, taken in connection with the facts of geological formation, will account for a good deal. Not, however, for all. Subterranean agencies introduce an element of uncertainty into the calculation which no diligence or ingenuity avails wholly to eliminate. Thus, in South America certain districts are observed to remain year after year, perhaps century after century, unscathed amid surrounding devastation. They are hence called "bridges," the disturbance seeming, as it were, to flow beneath them, like a river under a bridge. Prof. Milne suggests that their immunity may be due to the total reflection of earth-vibrations which would otherwise reach them (" Earthquakes," p. 141). An obstacle to the propagation of such is besides often interposed by faults and fissures. The partial repose of Quito is attributed to the frequency of cañons in its vicinity; as, similarly, the Capitol of Rome and the citadel of Capua were said to be protected by numerous deep wells or springs sunk round them.

"Bridges," however, cannot invariably be depended upon. Their privilege of safety is liable at any moment to be withdrawn. The peninsula of Caraya, for instance, forming the northern shore of the Gulf of Cariaco, was never known to be shaken until December 14, 1797; yet it has since had its full share of disturbance. The interior arrangement of strata was here doubtless subverted, and the barrier to the extension of shocks from the chalk-hills of the mainland to the slate-rocks of the opposite peninsula overthrown, by the violence of the blow which destroyed Cumana. Centres of disturbance, too, shift and travel in a way to defy anticipation; and the effects of the interference of earth-waves, to which some terrific catastrophes, as well as many apparent anomalies of rest or commotion, are ascribed, can rarely be calculated beforehand. Nevertheless, much knowledge of high value, both practical and theoretical, might be acquired by the continuous topographical study of earthquakes in countries exposed to their ravages. Prof. Milne states that in Japan (where, through his initiative, more has been done in this branch than in any other part of the world), although earthquakes occur there at the average rate of two a day, yet with proper care a building site may be chosen as free from shocks as if it were situated in Great Britain.

The publication now under review is, however, directed to a different and a wider purpose. Prof. O'Reilly had already compiled a "Catalogue of Earthquakes" for these islands (see NATURE, vol. xxxi. p. 351); he has now accomplished a similar task for the whole of Europe with the outlying districts along the Mediterranean and Black Sea shores, designed to form the basis of a map showing the larger geognostic relations of these phenomena. Without some such picture, he rightly observes, no geological map should be regarded as complete. The latter displays results; the former represents the forces at work to produce them. The importance of earthquakes in geological history is great and far-reaching. Their connection with the physical structure of a country is most intimate. Every one of its leading features is related to them, either causally or consequentially. The main lines of jointing and fissuring, with the inseparably associated strike of coast-lines, are, according to a view originated by our author, directly conditioned by the prevailing direction of earthquake shocks. Its correctness can be tested only by statistical inquiries such as those of which he here gives us a laborious example. Other questions of interest awaiting similar elucidation relate to seismic action with reference to coal-fields and to the progress of elevation or subsidence.

Fuchs gives many instances of shocks limited to or originating in carboniferous districts (" Vulkane und Erdbeben," p. 196), and explains them by the progressive decomposition of organic matters quickened by the admission of air in working the shafts. The resulting escape of fire-damp diminishes the volume of the beds; they give way with a concussion, and an earthquake ensues. Extremely curious, in this connection, is the close agreement between the curves denoting the monthly frequency of earthquakes and of colliery explosions pointed out by Prof. O'Reilly (*Trans.* R. Irish Academy, vol. xxviii. p. 297). Each shows a strong and precisely coincident maximum in March, while the earthquake maximum in November is less perfectly matched by a conspicuous increase, one month later, in the number of explosions. The analogy of the equinoctial maxima of auroræ and magnetic disturbances cannot fail to suggest itself; and there are other indications that seismic and magnetic perturbations are not wholly extraneous to each other. Both auroræ and earthquakes, for instance, distinctly gain in frequency during the half-year which includes our winter season and the perihelion passage of the earth; and there have been too many and too close coincidences between their occurrences to be purely accidental.

Sudden changes of level, especially depressions, are an ordinary concomitant of earthquakes. An internal collapse of the strata in some cases produces the shock; in others the shock ruptures supports or overturns foundations long unsound. Degradation by water has perhaps undermined them; contractions have taken place through cooling, through chemical action, possibly through slow evaporation. At last a crash comes, and a tract of land,

deprived all at once of its insecure props, settles down to a lower level, a forest perhaps subsiding into a lake, or the sea over-washing a stretch of shore.

Slower processes of change, however, are probably far more general and effective, and with these seismic relations are still in part obscure. Such changes depend, there is little doubt, upon variations of equilibrium between internal forces of expansion and external forces of repression. Where these are accurately balanced, the bounding surface of the earth remains unaltered; where subterranean heat gets the better of gravity, as through the denudation of large tracts, elevation ensues; where the weight of the superincumbent strata is augmented by deposition, there is slow subsidence. The effects of the earth's secular cooling must evidently, in the long run, be thrown wholly into the scale for contraction; and yet it is to them indirectly that the upthrusting of mountain-ranges is due. These might be compared to the folds and creases of a garment grown too ample for the shrunken body it covers. The terrestrial crust, indeed, is less easily adaptable than an old coat; not a wrinkle in it but represents a series of paroxysms, every one implying a greater or less amount of earth-shaking, past and present. The snap after prolonged strain, the shifting and twisting of rocks, the fissuring and faulting, the slipping and wrenching and grinding of tormented strata in the effort to satisfy the stresses put upon them, all result in earthquake action of the mechanical kind. Thus, mountain-making is essentially a seismic operation, not only while in progress, but in its effects during long subsequent millenniums. This is one chief reason why the lines of earthquake distribution follow so faithfully the general direction of mountain-ranges.

But besides those commotions which result from the catastrophic restoration of disturbed equilibrium, there are earthquakes of the volcanic or explosive class. This species has been defined as an "uncompleted effort to establish a volcano." Such abortive eruptions are occasioned, there is much reason to suppose, by the sudden formation of steam at great depths beneath the earth's surface. They arise where broken and disjointed strata facilitate the percolation of water to volcanic foci. A fractured crust and a plentiful aqueous store are their developing conditions. Hence their frequentation of sea-coasts. Prof. Milne remarks that most Japanese earthquakes originate in the Pacific, and that the steepest coasts are, on the whole, the most severely shaken; as is easily intelligible when we consider the violence of the dislocations necessary to produce them.

Earthquakes may then be broadly distributed, according to their kind, into two systems, now coalescing, now independent of each other. The explosive species follow volcanoes along sea-coasts, the mechanical sort are associated with mountain-ranges; all attend lines of weakness, and are more or less closely connected with the shrinkage by cooling of the terrestrial crust. Thus, every volcanic region is liable to earthquakes; though there are earth-shaken districts which are not volcanic.

The tendency to alignment in volcanoes has often been noticed: Prof. O'Reilly indicates a similar peculiarity in earthquakes, adding that the lines along which they range commonly approximate to great circles. This inference, or suspicion, can be verified only by detailed charting. There are great difficulties, however, in getting a true graphical representation of seismic activity. Not only deficiencies in records have to be contended with, but grave perplexities as to their treatment. They are fully admitted by our author. The number of shocks felt in a given spot is the criterion inevitably adopted; but these may vary to any extent in intensity, or may be the mere sympathetic reverberation of some distant catastrophe. The Lisbon earthquake of 1755, for instance, may quite possibly have shaken every square foot of the globe. The ideal seismic map would be one of earthquake origins, with their attendant areas of disturbance; but this is at present far from being attainable; and we can only acknowledge the indebtedness of science to those indefatigable workers who, like Prof. O'Reilly, promote knowledge by the best *present* means open to them.

OUR BOOK SHELF

Department of Agriculture, Washington: Third Report on the Chemical Composition and Physical Properties of American Cereals, Wheat, Oats, Barley, and Rye. By Clifford Richardson. (Washington, 1886.)

THIS Report is an important continuation of a most valuable work. The object in view is to obtain accurate information respecting the composition of the cereal grains produced in the various States. The grain analysed is in some cases the produce of seed issued by the Agricultural Department, but generally represents the ordinary crops of the district. A complete physical and chemical examination has been made of each sample of grain: the results are tabulated under the head of the State in which the grain was reared. The Report contains 77 analyses of different varieties of wheat grown in Colorado; 179 analyses of the kernel of oats, and 100 analyses of the husk; 57 analyses of rye, and 72 of barley. The extent of variation in composition, the relation of physical characters to chemical composition, and the influence of climate, are discussed. The results are further compared with those obtained by investigations in Europe. At the close of the Report are given some detailed analyses of cereal grains in which sugar, starch, and the albuminoids soluble and insoluble in alcohol, are separately determined. Analyses are also given of the very various products obtained from wheat by roller-milling. The whole is a magnificent contribution to the history of cereals. We now know far more of the characteristics of cereals grown on the American continent than we do of those produced in the United Kingdom. When will an English Agricultural Department inaugurate a similar study?

As we have no space for the details of the results, it is perhaps hardly fair to criticise. We would merely remark that dextrin is not reckoned by the best modern chemists among the constituents of barley, or of other cereal grains that have been thoroughly investigated. The method used for determining starch is apparently one yielding too high results, while the "fibre" shown in the analyses is far below the total cellulose and incrusting matter really present. We call attention to these errors of method, as they are very generally met with, and it is high time that they were remedied. R. W.

Longmans' School Geography. By George G. Chisholm, M.A., B.Sc. (London: Longmans, Green, and Co., 1886.)

ONE point which the recent discussions with regard to geographical education in this country has brought out

beyond dispute is that our teachers have wretched text-books in geography, and Germany has been held up to us as the model to follow in this as in many other respects in regard to geographical teaching. The Germans (as Mr. Chisholm points out in his interesting preface) have had long experience in working out an advanced system of education; they know that a limited period must be turned to account for the thorough teaching of a great variety of subjects, and accordingly they have learned to distinguish between what is indispensable as a ground-work and what must be omitted. In this country the study of geography is mainly a work of memory—the names of towns, rivers, mountains, with their populations, lengths, and heights. This and similar details are precisely those on which the Germans lay least stress, and as Mr. Chisholm has "earnestly endeavoured to guide himself by German examples," he anticipates that his book will appear more remarkable for what it omits than what it contains. Stated in his own words his object has been, in the first place, to draw a mental picture of the different countries and regions of the world, giving due relief to what is most distinctive in each region, and, secondly, to give special prominence to the relation of cause and effect, so as to enable pupils to realise that in geography there is something to understand as well as to commit to memory, in other words, to make geography a mental discipline as well as a body of instruction. Of course there is important work for the memory in geography as in every other branch of education, and this the author recognises, and provides for in his tables and printing. He insists, too, on the vital necessity of maps, without which there can be no adequate knowledge of geography. A text-book is supplementary to an atlas, and does not supersede it. These are high ideals which Mr. Chisholm sets before him; let us see how he fulfils them.

The whole volume contains 320 pages. The first 60 are devoted to an introduction dealing with mathematical and physical geography, which, as explained in the preface, is designed primarily for teachers, and is not intended to form part of the course for the pupils until they have gone through the whole body of the book.

The introduction is followed by a description of continents and countries. Of the 260 pages which remain for this purpose, Europe fills 150 pages, Asia 45, Africa 16, and America 32. The proportions are based on the degree of knowledge which an educated English boy or man should have of the respective countries and continents. Some of the divisions are original. Thus English counties are divided into corn and grazing, the countries of Asia into monsoon and non-monsoon countries. We have specially examined the sections devoted to the countries of Eastern Asia, for the sins of ordinary British school geographies are more apparent here than elsewhere—the sins, namely, of stereotyped inaccuracy, and of strings of names and numbers. Mr. Chisholm has not a superfluous line in any of these sections, the information is of the latest kind, and all the knowledge that the average boy requires of the countries is put in a short space.

As an instance of the care with which the work is done it may be mentioned that the puzzling variations of some Japanese names (*e.g.* Fujinoyama, Fujisan) are given and explained. On the whole, we are convinced that there is at present no school geography in the English language more calculated to give adequate and intelligent instruction in that subject than this, and can therefore strongly recommend it to those teachers who have lamented the absence of a sound text-book. It is to be hoped that Mr. Chisholm may see his way to producing a smaller work about half the size and price of this book for lower classes.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Physiological Selection and the Origin of Species

As Mr. Romanes has referred to my article in the current number of the *Fortnightly Review*, and stated that he is prepared to answer what he terms "the very obvious exceptions" which I have taken to his theory, I shall be glad to be allowed to state, very briefly, what those exceptions are, and to give an illustration of one of the more important of them.

(1) Mr. Romanes makes a great deal of the alleged "inutility of specific characters," and founds upon it his extraordinary statement that, during his whole life, Darwin was mistaken in supposing his theory to be "a theory of the origin of species," and that all Darwinians who have believed it to be so have blindly fallen into the same error. I allege, on the contrary, that there is no proof worthy of the name that specific characters are frequently useless, and I adduce a considerable series of facts tending to prove their general utility.

(2) In support of his view as to the swamping effects of inter-crossing, Mr. Romanes objects to the assumption of Darwin, "that the same variation occurs simultaneously in a number of individuals," adding: "Of course, if this assumption were granted, there would be an end of the present difficulty"; and his whole argument on this branch of the question rests on the assumption being false. I adduce evidence—copious evidence—that the supposed assumption represents a fact, which is now one of the best-established facts of natural history.

(3) Mr. Romanes states, as the special feature of his physiological varieties, that "they cannot escape the preserving agency of physiological selection." He gives no particle of proof of this, while I show that, on the contrary, it is hardly possible for them to survive to a second or third generation. It is on this point that I wish to give an illustration. Mr. Romanes speaks of his supposed variations as "showing some degree of sterility with the parent form," while continuing to be fertile "within the limits of the varietal form"; but I hold that any such variety (beyond single individuals) can hardly exist, while he has adduced no proof whatever of their existence. To show the improbability of their existence, let us suppose a definite case.

In a given species there is born an individual, A, which is infertile with the bulk of the species, but fertile with some few individuals of the opposite sex, *a*, *b*, *c*. Let there be a second individual, E, born from other parents in another part of the area occupied by the species, and fertile only with *e*, *f*, *g*. Other individuals, K, P, R, &c., may have similar relations, each infertile with the bulk of the species, fertile only with a few individuals which may be termed their physiological complements. Now each of these, separately, is a physiological variety, but the whole set, A, E, K, P, R, do not form one, but five distinct varieties. To form one variety all of them must be fertile with the same identical set of individuals of the opposite sex, and this seems to me to be so highly improbable that it must not be assumed till rigidly proved. Yet there is not one passage in Mr. Romanes' paper to show that he recognised this difficulty; on the contrary, he always speaks as if any number of separate physiological variations within one species must necessarily form one variety. It will easily be seen that the chances against any single variety of this nature being preserved are overwhelmingly great. For, first, at least two of the complementary individuals must survive to the breeding-season, and the chances against this are measured by the fertility of the species. If it produces ten young each year, the chances are between nine and ten to one against any one of them surviving. The chances against the *two* complements surviving will be about ninety to one; and then there remains the chances against the two meeting at the breeding-season, for, by the assumption, there is nothing whatever to bring them together but chance, and this may be any number of thousands to one.

There are, no doubt, other possible cases in which the physiological variety might be continued, but, as I have shown in my

paper, the chances against it are always very great. Here, then, are three objections to Mr. Romanes' theory which seem to me to be weighty and fundamental; yet he says, in effect, that he anticipated, and is prepared to answer, them. This, I must say, puzzles me; because in the whole of his lengthy paper, occupying seventy-five pages, I cannot find any adequate recognition of their existence, or any attempt whatever to answer them.

My apology for writing this is that I am shortly leaving England, and wish the readers of NATURE, who may not have seen the *Fortnightly*, to be aware of the character of the objections which Mr. Romanes declares that he anticipated, but apparently thought of too little importance to require any discussion in his paper. ALFRED R. WALLACE

I AM sorry that I have not succeeded in making my meaning clear to Mr. Romanes. I had hoped that my former letter (NATURE, September 2, p. 407) would have given some indication as to my father's views. With regard to the sentence quoted from the "Origin of Species," our views seem to differ so much that it seems useless to prolong the discussion.

FRANCIS DARWIN

Golf Club, Felixstowe, September 13

I HAVE read the numerous notes and letters in recent numbers of NATURE upon the origin of species and varieties with great interest. It seems to me that all your correspondents are raising an imaginary difficulty.

"If it is to the advantage of some particular variety not to resemble the parent form," then that variation must have been produced by some efficient cause acting upon the parent form alone. Is it not obvious that that cause still acting will be still more potent in producing that particular variation when the parent form intercrosses with the variety? This is, of course, supposing that the new variety is suitable to its environment; if it is not so, no amount of "propping up," whether by "amixia" or otherwise, would perpetuate it.

If, as is probably the fact, varieties or incipient species have arisen from individual divergences, amixia would tend to immediately suppress them in the case of animals and diœcious plants, as a new generation could not possibly arise without intercrossing with the parent stock. J. H. A. JENNER

4, East Street, Lewes

I SHOULD be glad to call Mr. Romanes' attention to a letter by Mr. Edmund Catchpool, published in NATURE, November 6, 1884 (vol. xxxi. p. 4), where he will find his theory of physiological selection very clearly put forward.

FRANK EVERSHED

113, Darenth Road, Stamford Hill, N.

Solution Discussion at the British Association

IT was a pity there was no discussion on solution in British Association, Section B, on Thursday last. More than the whole day was taken up with reading a great many papers, some of them having very little to do with the subject, so no time was left for discussion. I was indeed, by the courtesy of the Vice-President and the patience of the few remaining listeners, allowed to make a few remarks, but of course it was only possible for me to indicate that I had something to say.

In the papers referred to a good deal was said of solution being due to purely physical causes. Now this is either a truism or a veil to hide ignorance, and I am sure no one was a bit the wiser. What we want to get at is THE physical cause of solution. Again, a great deal was made of the part of the heat of solution that might be accounted for by the contraction in volume of the solution. This looks very learned and scientific, and no doubt is interesting from some points of view, but even if all the heat could thus be accounted for, it would not advance our knowledge of the cause of solution; it is merely surrounding the subject with cobwebs. The question would still remain, What is THE physical cause of this contraction?, and I maintain it is due to the affinity of all the elements for one another acting as pointed out in my papers on chemical affinity and solution published in NATURE, April 29 and July 22 of this year. The truth is, chemists, for convenience of study, drew a circle and called all within this "chemical affinity," and then forgot that the circle was their own making, and imagined it was Nature's work. This restriction has served its day, and must now be obliterated if we would understand the plainest teaching of the laboratory and make continued progress.

Portobello, September 9 WM. DURHAM

Actinotrocha of the British Coasts

IN NATURE of August 19 (p. 361), which I have only seen to-day, my friend, Mr. J. T. Cunningham, records as a novelty the finding in 1883 of *Actinotrocha* off Cromarty Firth. Without giving an exhaustive note of its occurrence off our shores since the discovery in 1856 of *Phoronis* by the late able and accomplished Dr. Strethill Wright, viz. one species from Ilfracombe, and another on an oyster-shell from the neighbourhood of Inchkeith in the Firth of Forth, the following remarks may be of interest. So long ago as 1858 the late Dr. Spencer Cobbold found *Actinotrocha* near Portobello, as was likely after Dr. Wright's discovery, and I have also since met with it in and off the Forth. Moreover, at the meeting of the Microscopical Society at which Dr. Cobbold read his paper, the lamented Dr. Carpenter mentioned that he had found *Actinotrocha* in abundance off the Island of Arran, probably when working at *Tomopteris* and other surface-forms with his friend, the enthusiastic E. Claparède, of Geneva. Besides these localities, Prof. Kolliker ("Kurzer Bericht an der westküste von Schottland," *Zeitsch. f. w. Zool.*, Bd. v. 1864) describes the occurrence of a *Phoronis* apparently identical with Dr. Wright's *P. hippocrepia* from Millport on the larger Cumbrae in the Clyde, a region in which the steam-yacht *Medusa* from Granton has lately been at work. It is probable, indeed, that *Phoronis* and its larval form (*Actinotrocha*) are more generally distributed round our shores than the scanty notices of them would lead one to suppose. Old shells in and off the mouth of the Forth, off the western shores, and these and other structures in the littoral region on the southern coast of England, as well as the shores of the Channel Islands, will probably produce many examples of *Phoronis*, while the careful scrutiny of the contents of the tow-net in similar localities will yield corresponding results as regards *Actinotrocha*. W. C. MCINTOSH

St. Andrews Marine Laboratory, August 25

The Manatee

I NOTICE in the review of Dr. C. Hartlaub's work on the Manatees, which appears in your issue of July 8 (p. 214), that the geographical range ascribed to that animal on the West Coast of Africa has its southern limit at the Quanza. A reference to earlier writers would, I think, justify us in believing that the manatee was once to be found as far south as the Cape of Good Hope, or else that it has been confounded with the hippopotamus.

Dapper, in his description of the Cape Settlement, speaks both of sea-cows—"zee-koeien of zee duivels, zoo groot als koeien, die bij wijlen te lande gaen weiden"—and of sea-horses—"zee-paerden, een zeer groot en wonderijgelyk zee-gedrocht" ("Naukenge Besch ijonige der Afrikaensche gewesten," p. 266; Amsterdam, 1676).

Here the hippopotamus is evidently the *zee-koe* or sea-cow, which occasionally feeds on dry land. May not the *zee-gedrocht*, the sea-monster, have been the manatee?

For Valentyn, also writing of the Cape of Good Hope, refers very explicitly to the manatee:—

"Onder de zee dieren telt men de zee koejen, de hier zeer veel en ongemeen swaar vallen, alzoo men er zommige van 4 of 5000 ponden gezien heeft. In West Indien *noemt men dit dier Manati* bij de Indianen, en anderen noemen het wel een Lamantine; hoewel er zijn die beide deze dieren nog eenigzins onderscheiden.

"Diergelijk zwaar zee paarden heeft men er ook, hoewel wat verder van de Kaap af, gezien. Zij vallen doorgans kastaniebruin" ("Beschrijving van Kaap de Goede Hoop," p. 115; Dordrecht and Amsterdam, 1726. Eighth volume of "Oud en Nieuw Oost Indien").

But here the manatee is called the sea-cow. What is the sea-horse (*zee-paarden*)? Can it be what Leguat saw at sea on his voyage from Amsterdam to the Cape—which he reached twelve days after the *rencontre?*

"Le premier jour de l'an 1691 nous eûmes le plaisir de voir assez distinctement une vache marine de couleur roussâtre (cf. the "kastaniebruin" of Valentyn) "qui faisoit voir la tete entière,

et quelquefois de la moitié du corps hors de l'eau. Elle était ronde et épaisse et paraissoit plus massive que nos plus grandes vaches. . . . Un de nos matelots nous assura que ces animaux avoient les pieds, comme vous pouvez voir dans la figure que voici."

This figure, however, except for the toes, which resemble fins or webbed feet, is unmistakably the hippopotamus! (See "Voyage et Avantures de François Leguat," vol. i. p. 35; Londres, 1708.) Leguat did not apparently consider it a manatee, for on p. 93 he gives a full description, with plate, of the lamentin or manati, which "se trouve en grande abondance dans les mers de cette Isle" (Rodriguez). The skin is "noirâtre."

Père Tachard plainly calls the hippopotamus the *vache marine*—he is speaking of the Cape: "on voit dans les grandes Rivières un animal monstrieux, qu'on appelle Vache Marine, et qui égale le Rhinocéros en grandeur" ("Voyage de Siam," vol. i. p. 78; Amsterdam, 1688). The plate accompanying is the hippopotamus, and we know that the Dutch colonists have always called this pachyderm the "zee-koe."

Kolbe ("Caput bonae spei hodiernum," p. 167, Nürnberg, 1719) speaks of the "zee kuh," the "meer kuh," the "zee pferd," and the "kuh fisch," all of which he appears to consider different names for the hippopotamus, notwithstanding that "in dem Tartarisch meere grosse Kuh-Fische schwemmern, die grosser als unsere Kühe in Europa waren, aber weder Schuppen noch Hörner hatten." This must be the dugong, surely.

Bogaerts ("Asia," p. 105; Amsterdam, 1711) distinguishes between "zee-paarden" and "zee-koien."

Dampier's mention of the manatee is probably well known:—"While we lay here (Blewfield River, between the Nicaragua and Veragna Rivers) our Moskito men went in their canoa and struck us some manatee or sea cow. Besides this Blewfield River I have seen of the manatee in the Bay of Campeachy, on the coast of Bocca del Drago and Bocco del Toro, in the River of Darien, and among the South Keys or little Islands of Cuba. . . . I have seen them also at Mindanea, one of the Philippine Islands, and on the coast of New Holland." Then follows a full description (see Dampier's "Voyage Round the World," vol. i. p. 33 *et seq.*, also pp. 2, 9, 41, 381, 463, and 547; London, 1729). Dampier also points out that the so-called manatee of St. Helena is really a "sea-lion."

Cape Town, August 4 W. HAMMOND TOOKE

Time Reform in Japan

THE following communication may perhaps interest your readers.

On my return home from America and Europe, I presented a report on the resolutions of the International Meridian and Time Congress, held at Washington last year, to which I was sent as a delegate. A Committee was appointed to discuss the matter contained in my report, and reported favourably. The following decree was issued on July 12, 1886, under the Imperial seal:—

(1) The meridian passing through the centre of the transit instrument at the Observatory of Greenwich shall be the initial meridian for longitude.

(2) Longitude shall be counted from this initial meridian in two directions up to 180°, east longitude being + and west longitude - .

(3) On and after the first day of the first month of the twenty-first year of Meiji (January 1, 1888), the time of the meridian of 135° E. shall be used as the standard time throughout the empire. D. KIKUCHI

Science College, Imperial University, Tokio, Japan

Tremblement de Terre du 5 Septembre

L'ÉBRANLEMENT des couches terrestres, qui peut être considéré comme la suite du tremblement de terre du 27 août, a eu son centre dans le Piémont, dans les environs de Suse, au pied du Mont-Cenis. Le phénomène a été composé des secousses suivantes, qui ont toutes été tres-faibles dans la Suisse.

Secousses préparatoires. 4 septembre, 11h. 35m. soir (heure de Berne) Colombier (Neuchâtel); 5 septembre, 8h. 16m. soir, Briançon (Hautes-Alpes, France).

Grande secousse. 5 septembre, 8h. 55m. soir. Nous en avons des observations de Bienne, Berne, Lausanne, Morges, Genève, Vevey, Aigle, Villars-sur-Ollon, Bex, Monthey, Troistorrents, Sion, Savièse.

Secousses consécutives. 5 septembre, 11h. 55m. soir, Genève; 6 septembre, 4h. 10m. matin, Monthey (Valais); 7 septembre, 0h. 43m. matin, Genève. F.-A. FOREL

Morges, 12 septembre

Lunar Rainbow

A BEAUTIFUL lunar rainbow was plainly visible here for a few moments last evening. The eastern sky being clear, the moon looked fully out from behind dark clouds in the west at a moment when rain was falling lightly. Turning quickly away from her light, in the hope of seeing a bow, I was not disappointed. A semicircle of pale, whitish light, was projected against the eastern sky, much smaller in diameter, apparently, than a sun-bow, and without any traces of colour.

Reflecting on the circumstance that repeated efforts have never, previously, enabled me to see a lunar bow, although the conditions necessary for its formation are common enough, I am tempted to think that the phenomenon can only be seen when the atmosphere is unusually clear. The light issuing from the bow is so faint that the slightest mistiness of the air intervening between itself and the spectator is probably sufficient to, practically, extinguish it. Last night the air here and over the Channel was extraordinarily pellucid, lights on the French coast which are hardly ever seen being plainly visible, while others, nearer neighbours, flashed with most unusual brilliancy.

D. PIDGEON

Arthur Villa, Hythe, Kent, September 6

Aurora

THE aurora seen in Ireland on July 27, and described in NATURE, August 5, p. 312, was visible in this vicinity. It was the finest observed thus far this year, with the exception of that of May 8. Other dates on which the aurora has been seen in this locality recently are as follows: June 29, June 4, and April 14. It has been noted that these appearances of the aurora have been coincident with the return of the disturbed area on one side of the sun (see NATURE, July 22, p. 278), and likewise with widespread and violent storms.

Lyons, New York, August 25 M. A. VEEDER

THE SOLAR ECLIPSE OF AUGUST 29

THE following communication, dated Grenada, September 5, is published by the *Times* from its correspondent with the Eclipse Expedition. It should be compared with the communication made by Prof. MacAlister to Section A at the Birmingham meeting of the British Association (NATURE, September 9, p. 441), and with the article in the same number (p. 437), describing the arrangements for observation.

"The observations of the corona during the last two eclipses, including that observed in Egypt, have been confirmed by the present. Capt. Darwin's observations with the coronagraph seem disappointing, the glare of irradiation from the body of the sun, and not the true corona, being visible on his plates. The bright lines seen in the spectra of the prominences are displaced in such a direction as to prove that there is in them a downrush of gas towards the sun.

"The curious prolongation of the corona observed on several previous occasions to occupy the sun's equatorial plane, does not appear in any of the photographs taken, though it was visible at all the stations except Mr. Lockyer's."

PHOTOGRAPHY OF THE SOLAR CORONA

UNDER the above title we have received the following communication with reference to the results of the recent eclipse observations:—

Accounts have appeared in your journal of my attempts to photograph the corona of the sun without an eclipse. Many of the plates obtained presented appearances which seemed not to myself only, but to several scientific men who must certainly be con-

sidered to be amongst those who are exceptionally competent to give an opinion on this point, to be most probably due to the corona. Plates taken in England about the time of the eclipse of May 6, 1883, and drawn by Mr. Wesley before any information reached this country of the observations of the eclipse, presented not only a general resemblance to those taken during the eclipse, but showed the remarkably-formed rift on the east of the sun's north pole which is the main feature of the corona, as photographed at Caroline Island. It is true that since the summer of 1883 I have not been able to obtain in England photographs which show satisfactory indications of the corona; but the abnormally large amount of air-glare from finely-divided matter of some sort, which has been present in the higher regions of the air since the autumn of 1883, might well be considered a sufficient cause of the want of success. This well-known state of the sky rendered the plates taken by Mr. Ray Woods in Switzerland in the summer of 1884 inconclusive as to the success of the method. During the past year photographs of the sun have been taken at the Cape of Good Hope, and are under discussion by Dr. Gill.

Such was the state of things before the eclipse of August 29. The partial phases of this eclipse furnished conditions which would put the success of the method beyond doubt if the plates showed the corona cut off partially by the moon during its approach to and passage over the sun. As the telegrams received from Grenada and a telegram I have this day received from Dr. Gill at the Cape of Good Hope state that this partial cutting off of the corona by the moon is not shown upon the plates, I wish to be the first to make known this untoward result. I regret greatly that a method which seemed to promise so much new knowledge of the corona, which under ordinary circumstances of observation shows itself only during total eclipses, would seem to have failed. At the same time, I am not able to offer any sufficient explanation of the early favourable results to which I have referred briefly in the opening sentences of this letter.

WILLIAM HUGGINS

Upper Tulse Hill, S.W., September 11

In reply to a similar communication which appeared in the *Times*, Mr. A. A. Common writes to that journal as follows:—

"Dr. Huggins, in his letter in to-day's issue, seems to consider that the failure to get a picture of the moon projected on the corona of the sun during the partial phases of the last eclipse is fatal to his method of photographing the corona; but it is quite possible, and, indeed, probable, that this is due entirely to the state of the sky, for against such unfavourable negative as this we have the positive evidence that the moon has been seen so projected in various solar eclipses, and in one case it has been so photographed. This was by Liais, at Paranagua, in 1858, under conditions that were not, as far as concerns the processes employed, nearly so favourable as those now in use. This single piece of positive evidence, if correct, is of vital importance in showing that the present failure is probably due only to such temporary causes as have prevented Dr. Huggins getting lately such promising plates as those he obtained in 1883.

"Ealing, September 13 "A. A. COMMON"

THE RECENT AMERICAN EARTHQUAKE[1]

THE author gave a brief account of the earthquakes in Eastern Europe of August 27, which seem to have travelled eastwards from Malta to the south of Italy. It is a curious coincidence that the first important indications of earthquake disturbance in the United States took place on that date, when the geyser of the Yellowstone spouted forth and when the first moderately severe shock at Charleston occurred. The principal shock was on Tuesday night, August 31. This is the one which has done most damage, and which was felt over a wider area than any previously recorded in North America. It has, however, been succeeded by shocks, fortunately of less intensity, which have been felt over a still wider area. The later shocks of Thursday and Friday were felt in Nevada and California.

The author gave a description of the earthquake, founded upon the newspaper telegrams and upon a telegraphic despatch which Major Powell had kindly forwarded at the author's request. The latter is as follows:—

"The earthquake is the most severe on record in the United States, and affected the greatest area. Origin along line of post-Quaternary dislocation on the eastern flanks of the Appalachian, especially where it crosses central North Carolina. There were slight premonitory shocks in the Carolinas for several days, moderately severe shocks occurring near Charleston on August 27 and 28. The principal shock, causing great destruction in Charleston, originated in central North Carolina on August 31, 7.50 p.m., 75th meridian time. Thence the shocks spread with great rapidity in all directions, with velocity varying from 25 to 65 miles a minute, over an area of 900,000 square miles, or one quarter of the United States—from the Gulf of Mexico to the Great Lakes and Southern New England, and from the Atlantic seaboard to the Central Mississippi Valley. In the Carolinas it was accompanied by landslides, crevasses, and great destruction of property. Half of Charleston is in ruins; about 40 lives were lost. No sea-wave has yet been reported. A second moderately severe shock occurred at Charleston at 8.25 a.m. September 1. Minor shocks followed at increasing intervals. The principal shock was felt over this vast area in intervals of 15 minutes, and recorded at some principal points on a scale of intensity of 5 as follows:—Raleigh, 4, 9.50 p.m.; Charleston, 5, 9.54; Cedar Keys, Florida, 2, 10.05; Knoxville, 3, 9.55; Memphis, 4, 9.55; St. Louis, 1.2, 10.00; Milwaukee, 3, 10.06; Pittsburg, 4, 10.00; Albany, 2, 10.00; Springfield, Mass., 1, 10.00; New York, 2, 9.53."

Prof. Carvill Lewis has studied a previous earthquake in the North-Eastern States. This ranged along the north-eastern flanks of the Appalachian Chain. The author described the structure of Eastern North America, and the lines of old earth-movements therein to which both earthquakes seem to be related.

The local phenomena of the recent earthquake may be summarised as follows:—Fissures were formed, some running north to south, some east to west, out of which mud and sand were ejected. Several telegrams speak of stones falling from the air, which (if true) must previously have been ejected from such fissures. No tidal wave has been recorded, nor has any alteration of level of land or depth of sea occurred, although the earthquake was noticed at sea off Charleston; but some passing disturbance of the water seems to have occurred at Sullivan's Island near Charleston, for the high water spoken of could not be a spring tide, as the tides then were the neap tides. The accounts agree in the earthquake being accompanied by rumbling noises. Accounts differ as to the direction of the vibratory movement, but it was probably from the south or south-south-west to north or north-north-east, both at Charleston and New York. As usual in earthquakes, wells and springs have been affected; some dried up, whilst water has appeared where before there was none. The natural gas wells of Pennsylvania have been affected, and the supply much diminished. Perhaps the most interesting phenomenon is the

[1] "Notes on the Recent Earthquake in the United States; including a Telegraphic Despatch from Major Powell, Director of the United States Geological Survey." Read at the British Association by W. Topley, F.G.S., Geological Survey of England, President of the Geologists' Association.

outburst in the Yellowstone Park of a geyser which has been quiescent for four years.

All the evidence so far published tends to show that the earthquake was a true seismic disturbance, which was probably transmitted along certain lines of great rock-masses, or along lines of weakness; but details to enable us to determine these points are not yet to hand.

DR. KLEIN'S REPORT ON MILK SCARLATINA

In a recent Report to the Local Government Board, "On Certain Observed Relations between Scarlatina in various Districts of London and Milk supplied from a Dairy Farm at Hendon," Mr. Power has related the circumstances (NATURE, vol. xxxiv. p. 393) under which I became associated in inquiry at the farm in question; and, while briefly indicating certain provisional inferences of my own as to the nature of the malady discovered among the cows there, Mr. Power goes on to promise an account by me of the special features and pathology of the disease. This I now proceed to give.

The cows (I. and II.) which were the first subjects of my investigations had on the teats and udder several flat irregular ulcers, varying in diameter from $\frac{1}{4}$ to $\frac{3}{4}$ of an inch; some ulcers were more or less circular, others extended in a longitudinal direction on the teat. The ulcers were covered with a brownish or reddish-brown scab, which, when scraped away, left exposed a granulating slightly indurated base. The margin of such ulcer was not raised, nor was there any perceptible redness of the skin around. But where I afterwards got the opportunity of watching the earlier stages (especially in animal IV.) it was noticed that a small vesicle made its appearance on a greatly swollen and red teat, in the course of a couple of days assuming the character of the above ulcers. In another cow, an ulcer about $\frac{1}{2}$ inch in diameter, was becoming covered in its central part with a scab, while at its margin vesiculation was still distinctly visible.

As a rule, *i.e.* in most animals, the disease affected the teats, but in some there was also on the lower part of the udder here and there an ulcer. In such animals, patches denuded of hair were noticed on various parts of the skin, the tail and back particularly. In these patches the epidermis was scaly, and the cutis more or less thickened. The animals looked thin, but not strikingly so, except in one or two cases of animals that had only a few weeks ago been admitted to the place, and which therefore had calved comparatively recently (see Mr. Power's Report). As regards the feeding capacity of affected animals, their milking power, and their body temperature, nothing abnormal could be detected.

Two animals (to be referred to as cow III. and cow IV.) became the special subjects of study after they had been removed from the farm to the stables of the Brown Institution.

The temperatures (Centigrade degrees) of cow III. were as follows:—

	Morning temperature	Evening temperature
January 4	38°·8 ...	38°·7
„ 5	38·9 ...	38·9
„ 6	38·8 ...	38·3
„ 7	38·9 ...	—
„ 8	39 ...	39
„ 9	38·8 ...	38·7

The temperature afterwards remained as above without alteration.

The temperatures of cow IV. were:—

January 6	38°·4 ...	38°·3
„ 7	38·7 ...	—
„ 8	38·4 ...	38·8
„ 9	38·6 ...	38·5

In animal III. the ulcers were present, and on January 4 were at their full development and covered with crusts. They gradually died away, and subsequently healed up by January 10, leaving, however, a whitish indistinct flat scar.

When this animal was received there were noticed on its coat several patches where the hair was gone, and the epidermis was rough and scaly.

Animal IV. when received showed several scabs in the skin of the back; it had also muco-sanguineous discharge from the vagina (the animal was in the third month of pregnancy) and redness and excoriation of the mucous membrane of the vagina. One teat, which was much swollen and inflamed, presented in several places brownish crusts. These when taken off left an infiltrated firm sore, from which, when squeezed, a thickish lymph oozed out. Similar crusts were found on other teats and on the udder. The greatest development of the sores in this cow was on January 7. On January 9 the sores were decreasing; the animal was then killed.

On opening the chest it was found that both lungs exhibited in the upper posterior lobes numerous petechiæ under the pulmonary pleura, the peripheral lobules of these parts being much congested. There were numerous adhesions by recent soft lymph between the lower lobes of the lung and the costal pleura, particularly laterally. In the liver there were several reddish streaks and patches, reaching from the surface of the organ to a depth of about a quarter of an inch. In these patches the liver tissue was much softened. The spleen and kidneys, with exception of slight congestion, appeared normal. In the placenta there were numerous petechiæ.

Cow III. was killed on March 12. For some days previously the animal had been getting very thin, notwithstanding its ravenous and excessive eating. On post-mortem examination the following appearances were found:—

In the lungs there were numerous lobules, especially in the peripheral parts, which showed great congestion; there were in addition pleural adhesions; the cortex of the kidney was congested, but its medulla was pale.

Experiments were now made with the matter of the ulcers, with a view of ascertaining whether or not the disease was transmissible to other animals.

On January 7, when the ulcers of cow IV. had reached their maximum development, I took scrapings from some of the ulcers on the udder and teats, having first removed the crust, and inoculated in several places the skin of groin and inside of ear of two calves (1 and 2). For inoculation a superficial small incision (not longer than about a quarter of an inch) was made, passing in an oblique direction through the superficial part of the corium, and into this pouch a particle of the scraping was rubbed.

On January 9, with scraping of ulcers of the cow before she was killed, I inoculated two calves (3, 4), introducing the matter as before into the corium of the groin and of the inside of ear.

Calves 1 and 2 showed during the first three days after insertion of the matter no change at the seat of inoculation.

Four days after inoculation:—There was in calf 1 one place in the groin which promised to become an ulcer. Calf 2 showed on the ear one promising place, the other places of inoculation having nearly healed.—At the same distance of time after inoculation calf 3 showed two promising places on the ear, and calf 4 showed two promising places in both groin and ear. Calf 3 also showed a kind of vesiculation at the margin of the spot inoculated and commencing formation of a crust in the centre. What I call promising places of inoculation were spots that had become swollen and tender, the other and not promising places were spots that seemed healing or were already healed and dry.

On the sixth day:—Calf 1 showed four successful places in the groin; the places had become swollen and enlarged with imperfect vesiculation at the margin and formation of crust in the centre. Calf 3 had four successful places on the ear, and calf 4 had the same number in the groin.

On the seventh day:—In calf 1 all places except one in the groin had nearly disappeared. This place was now a distinct ulcer covered with a crust, on removing which a granulating infiltrated base was exposed. In calf 2 all places of inoculation were decreasing, covered with small scabs, easily detached. In calf 3 the sores on the ear had enlarged to about half an inch in breadth, each of them covered in their whole extent by a brownish crust. In calf 4 all except one place on ear were healing.

On the eleventh day:—Calf 1 had still one ulcer in groin not yet healing. Calf 2 had one ulcer on ear not quite healed up. Calf 3 had four big ulcers still progressing; crusts thick, and corium much indurated. Calf 4 had one ulcer on ear much diminished in size.

By the eighteenth day:—The ulcerations in calf 3 (one ulcer had been cut out for microscopic examination) had all healed up and become converted into flat scars. In the other animals the healing was completed at an earlier date.

Simultaneously with the above experiments several inoculations with materials of the ulcer of cow No. IV. had been made into the skin of the groin of ten guinea-pigs and of three dogs. In the guinea-pigs no result was obtained; but in one of the dogs one place of inoculation appeared swollen and inflamed on the third day. On the fifth day this place was an oblong ulcer of about a quarter of an inch in diameter; the margin was red and swollen, but the centre was without crust (the animal had been frequently seen to lick it). On the seventh day the ulcer was much smaller, and it had nearly healed up by the tenth day.

From these experiments there can be no doubt whatever that by inoculating a particle of matter from the sores of an affected cow a positive result has been obtained in all four calves. In calf 3 this result was best and most striking. After an incubation of about three days the places of inoculation became swollen, tender, and spreading; on the fifth to the sixth day the change was distinct, the successful places having become sores; in the marginal part showing vesiculation, and in the centre formation of crusts. The sore enlarged during the next few days, and on removing the crust a raw surface was exposed, the corium itself being found infiltrated. According to the intensity of the process the retrogressive change sets in later or sooner; in slight cases the healing begins about the ninth or tenth day, in severe cases (calf 3) not before the end of the second week.

Having thus demonstrated this disease of the cow to be directly communicable from animal to animal, I set to work to study its minute anatomy.

The microscopic examination of fine sections through the ulcer of the cow shows the following conditions:—

The corium throughout the whole extent of the ulcer is infiltrated with round cells. This infiltration, though densest in the central portions of the ulcer, is sufficiently pronounced even in the peripheral parts, but it gradually fades away on passing from the ulcer to the normal skin. The infiltration in the deeper parts of the corium is limited to the vascular branches, but in the superficial parts is more diffuse, the papillæ becoming at the same time thicker. This thickening of the papillæ fades off towards the periphery of the ulcer. The most noteworthy changes are, however, present in the epithelium. In the peripheral portions of the diseased part there are present in the superficial layers of the stratum Malpighii close to the stratum lucidum, as also in the stratum lucidum itself, numerous cavities of different sizes. These cavities lie closely side by side; the most superficial ones are either covered by the stratum lucidum or extend between the layers of this stratum. The former cavities descend into the depth of the epithelium; at the very margin of the diseased part they are smallest, and they do not in depth comprise more than the superficial third of the stratum Malpighii. They enlarge in depth gradually as we pass from the periphery of the ulcer towards its centre; at its very centre they involve the whole thickness of the stratum Malpighii. At the same time it is to be noticed that, at the marginal parts, the cavities, although closely placed side by side, are well separated from one another by thicker or thinner trabeculæ composed of epithelium; while at or near the centre the ulcer these trabeculæ get destroyed, and the cavities become confluent, and the covering layers of the cuticle having here also given way, their contents extend on to the free surface of the ulcer. These contents, which go to form what has been above mentioned as the crust, spread thus gradually over the surface, not only of the centre, where the stratum lucidum has become lost, but also over the rest of the ulcer. In the marginal positions, *i.e.* where the superficial layers of the cuticle are still present as cover of the above cavities, this layer (*i.e.* the stratum lucidum) separates the contents of the cavities from the crust. The contents of these cavities consist (*a*) of an albuminous fluid looking, in hardened sections, uniformly granular or containing also fibrinous threads; (*b*) of a few red blood corpuscles; and (*c*) chiefly of round cells or pus cells, the nuclei of which, near to and on the surface, gradually break up into amorphous granular matter.

In the central parts of the ulcer the whole exudation undergoes degeneration into debris, and not only in its superficial, but also in its deeper portions. While some cavities contain very few cells and are filled chiefly with albuminous fluid (granular or fibrinous), others are almost entirely filled with pus cells closely packed together. In the papillæ near the cavities the blood-vessels are engorged and there is also escape of red blood disks.

On a careful examination it is evident that the origin of these cavities is in enlargement of and exudation into the tissue of the papillæ, but only of those portions nearest to the stratum lucidum, and from hence arises formation of cavities in the cuticle. The whole anatomical details of the distribution and arrangement of these cavities recall vividly the conditions observed in the vesicles of cow pock and of sheep pock, and on comparing under a low power of the microscope a section through a sheep pock with a section through the ulcer of the cow now under consideration, the similarity is very striking indeed.

There are, however, anatomical differences between the two diseases. The infiltration of the corium is slighter in the cow ulcer than in the sheep pock, and in the cow ulcer the cavities form in a more superficial stratum of the epidermis.

There is in the disease we are now considering a good deal of infiltration of the epithelium by round cells derived from the cavities, not only into the stratum Malpighii, but also, and particularly in the marginal parts, into the cuticle; the round cells burrowing in great numbers between the scales of this stratum, and ultimately reaching the free surface to join those of the crust.

Fine sections made through the ulcer artificially induced by inoculation in the ear of calf 3, proved its complete identity in anatomical respects with the ulcer in the cow. The infiltration of the superficial corium; the formation of cavities, filled with exudation cells and fluid, in the superficial layers of the epithelium, particularly between the layers of the cuticle; the final destruction in the centre of the ulcer of the covering cuticle; and the extension of the exudation over the free surface to form here the crust, are the same in both instances.

Microscopic examination of the internal organs of cow IV. revealed facts as follows:—

In the lung.—Sections made through the portions above mentioned as containing much congested lobules, show not only great congestion of the blood vessels, large and small, but a large amount of hæmorrhage; blood in substance being present in the air vesicles and infundibula, in the lymph spaces of the interlobular septa, and in the tissue and lymphatics of the pleura. In the latter membrane numerous diplococci are to be met with. Here and there the same diplococci occur in the alveolar wall and in the tissue of the interlobular septa.

Sections through *the liver* show a great deal of change. Under the capsule, as well as in the substance of the liver, there occur, in connection with the interlobular branches of the portal vein, larger and smaller foci of inflammation, consisting in the presence of numerous round cells. Some of these foci are several millimetres in diameter, others are very small. From the interlobular tissue the inflammation extends into the lobules between the liver cells. The liver cells of these lobules involved in the inflammatory process are swollen up, and many of them are undergoing disintegration. In some of these foci, particularly those situated in the vicinity of the capsule, the round cells are so much crowded that given foci look almost like miliary abscesses. The blood vessels are much distended and filled with blood.

Numerous diplococci and short coccus chains occur in the parts surrounding the inflammatory foci. These are particularly numerous near the capsule in the vicinity of inflamed parts.

Sections through the *kidney* showed well-marked glomerulonephritis; infiltration of the sheath of the cortical arterioles with numerous round cells; the epithelium of the convoluted tubules swollen, opaque, and in many places disintegrating.

The lungs and kidney of cow III. showed on microscopic examination the same appearance as in cow IV.; in addition there was a good deal of round-cell infiltration in the wall of the infundibula and bronchi in the lung, and around the cortical arterioles in the kidney. In the blood-clots filling the alveoli and small bronchi of the lung there were present larger and smaller clumps of micrococci.

Search was now made for micro-organisms inhabiting the tissues of the ulcer of the cow, with a view of ascertaining what were present, and afterwards whether any single kind of those found had the power, when dissociated from the diseased tissues and inoculated into healthy animals, of transferring the disease.

Removing the crust, scraping off the most superficial layer, then squeezing the ulcer so as to collect a droplet of lymph, I spread it in thin films on cover-glasses, and dried, stained, and mounted the several specimens in the usual manner. Such a specimen, examined under the microscope, revealed a number of red blood disks, mixed up with large numbers of pus cells, each of which contained two, three, or four small nuclei and remnants of epi-

thelial cells. Amongst the pus cells numerous dumb-bells of micrococci (or diplococci), and a few short chains of the same, were met with. In size these micro-organisms do not differ from those described in connection with foot-and-mouth disease. In many sections—stained in fuchsin, or in methyl blue, or in gentian violet—through the diseased tissue of the cow, as well as that of calf 3, there were found the same diplococci and chains in the contents of the superficial cavities, as well as in the depth of the epithelium. In the latter stratum they were met with abundantly throughout the whole extent of the marginal portion of the ulcer, but not beyond it. In the superficial parts, namely, in the contents of the cavities in the stratum lucidum, the same chains were to be found, provided the pus cells were not too closely packed. They were very numerous in the tissue of the crust, and also in the superficial central portions of the ulcer that had undergone degenerate change. There occurred also in the crust and in the necrotic parts of the ulcer numerous clumps of zooglea of micrococci; but these micrococci are not to be confounded with the chains of streptococci to be presently described, nor yet with those streptococci which are found occurring singly.

From the deeper parts of an ulcer of cow IV. material was obtained with which tubes containing either solid nutritive gelatine, or Agar-Agar mixture, were inoculated. After some days, and in both media, a micrococcus appeared, the growth of which was extremely characteristic. These are its characteristics, in the nutritive gelatine: after 3 to 6 days' incubation at 20° C., the growth made its appearance at the point or line of inoculation, in the form of small points or granules, whitish in colour and tolerably closely placed. During the next few days their number and size increased. At the end of a fortnight the line of inoculation was visible as a streak of whitish granules or droplets, some large, others small, more or less closely placed. On the surface of the gelatine the growth, like a film of granules, spreads slowly in breadth, but even after months remains small. When inoculated into the depth of the gelatine, the channel of inoculation becomes visible as a whitish streak, made up of smaller and larger droplets. The gelatine is not liquefied by the growth. The same characters are assumed by the growth in Agar-Agar mixture and in solid serum. The general aspect of the growth in gelatine, in Agar-Agar, and in serum, is very similar to that presented by the *streptococcus* of foot and mouth disease (see my report of this year upon that malady [1]), but with this difference, namely, that in gelatine tubes the streptococcus of foot-and-mouth disease is a little faster in its growth, and its component granules are a little more distant. Nevertheless, I have tubes of both kinds of organisms in gelatine and in Agar-Agar—tubes which cannot be from their general appearance easily distinguished. In faintly alkaline broth, or in broth and peptone, the micrococcus of the cow ulcers grows readily, and in the same manner as that of foot-and-mouth disease. But there is one test by which the two kinds of organism can be very readily distinguished: the streptococcus of foot-and-mouth disease, when grown in milk, does not affect the fluid character of the milk, whereas milk inoculated with the organism obtained from the cow's ulcer will, if kept for two days in the incubator at 35° C., have been turned completely solid. This difference is a very striking difference, and a few days' growth in milk suffices for distinguishing without fail between the two.

The microscopic examination of a culture in broth peptone, in gelatine, or in Agar-Agar mixture shows that the growth consists of spherical micrococci, arranged as diplococci, and as shorter and longer straight, wavy, or curved chains—streptococcus,—these latter sometimes of great length. As regards the shape of the micrococci, the mode of their division, the branchings of the chains, the presence here and there in the chain of a large element amongst the smaller ones, the organisms of the ulcers hardly differ from the description which I am preparing of the streptococcus of foot-and-mouth disease. The elements of a coccus chain of the foot and-mouth micro-organism are, however, smaller than those of the disease under consideration.

The streptococcus chains of a growth in broth are short during the first few days; but later on, when the growth settles down more into the deeper parts of the broth, the chains become of great length. So also in Agar-Agar tubes of one to two or more weeks' incubation.

A curious fact, to which importance must provisionally attach, is this: In a cow having several of the ulcers on the teats, the fingers of the milker pressing over the ulcers would constantly rub off from the latter particles of matter, and the fingers and the teat being kept moist, this matter would easily mix with the milk as it passes from the teat. To learn whether the milk *while in the udder* contained the streptococci, the following experiment was made: A teat free of any ulcer was milked so as to obtain a few ounces of milk, and from this milk a large number of gelatine and Agar-Agar tubes were inoculated; a second teat of the same cow, affected by an extensive ulcer, was milked to the same extent, and from the milk thus obtained a large number of other gelatine and Agar-Agar tubes were inoculated. In the first series no single tube showed the growth of the above-described streptococcus, whereas in the second series one gelatine tube and one Agar-Agar tube were found to develop the typical growth of the streptococcus.

We cannot draw any certain inference from this one observation, but evidently the experiment deserves repetition.

With a cultivation (a third sub-culture, in Agar-Agar mixture of this streptococcus, I, on February 1, inoculated subcutaneously in the groin two calves (5 and 6) On February 27 calf 6 was found dead. The subcutaneous tissue at, and for some distance around, the seat of inoculation showed much effusion, and the inguinal glands were swollen and red. There was peritonitis, with sanguineous exudation, congestion, and hæmorrhagic spots in omentum and in the serous coat of the stomach. The spleen appeared small and its capsule thickened. The liver was greatly congested. Kidneys were large and much congested. The ileum was much congested in its mucous membrane, and the epithelium detached in flakes. The mesenteric glands belonging to the ileum were greatly enlarged and hyperæmic. Both lungs were congested, the superficial lobules showed so much congestion that they looked almost solid, and were of a deep red colour. A few petechiæ under the pleura. Bronchial glands enlarged and congested. There was pericarditis, and the heart was distended by, and filled with coagulated blood. The organs of the throat were found much congested. The hairy parts of the skin were not examined.

Calf 5 showed on March 7, around the nostrils and lips of the mouth, and on hard palate and gums, numerous irregularly outlined patches not raised above the level of the skin. These patches had a discoloured, brownish, very slightly raised margin, and a paler centre; they were round or irregular, some as small as $\frac{1}{8}$ of an inch, others four to six times larger. The animal was killed on March 8. On post-mortem examination the following appearances were noted: Congestion of some of the peripheral lobules in both lungs; the pleura pulmonalis slightly opaque, numerous soft lymph adhesions between it and the costal pleura; in the spleen several hæmorrhagic patches under the capsule in the shape of bullæ filled with semi-congealed blood; spleen pulp softened and very congested; kidney congested; organs of the throat congested.

There can then be no doubt that a definite disease has been produced in both animals, of which the affection of the lungs is a conspicuous feature, and coincides with, though more pronounced than, the lung disease noticed in cow IV.

In calf 5 there was, in addition, the disease of the skin and in the mouth, which, as the microscopic examination proved, is in a certain degree similar to the disease in cow IV. and calf 3. More in detail, this is what is found as regards the skin: The tissue of the papillæ and of the superficial corium is infiltrated with round cells, and the blood-vessels of the papillæ are distended and filled with blood. In their peripheral portions, their most superficial parts, the papillæ are very much distended by extravasated blood and round cells;—in fact the first rudiments of cavities are forming in them. The same condition, but more pronounced, obtains in the cuticle, where between its layers there are present small cavities filled with blood and round cells, or only fluid and a few round cells. There is, in addition to this, a general infiltration with round cells of the layers of the cuticle. The brownish-reddish colour of the marginal parts is due to this condition. In the central part the cuticle is loosened by the formation of such cavities containing fluid and a few round cells; by this its layers were separated and ultimately detached. In the cavities of the cuticle occur very fine diplococci and chains. So also in the infiltrated and enlarged papillæ, and in the deeper layers of the epithelium in the whole extent of the diseased skin, diplococci and short chains are present.

In neither of these cases of subcutaneous inoculation was there found any rent or breakage of the stratum Malpighii, *i.e.* no real ulcer. The anatomical features here described in many respects

[1] To appear in the Supplement to the fifteenth volume of the Board's Reports.—G. B.

resemble the lesion of the skin in human scarlatina (see my report for 1876). I did not, unfortunately, look at other (the hairy) parts of the skin to see whether there were any such patches in this calf. (Some observations on the kidney of calf 5 are noted in the sequel.)

Examination of the organs of calf 6:

(*a*) *The lung.*—Congestion of all blood vessels, large and small. Transudation of fluid and hæmorrhage into the alveolar cavities of part of some lobules of the lung, while the rest of the alveolar cavities are collapsed, the capillaries around them very much congested; infiltration with leucocytes of the interlobular septa, extending also into the inter-alveolar septa. In some of the lobules next to the pleura the engorgement of the capillaries is extremely great, blood *en masse* filling the alveoli to the extent of producing a state of red hepatisation. The pleura itself is thickened by exudation of fluid and leucocytes. The bronchi do not show any distinct alteration. Numerous diplococci and a few chains are met with in the pleura and in the congested parts of the lobules, in the alveolar wall, and in those alveolar cavities which contain exudation and blood. The bronchial glands show great changes: the capsule and septa being much thickened by exudation and leucocytes; the lymph vessels everywhere filled with round cells; the tissue of the follicles and medulla much swollen.

(*b*) *The liver* shows extreme congestion of all vessels in all parts, inter- and intralobular. The liver-cells are opaque, granular, and atrophic.

(*c*) *The ileum.*—The epithelium of the surface detached and gone; the epithelium of the Lieberkühn follicles loosened, and in most places detached; the mucosa shows great congestion and infiltration; in the superficial layers the villi show hæmorrhage, the tissue being filled with blood corpuscles, fibrin, and leucocytes; and in many spots the superficial layers of the mucosa are necrotic.

The Peyer's glands are much swollen and inflamed; the central portions of their follicles are breaking down.

Micrococci and bacilli pervade everywhere the tissue of the mucosa. The mesenteric glands in relation with the ileum have their capsules, septa, follicles, and medullary cylinders much congested and inflamed.

(*d*) *The kidney.*—The changes in this organ are highly interesting, since they completely coincide with those in acute scarlatina nephritis in man: great congestion of the cortex, leading in some parts to hæmorrhage into the parenchyma; glomerulo-nephritis with exudation of albuminous fluid and blood into the cavities of the Malpighian corpuscles; granular or opaque swelling of the epithelium of the uriniferous (convoluted) tubules, with degeneration into granular debris of many of the epithelial cells; miliary foci of aggregations of round cells around small bloodvessels; congestion of the medulla.

[The kidney of calf 5 was also examined microscopically, and the changes were exactly the same as those found in the kidney of calf 6, viz. congestion of the glomeruli, glomerulo-nephritis, transudation of albuminous fluid and red blood corpuscles into the cavity of Bowman's capsule; opaque swelling of the epithelium of the convoluted tubules; granular disintegration of the epithelium in many places; infiltration with round cells around some arterioles of the cortex; and congestion of the medulla.]

(*e*) *The heart's blood* was examined for organisms, and in it, by the staining with Weigert's gentian violet, a few diplococci and a few chains could be distinctly detected.

Cultivations were made with this blood in tubes containing Agar-Agar mixture, and a growth of the streptococcus was obtained in all respects identical with the streptococcus that had been employed for inoculation of this animal.

In view of the whole of this evidence, I consider it conclusively established that this streptococcus is identical with the virus of the cow disease.

We have, then, inoculated subcutaneously with sub-cultures of the streptococcus these two animals, calves 5 and 6, with the result of producing a general disease, which in many respects bears a close resemblance to human scarlatina. The minute anatomical characters of the eruption on the skin around the nostrils and mouth in calf 5 is of much significance in this connection, as also is the disease in the liver in both animals, and above all, the disease in the kidney. This latter organ corresponds so closely with a kidney of an acute case of human scarlatina that sections made of the one and compared with those of the other, of which I preserved a large collection from my former investigation into the anatomy of human scarlatina (see Medical Officer's Report for 1876), show no difference whatever.[1]

The outcome of the investigation thus far, and it is of importance until further differentiated observations shall have been made, may be stated thus:—By inoculating the virus directly taken from the local disease (the ulcer on the teats) of the cow into the corium of the calf the same local disease is produced, namely, a change in the skin, which commences as a congestion of the papillæ and corium, and an exudation of fluid and leucocytes. This leads, in the superficial parts of the epidermis, to the formation of cavities, which, enlarging, and extending, and opening on to the surface, and extending into the depth, ultimately lead to the formation of an ulcer. But the virus, in the form of an artificial cultivation of the streptococcus derived from the above ulcer of the cow, when inoculated into the subcutaneous tissue, that is, when introduced almost directly into the vascular system (for all matter injected subcutaneously is easily absorbed by the lymphatics and carried into the blood system) sets up a general disease resembling to a considerable degree in its anatomical features human scarlatina.

Furthermore, as respects the concern that cow's milk may have in the communication of disease—the consideration which led to the present investigations—we have some facts which appear to me to afford very suggestive indications for further pathological study. As I have pointed out on a previous page, it would seem that the milk pure does not contain the organism, but (whether or not this observation be confirmed) the milk during the act of milking is pretty sure to become contaminated by the fingers of the milker bringing down into the milk particles from the ulceration on the teat. The organism contained in these particles would find in the milk a good medium in which to multiply. Such milk would then practically correspond to an artificial culture of the streptococcus, such as we have found capable of setting up a general disease, when inoculated subcutaneously into calves. It is true we have as yet no experience of the inoculation of a known milk sub-culture into the human subject, but in the case of calves we have learnt that the general disease resulting from inoculation of an Agar-Agar sub-culture had characters closely allied to, if not identical with, human scarlatina. Then, feeding of animals with the cultures has not yet been tried, so that at present we are without information as to the characters of any disease that may be produced in calves by that means; whether or not calves fed with milk sub-culture of our streptococci exhibit the same pathological states as we have found to be produced by inoculation of calves with an artificial culture—states that bear so marked a resemblance to those of scarlatina in the human subject. In order completely to understand these and other relations, more experiments are required, and these I hope soon to have an opportunity of making.

Until I am in a position to state at greater length the peculiarities of the infective phenomena of the disease under consideration, I refrain from further comment on its various interesting and promising aspects.

THE BRITISH ASSOCIATION

SECTION E

GEOGRAPHY

Opening Address by Major-General Sir F. J. Goldsmid, K.C.S.I., C.B., F.R.G.S., President of the Section

However diffident I may feel in undertaking the duties of President of the very important Section of Geography at this anniversary, I have no right to take shelter under that diffidence for any shortcoming in the fulfilment of my task. All I would seek at your hands is indulgence for one whose training and antecedents have scarcely fitted him for appearing before you in a quasi-professorial capacity, and whose brief tenure of a Presidential chair at a meeting such as this must be regarded as rather an incidental passage in the annals of the British Association than a fair illustration of its *modus operandi*, or principle of selection in respect to its officers.

As to the subject of my opening address, I know none more befitting the occasion than the means of popularising the branch

[1] Referring to the commencement made in 1882 of investigation of the results producible in the cow by inoculation with the material of human scarlatina, see p. 67 of report of that year, I would propose that this study be extended without loss of time.

of science to which the meetings in this Section will be devoted, and thus attracting towards it that attention which it merits—nay which, in this our country if anywhere, it demands and necessitates.

The question is a wide one, but I will endeavour to narrow the field of its discussion to suit our purpose of to day, and keep within reasonable limits. A few words will suffice to lay before you the programme. It embraces: first, the uses of geography, an exposition of which should prove, and a due apprehension of which should admit, the necessity of its inclusion among the special studies of public schools; secondly, the mode of imparting a knowledge of geography so as to render it at once practical and engaging; and finally, such illustrations of modern travel and research as may serve to demonstrate how urgent is the study of geography to all classes in this country.

Before closing the subject, I shall endeavour to draw your attention directly, if somewhat cursorily, to the progress made by travellers and geographers in furthering what I may for the nonce describe as the objects of their profession during the past year, or since the last annual meeting of the British Association, at Aberdeen. But I shall only dwell upon such instances of geographical progress as from their character and locality come within the range of my personal experience, and serve to illustrate the main argument of this address.

To begin then with the uses of geography. There are doubtless many who will say demonstration here is superfluous, and that if its use was not admitted it would find no place in school studies, which is contrary to fact in many instances; there would be no primers or elementary works on the subject, whereas they may be reckoned by the score; books of travel would be rather entertaining than instructive, a charge which many recently published volumes would disprove; and so forth.

Some again will argue that its uses, such as they are, must be restricted to the few specialists who aspire to be geographers, and that for the million it is enough to carry about a rough idea of the four quarters of the globe, the principal countries and capitals in them, and a sufficient amount of preliminary instruction to understand Bradshaw and Baedeker. A third, and perhaps the largest category among educated people, consists of those who are indifferent to the whole question, and are content to find in geography either an honoured branch of science, or a mere nominal study, according to the views of the latest speaker, or most plausible reasoner. If it be allowable to apply things holy to things profane, no truer illustration of this class can be given than the Scriptural definition of men who receive seed "in stony places."

To the first of the above I would say that the place which geography holds among school studies is not that which it ought to hold if its uses were understood and appreciated. Primers and elementary books already published are good enough in their way, but the instruction they contain is not seriously imparted; and it may be that something fitter and more attractive to the beginner could be produced. At present all school-books on geography may be said, as a rule, to be consigned to the shelf of secondary subjects; and this is not the treatment which should be reserved for a study of such real magnitude. By and by it will be my endeavour to establish by argument and example the indisputable character of its importance.

For those who look upon geography as a profession which needs rather separate training than general education, and would prefer to leave its acquirement to travellers aiming at distinction, specialists in Government employ, and the more zealous and scientific Fellows of the Royal or any other Geographical Society, I can only express my regret that the delusion under which they lie unfits them so thoroughly to understand and much less satisfy the wants of a rising generation. By denying the universal character of the study they clearly misapprehend its true scope, and are dwarfing it to within the narrow limits of a conventional school task.

As a matter of State or public school education the science of geography should in truth be elevated, not degraded. In my humble opinion it should be placed on a par with classics, mathematics, and history, with each and all of which it has affinity. Undoubtedly there are accomplishments which come, as it were, of themselves, or are the outcome of lightly-sown seeds in the home. These for the most part are rather mechanical than mental, though some may have advocates to claim for them intellectual honour. But a knowledge of geography is not to be so acquired: it will not come like handwriting with incidental practice, nor is it to be gained by mere travelling. To move from place to place, whether across seas or continents, or both, to go round the globe itself and visit every important country and capital in the track chosen, even to prefer byways to railways, and search into obscure and hidden spots rather than those which are more generally frequented—all this process affords admirable matter for the note-book of the man of the world and observer, but will not educate in geography, unless the student himself has a serious purpose to turn his wanderings to the account of science. The cursory description which would apply to men and women, cattle and conveyances, hotels and caravansaries, restaurants, coffee-houses, and the like, in a moving panorama, is not always suited to bring out in bold relief the physical aspects of a country.

To the indifferent and wavering, to those who would wish to promote the study of geography if they could feel persuaded that it needs promotion, but who would leave to the better judgment and experience of others the decision on the whole question; to those who are content to accept the institution of a professorial chair in honour to the science, or to leave geographical study to the primitive teaching of their own childhood, whichever course be most in accordance with the temper or fashion of the times—I can perhaps do no better than appeal on the grounds of urgency—in other words, of the real importance of the cause for which, in common with abler and worthier advocates, I would now most earnestly plead. . . .

. . . I almost seem to be treading upon the threshold of platitudes when seeking to explain why geography should be useful to young men of ordinary culture, for whatever career they may be destined. In some cases it is naturally more urgent as a study than in others. The military man, for example, should be more or less a scientific geographer. His profession may require him to survey and describe new regions; and a campaign over a beaten track should find him acquainted with the minute topography and physical aspect of places, at least the names of which are familiar household words. The sailor should in like manner bear in mind the configuration and character of sea-coasts, and carry about the landmarks of his own observations as well as those to which he may refer in books. To both must geography be eminently a professional study. But, considering the enormous extent of our Indian Empire and colonies, and the many foreign States with which we must have intimate relations, is any Englishman, I would ask, competent to discuss, much less to serve, the interests of his country who knows nothing of the physical features, resources, products, population, and statistics of these? It seems to me to be the duty of every loyal subject and citizen, high or low, rich or poor, to seek information on these heads wherever it may be obtained.

But of all men who should realise geography in its broad, comprehensive sense—both as an aid to history, and as a science to which history may be subordinate—first in order is the statesman, in whose province falls the disposal and partition of countries or regions. What should we say of the judge—we may be thankful there are none such on the English bench—who not only gave his decision without mastering the merits of the case before him, but who was also ignorant of the law and precedents which should guide him in the treatment of those merits? The argument might apply with equal force to other callings from the members of which professional opinions or decrees are required by their fellow men. Why, the evil would be so great and so palpable that its existence would not be tolerated for a single day: and the only reason why it *is* allowed to prevail in matters geographical is that though equally great in respect of these it is not equally palpable. The statesman may not know the situation of this or that particular palce, nor its products and resources, but neither does the public. One is not taught geography any more than the other; so that while ignorance and error are brought to bear on a spurious judgment, the critic is not in a position to point out the real flaw, and the blunderer escapes the scathing condemnation which would otherwise await him in the columns of the morning paper.

Let us suppose a case by way of illustration—a case which conveys no exaggerated idea of what happens, or may happen in the course of a year—a case which without being an actual occurrence has in it the flavour of actual occurrences. There is a large tract of land in the far West or far East, it matters not which. All that is known about it is that it is called Laputa or Barataria, and that it is situated in the central part of a region or continent so vast that it might be reasonably called the largest quarter of the globe. Well: it is encroached upon by a powerful neighbour, and England requires the preservation of that land's integrity

and independence. Her best instructors on the matter have told her that such is her interest, and she believes them. Intervention, therefore, becomes necessary; negotiations ensue; and the whole question revolves itself into a partition of territory and demarcation of boundary—in other words, the question becomes one of geography—what I should call, for reasons to be explained hereafter—Political Geography. Who, if not the ruling statesman, should know the true principle on which to deal with a large settlement of this nature—one, it may be, involving ethnological, commercial, humanitarian, quite as much as territorial, considerations? Who, if not the agent on the spot, should know the details to regulate the application of the principle? But the statesman should be in full possession of his agent's details, and be capable of appreciating them not only from the latest reports supplied, but from a certain insight into the matter obtained from early study. He should have been coached in that comprehensive kind of geography which would have embraced the particular information required. Under present arrangements it is not so. The geography taught at schools is too simple or too scientific—too complex or too superficial; in any case it is not the geography which would benefit the Cabinet Minister in solving a territorial difficulty any more than would those "ingenuæ artes" which have so strong a civilising influence on the natural man. Experience in classics may forestall the faulty quotation and false quantity, but fail to suspend the false move on the political board. And it need not be said that, while the first, in point of fact, affects the speaker only, the last concerns the happiness of the million.

We now reach the second consideration: the mode of imparting a knowledge of geography so as to render it at once practical and engaging; and I may be pardoned if I dwell upon this somewhat lengthily, for it involves the gist of the whole question before us. It is always easier to detect a flaw than to find a remedy, and in the present case the flaw is generally admitted by experts. There may be differences of opinion on its character and extent, but apparently there are none on its existence. I shall have to recur to the first, but would ask leave to dismiss the last as established. We are told on excellent authority that in our own country the elements of success in geography are wanting, and the conclusion has been practically accepted by the representative Society of this branch of knowledge. The remedy has been suggested, and in a certain sense partially applied, but a great deal more remains to be done, and the many views entertained and expressed by competent men on the claims and requirements of geography in England render necessary a short review of what may be called the "situation," including notice of work achieved in the direction of reform. . . .

Of late years the Royal Geographical Society, in pursuance of its originally expressed aims and objects, and strong in the experience of a long and prosperous career, has endeavoured to arouse the rising generation to a sense of their shortcomings as regards the particular science in the promotion of which it has its own *raison d'être*. It granted prizes to such public schools as chose to compete for them, and after sixteen years' trial discontinued the grant, owing to unsatisfactory results. It opened correspondence with schools and colleges, and made other judicious and laudable attempts to evoke sympathy and support. But all its proceedings have been as it were preliminary, and may be considered rather as foundation-stones of a temple of success than the outer walls or any visible part of the building itself. A more recent attempt to reach the masses was the Exhibition of Educational Appliances. Objects used in geographical instruction at home and abroad were collected and arranged in galleries hired for the occasion, and the public were invited to inspect them. At the same time appropriate lectures were periodically delivered, by competent and experienced men, to the visitors, many of whom were not merely interested amateurs, but persons actually engaged in school teaching. Attention was called to the fact that the Exhibition was purely educational; that there were in it specimens of German, Austrian, and Swiss maps, executed with a finish and detail unusual in our school maps at home; but that as the Society's inquiry embraced Universities as well as schools, part of the appliances exhibited were used in Continental Universities, though in reality some of the finest maps shown were found also in the higher schools of Germany and Austria. Besides maps, there were in the collection globes, models, and text-books, the presentations not being confined to countries visited by the inspector, to whom the task of collection had been intrusted, but from others also; and these were further supplemented by contributions from British publishers.

The result of this new departure—if the term be allowable—was pronounced very satisfactory, and at the close of the Exhibition, or in the spring of the present year, the Council considered what would be the next best step to take in furtherance of their desire to raise the character of geographical study. At a later date, on the recommendation of their Educational Committee, they resolved on addressing the Universities to the effect that chairs or readerships be instituted similar to those which were at that time filled in Germany by Carl Ritter at Berlin and Profs. Peschel and Richthofen at Leipzig. In carrying out the resolution alternative schemes were submitted. The Council would appoint, under approval of the University authorities, a lecturer or reader in geography, paid out of the Society's funds, he being accorded a fitting local status; or each University might join with the Council in the matter of payment, and a reader be appointed by a committee on which the Society should be represented. . . .

It will thus be seen that special efforts have been made and continue to be made to popularise a science which has never, so far as can be ascertained, held its proper place in the educational programme of our schools or Universities. We must not, however, lose sight of one important consideration. More remains to be done than to institute a chair, a professorship, a readership. It must be clearly understood on what general lines of study we are about to proceed. Is geography to be taught in its full, comprehensive sense, as something involving a knowledge, more or less, of mathematics and astronomy, of ancient and modern history, of ethnology, zoology, botany, geology, of men and manners, laws of nations, modes of government, statistics and politics, something requiring in the disciple a quick ear, a searching eye, an appreciation of scenery and outer subjects as well as physical aspects of country, a power of picturesque but an adherence to accurate description? If so—and I believe I have only stated the qualifications of the travelled and finished geographer—would it not be well to inquire whether the component parts of the science should not be reconsidered, and a subdivision effected which would make it easier to deal with than geography as now understood, under the terms physical, political, and perhaps commercial? . . .

Not six months ago I wrote as follows:—"We are authoritatively told that, at one of our greatest public schools, which may be fairly taken as representative of its class, there is no systematic teaching of geography at all, but 'that in the history lessons, as well as in the classical lessons, a certain amount of geography is introduced incidentally.' Again, if we look at the Universities abroad, it has been found the custom, until quite lately, both in France and Germany, to combine the chairs of geography and history under one professor. Now the 'incidental' character of geographical instruction is a tacit declaration of its unimportance, which every day's experience shows to be without warrant; and its combination with history may be an expedient to render it less distasteful than it appears as a separate study. But a useful hint may be taken from the Continental practice, and a partial fusion of two departments effected, which would commend itself to common-sense, and, to judge from the recorded opinions of certain of our educational experts, might not be objected to by head masters in England collectively. Let us, then, endeavour to extract from the lessons of conventional geography that part which is inseparable from the study of nations and people, and place it under a new and more appropriate head. In this view, so-called 'political geography,' stripped of its purely scientific belongings, would be taught in connection with history, and made an essential ingredient in the early training of British statesmen, whose after-reputation should be more or less the outcome of a University career, the grounding of a public or grammar school, or private tuition. It is difficult to reconcile the amalgamation of what may be considered 'scientific' geography with history. One is as thoroughly apart from the other as geology is from astronomy."

The meaning of the verbal combination "political geography" requires some kind of analysis. Conventionally, and in an educational sense, it is the description of the political or arbitrary divisions and limits of empires, kingdoms, and States; their inhabitants, towns, natural productions, agriculture, manufactures, and commerce, as well as laws, modes of government, and social organisation—everything being viewed with reference to the artificial divisions and works made by man. Accepting this interpretation of its objects, who can hesitate to admit its palpable and immediate relation to history? The mathematical science which investigates the physical character of territory and territorial boundaries is in this case but a secondary requirement, and

can be always fairly disposed of in the recognition of results. Otherwise, we have simply commercial geography with ethnography, and considerations which we may call political in the present but which are undoubtedly historical in the past. Surely, then, it would be wise and reasonable to combine the studies of history and political geography—putting a wider interpretation than the conventional one upon the latter designation in such a manner that the two together should be just the sort of *pabulum* dispensed to the rising generation of statesmen, diplomatists, and all who aspire to the name of politician, in its higher sense of capability to promote as well as to discuss the national welfare.

And admirable lecture on "Geography in its Relation to History" was delivered by Mr. James Bryce—the late Under-Secretary for Foreign Affairs—in connection with the recent London Exhibition of Geographical Appliances. Those who are acquainted with it will readily understand why I pause to remark on its enlightened teaching; to those who have not that advantage I would explain that it seems to embody the arguments of Modern Thought on the important question we are now considering, and that a brief allusion to it is therefore no irrelevant introduction here. The lecturer, seeking to demonstrate that history and geography touch one another in certain relations and interests, laid down the proposition that man is, in history, more or less "the creature of his environment"; that "on one side, at all events, he is largely determined and influenced by the environment of nature"; and that "it is in discovering the different effects produced on the growth of man as a political and State-forming creature by the geographical surroundings in which he is placed" that one point of contact is found. He, moreover, maintained that man, "although he may lift himself above his environment, cannot altogether escape from its power." Dividing the influences thus exercised into three classes, he showed that those arising from the configuration of the earth's surface affected movements of races, intercommunications, and barriers of separation; that those belonging to climate affected the occupation or abandonment of particular localities on the score of health, fertility, or non-fertility of soil, and consequently commerce and cultivation; and that those which owed their existence to natural products unmistakably directed the energies of peasantry and people into certain fixed channels of enterprise—a result which applies to the zoology as well as to the mineral and agricultural resources of a country. He made the very true observation that the "animals affect man in his early state in respect to the enemies he has to face, in respect to his power of living by the chase, in respect to the clothing which their furs and skins offer to him, and in respect to the use he is enabled to make of them as beasts of burden or of food"; and he therefore concluded that "zoology comes to form a very important part of the environment out of which historical man springs." A volume might well be written on this suggestive theme alone; and if, as I believe, the proposition of a human being's dependence on environment be admissible in its entirety, what a field of speculation is open to the inquirer! A condition held applicable to the unreckoned millions of to-day must have had a marvellous effect in giving character to original Man!

This conception of man's environment supposed heads or branches of geography, all bearing upon history, which might be distinguished by names such as ethnological, sanitary, commercial, linguistic, political and military, legal—the last leading to the consideration of the Suez Canal and sea-channels in which several States have interests. As time, however, will not allow me to quote the lecturer's apt and well-put illustrations which followed, I may mention that the express object with which they were introduced was to show how "the possession of geographical knowledge, and a full grasp of the geographical conditions" with regard to some of the leading countries of the world, "will enable a person studying their history to make the history more intelligible and real." In strict conformity with this opinion, and in the conviction that the want of geographical knowledge and "full grasp" of geographical conditions will betray men in power to commit dangerous mistakes, calculated to injure the national prestige and credit, and men out of power to become their upholders in error, I would express the hope that, in any future arrangements which may be perfected for the better education of our countrymen, while physical and scientific geography are invested with a degree of prominence and honour to which they have hitherto never attained, that branch of study which we have been accustomed to call political will be reconsidered and, if necessary, newly defined by competent men. The conclusion at which I have myself arrived—one which I am quite ready to abandon before the arguments of sounder reason—is that we have here something which belongs mainly to history, and, in such light, its scientific should be separated from its non-scientific elements. A partition should be made which would equally suit the mind of the student whose tendencies are rather towards metaphysics than mathematics, as of him who is a votary of practical science only. I do not presume to touch upon the action of Universities, except to say that I can conceive no better example could be afforded that the intellect of England had due regard for the material interests of England than by the creation of a chair for scientific geography and the relegation of that which is non-scientific to the chair of history. . . .

Time warns me that I have detained you long enough, and that if my illustrations apply to the argument intrusted to your consideration, the application should at once be made evident. To my own mind the bearing is clear. A Boundary Commission represents the three branches of Science, Research, and Diplomacy—in other words, all that comes under scientific geography and political geography. The first, you will understand, comprises the survey of country, mapping, and determination of localities. The second has to do with the definition of territorial limits, and, in such sense, with history, ethnology, and laws of nations. That all this has been done, and well done, on the present occasion is not disputed, any more than that enlightened attention will be given to the due disposal of results. But are not these matters of sufficient importance to be taught as daily lessons in our schools, and presided over in University chairs? Even those barren and desolate lands of which we have now spoken—and I have myself traversed many miles of such, some, indeed, in the near vicinity of the Perso-Afghan frontier, between Herat and Farah—they may have a meaning which can only be understood by the initiated, by those who have made them a long and seriously-undertaken study. To the many they are but miserable deserts displayed in incomplete maps; to the few they may have a value far beyond their outer show. Were I asked to sketch out the kind of manual which might be useful in preparing officers for dealing with questions such as these, I would solicit reference to a late paper which I contributed to a quarterly journal, and which I have once before quoted. In it I stated:—

"Asia itself is a stupendous study, but the difficulties may be smoothed to the learner by the judicious employment of method which, after disposing of essential generalities, would naturally tend to division and subdivision. The first would imply a region such as Turkestan; the second, a group of States or single States only, such as Bukhára and Khiva. Given, then, a particular area, the next consideration should be to explain its physical geography. This should comprise the scientific description of its mountains, rivers, and valleys. Its geography should be comprehensive in respect of direction, elevation, watersheds, and connection with plains and plateaus; its hydrography should treat of sources and mouths, basins, drainage, and connection with lake and swamp. Climate and the more important forms of animal and vegetable life should follow in due course; indeed, something of geology, zoology, and botany, and it may be more besides, might reasonably be added to satisfy the requirements of purely scientific teaching. After science, history would follow, and, joined to history, an account of the religion, manners, and customs of the people, as affected by the historical narrative; a statement of the artificial lines of separation which have replaced natural boundaries in consequence of the wars, revolutions, or arbitrary changes which have characterised certain reigns or epochs; an exposition of the form or forms of government in vogue at different periods; and, finally, a chapter on trade and commerce, including a notice of indigenous products and manufactures. Maps, applicable to relations of territorial changes, would be of immense value; and an historian's criticism on these relations, if offered in that fair spirit which alone is justified in composing history, would be an indispensable complement." . .

REPORTS

Second Report of the Committee, consisting of Prof. Balfour Stewart (Secretary), Sir W. Thomson, Sir J. H. Lefroy, Sir Frederick Evans, Prof. G. H. Darwin, Prof. G. Chrystal, Prof. S. J. Perry, Mr. C. H. Carpmael, Prof. Schuster, Capt. Creak, and Mr. G. M. Whipple, appointed for the Purpose of

Considering the Best Means of Comparing and Reducing Magnetic Observations. Drawn up by Prof. Balfour Stewart.—It is with deep regret that the Committee record the death of one of their number—Capt Sir Frederick Evans, so well known for the valuable contributions which he had made to terrestrial magnetism. His eminent scientific qualities combined to make him a greatly esteemed member of this Committee, who now deplore his loss.

The Committee have added to their number the following gentlemen:—The Astronomer-Royal, Mr. William Ellis, Prof. W. G. Adams, and Mr. W. Lant Carpenter. They could hardly consider their list complete without the addition of the first two names, and they are glad that, although not members of the British Association, these gentlemen were not unwilling to serve on one of its Committees.

Since the last meeting of the Association Mr. G. M. Whipple has made a comparison between the method of obtaining the solar-diurnal variation of declination adopted by Sir E. Sabine and that of Mr. Wild. These methods were applied to three years' observations at the Kew Observatory, and the results were compared with those deduced by the Astronomer-Royal from the same three years at Greenwich. The comparison will be found in Appendix IV. to this report.

The Committee think that this comparison deserves careful study, but they do not feel themselves able to pronounce as yet upon the comparative merits of these various methods. Nevertheless, they are of opinion that it is highly desirable to record the daily mean values (undisturbed) of the three magnetic elements side by side with their solar-diurnal variations.

It will be seeen by Appendix III. that Sir J. Henry Lefroy has continued his comparison of the Toronto and Greenwich observations. He has obtained from the smooth curves—that is to say, taking Mr. Wild's method—results which appear to show that the turning-point of the declination is decidedly later in local time at Toronto than at Greenwich. Sir J. H. Lefroy attributes this to the fact that these two stations are on different sides of the Atlantic.[1]

Appendix II. exhibits, by aid of a diagram, an interesting comparison of Senhor Capello between the diurnal variation of the inclination and that of the tension of aqueous vapour. It is remarkable to notice the great similarity between these variations; a similarity which holds separately for each month of the year. Senhor Capello hopes that these results may be confirmed by a more extended series of observations.

The researches to which allusion has now been made refer to the solar-diurnal variation, excluding disturbed observations. With respect of disturbances, Sir J. Henry Lefroy has continued his comparison of Toronto and Greenwich, and his results are indicated in Appendix III.

Prof. W. G. Adams has, it is well known, made, in connection with another Committee, extensive comparisons between the simultaneous traces of magnetographs in various places. He is at present engaged on such an undertaking, and the Committee are in hopes that when this is completed he will give them the benefit of his experience. The subject is an extremely interesting one, and it seems not impossible, judging from the Greenwich results as obtained by Sir G. B. Airy, that magnetic disturbances may be in a great measure due to earth-currents, so that an easy approximate method of recording the latter may be obtained from magnetograph indications.

The Rev. S. J. Perry and Prof. Stewart (Appendix V.) have completed their preliminary comparison of certain simultaneous fluctuations of the declination at Kew and at Stonyhurst in a paper which has been published in the *Proceedings* of the Royal Society, No. 241, 1885. The results are virtually those which were stated in the last report of the Committee. The comparison is being continued and extended.

Prof. Stewart and Mr. W. Lant Carpenter (Appendix VI.) have given the results of other four years' reduction of Kew declination disturbances classified according to the age of the moon. These are very similar to the results of the first four years given in our last report. The same observers give a comparison, extending over four years, between declination disturbances and wind values, which appears to them to show that there is some relation between these two phenomena. They are anxious to continue and extend both these inquiries.

Prof. Stewart has pointed out certain general considerations which appear to indicate that the solar-diurnal variation may perhaps be caused by electric currents in the upper atmospheric regions. Dr. Schuster has likewise made a preliminary application of the Gaussian analysis, tending to confirm the hypothesis that currents in the upper regions are the cause of these variations.[1]

By this analysis Dr. Schuster obtains certain relations between the solar-diurnal variations of the three magnetic elements which ought to hold on the hypothesis that these variations are caused by currents in the upper atmospheric regions. One of these is that the horizontal force component of the daily variation ought to have a maximum or minimum at the time when the declination component vanishes—that is to say, attains its mean position. Another is that the horizontal force ought to be a maximum in the morning and a minimum in the afternoon in the equatorial regions, while in latitudes above 45° the minimum ought to take place in the morning. A third is that in the equatorial regions the maximum of horizontal force ought to be coincident with the minimum of vertical force, and *vice versâ.*

These conclusions are sufficiently well confirmed by observations, and thus render hopeful the first attempt to apply the Gaussian analysis to the solar-diurnal variation.

The appendices of Capt. Creak (I.) and of Dr. Schuster (VII.) have reference to this subject, and indicate the importance of some action being taken by the Committee to prepare for a thorough application of the Gaussian analysis to the magnetic variations. It will be seen from the remarks of Dr. Schuster that some time must elapse before observations are obtained sufficiently good and complete to justify a systematic application to them of mathematical analysis. This circumstance has induced the Secretary to lay before this Committee in Appendix VIII. a provisional working hypothesis regarding the cause of the periodic variations of terrestrial magnetism which has gradually grown up by contributions from various quarters.

While this Committee do not hold themselves responsible for the various statements contained in this hypothesis, they would point out the desirability of ascertaining to what extent well-known magneto-electric laws may succeed in accounting for the phenomena of terrestrial magnetism, and likewise the desirability of ascertaining to what extent the magnetic earth appears to be subject to the laws of ordinary magnets.

A preliminary working hypothesis of this nature might serve to elicit facts while the material for the Gaussian analysis is being completed, and it would add to the interest of the final result if we should obtain reason to think that electric currents in the upper atmospheric regions are at once the *immediate causes* of magnetic variations and the *effects* of atmospheric motions in these regions, so that a knowledge of the one set of currents might possibly enable us to determine the other.

Finally, in Appendix IX. we have a list drawn up by Sir J. Henry Lefroy of the various stations where magnetic observations of any importance have been made.

The Committee have drawn 10*l.* 10*s.*, and returned to the Association a balance of 29*l.* 10*s.* They would desire their re-appointment, and would request that the sum of 50*l.* should be placed at their disposal, to be spent as they may think best on the researches mentioned in this report.

Third Report of the Committee, consisting of Prof. Balfour Stewart (Secretary), Prof. Stokes, Prof. Schuster, Mr. G. Johnstone Stoney, Prof. Sir H. E. Roscoe, Capt. Abney, and Mr. G. J. Symons, appointed for the Purpose of Considering the Best Methods of Recording the Direct Intensity of Solar Radiation.—The Committee, in conformity with their last report, have had constructed by Mr. Casella an instrument of the following description:—It consists of a thick-sided copper cube, one side of which is to be exposed to the sun. In the thickness of this side are inserted two thermometers; a third is put in the side opposite; while the bulb of a fourth occupies the hollow centre. If the readings of these instruments are found to have any constant relation, the fourth instrument will be replaced by a flat bulb thermometer exposed to the sun's rays through a hole in the sun-ward side of the cube. The Committee suggest that they be re-appointed, and that the sum of 20*l.* be again placed at their disposal.

From the Report of a Committee, consisting of Profs. G. H. Darwin and J. C. Adams, for the Harmonic Analysis of Tidal Observations. Drawn up by G. H. Darwin.—Major Baird's "Manual of Tidal Observations" is now printed, and will be

[1] See Appendix by Sir G. B. Airy to the Greenwich Observations, 1884.

[1] An account of these researches will be found in the *Phil. Mag.*, April and May 1886.

sold by the British Association, 22, Albemarle Street, W. The Indian tidal results of all previous years, and those given in the various reports to the British Association, have been reduced by Major Baird to the standard form recommended in the report of 1883. To these have been added the results derived by the United States Coast Survey, and the whole has been published in the *Proceedings* of the Royal Society, No. 239, 1885, in a paper by Major Baird and Prof. Darwin. In the course of the Indian tidal operations a discussion has arisen as to the determination of a datum-level for tide-tables. The custom of the Admiralty is to refer the tides to "the mean low-water mark of ordinary spring tides." This datum has not a precise scientific meaning, as the diurnal tides enter into the determination of the datum in an undefined manner; and it follows that two determinations, both equally defensible, might differ sensibly from one another. A datum-level should be sufficiently low to obviate the frequent occurrence of negative entries in a tide-table, and it should be rigorously determinable from tidal theory. It is now proposed to adopt as the datum-level at any new ports in India, for which tide-tables are to be issued, a datum to be called "the Indian spring low-water mark," and which is to be below mean sea-level by the sum of the mean semi-ranges of the tides M_2, S_2, K_1, O ; or, in the notation used below,—

$$H_m + H_s + H' + H_o$$

below mean water-mark. This datum is found to agree pretty nearly with the Admiralty datum, but is usually a few inches lower. The definition is not founded on any precise theoretical considerations, but it satisfies the conditions of a good datum, and is precisely referable to tidal theory. If, when further observations are made, it is found that the values of the several H's require correction, it is not proposed that the datum-level shall be altered accordingly, but, when once fixed, it is to be always adhered to. The report then shows how harmonic analysis might be applied to the reduction of a short series of tidal observations, such as might be made when a ship lies for a fortnight or a month in a port. The method has been applied by Mr. Alnutt to the computation of tide-tables at Port Blair and several other ports, and the computed results are compared with those given both by a rigorous instrument and by actual observation. It is remarked that, while better agreement was to be desired, the errors are inconsiderable fractions of the whole intervals of time and heights under consideration. An attempt made to detect the nineteen-yearly tide by observations at Karachi has led to the belief that it is extremely improbable that this important datum will ever be detected.

P. T. Main presented a *Report on our Experimental Knowledge of Certain Properties of Matter*.—The report discussed recent work on the testing of Boyle's law for very low as well as for very high pressures, the researches of Amagat and C. Bohr being included. It then passed to the verification of Gay-Lussac's law. Recent researches on the saturated pressures of vapours at various temperatures, especially those of Ramsay and Young, were next considered, especially to determine (1) whether statical and dynamical methods of observation lead to similar results ; (2) whether the pressures of ice and of water-vapour are the same at the same temperature. The important question of the pressure of mercury-vapour, and modern research in the determination of the critical points of nitrogen and other gases, and in the measurement of vapour-densities, were also included.

Prof. George Forbes presented the *Report of the Committee on Standards of Light*.—The Committee had met repeatedly during last winter. It had been proposed in last year's report to carry on experiments on electrical standards in the hope of arriving at an absolute standard of light. One of the first steps was to discover a means of reproducing a definite temperature, and certain experiments were proposed for this purpose. At one of the first meetings of the Committee Capt. Abney announced that he had already found a means of doing this in a different manner to that proposed in the Committee's report, and depending only upon the change of resistance of the carbon filament. Under these circumstances the Committee left this part of the experimental investigation to be reported upon by Capt. Abney. His further researches had, however, led him to believe that the law which he had announced to the Committee did not hold with all qualities of carbon filament. He had, however, been engaged upon further experimental researches, which were almost ready for publication, and which had an important bearing upon the labours of the Committee. In last year's report attention was drawn to the value of the pentane standard of Mr. Vernon Harcourt as a practical reproducible standard, and Mr. Rawson had been since then engaged in a further examination of that standard. Sir James Douglass had also made some experiments which were not quite completed, but had gone so far as to give great promise. Some account of the experiments in that report had been expected by the Committee, but the absence of Sir James Douglass on official business had interfered with this. At one of the first meetings of the Committee the Secretary showed what he had done in the way of improving thermopiles such as it was hoped would be of use in the investigations recommended in last year's report, and he was instructed by the Committee to proceed with the construction of the instrument, which had been completed, and was now to be placed before the Section and described in a separate paper. The Committee requested to be re-appointed, with a grant of 25*l*.

Report of the Committee on Electrolysis, presented by Prof. Oliver J. Lodge (Secretary).—The report, which was only an interim report, stated that only one meeting had been held, but a large amount of correspondence had passed, as well as work done by the various members. This work was discussed in separate papers. The Committee asked to be re-appointed with a grant to defray the expense chiefly of printing selected memoirs, and of getting pure substances.

Report of the Committee on English Channel Tides.—The Committee had received the records of the self-recording tide-gauges at Dover and Ostend for the four years 1880 to 1883. These are so bulky that they content themselves with discussing in an appendix to the report the records of four periods of a fortnight in the year 1883, namely, at the solstices and the equinoxes. Of these diagrams were shown. The Committee suggest that they hand over their papers and records to the Committee for the Harmonic Analysis of Tides.

Prof. Johnson submitted the *Report of the Committee formed in Canada to establish a System of Tidal Observations in that Country*.—He said they had communicated with the Government in the matter, and while, owing to the expense at present incurred in hydrographical work on Lake Ontario and elsewhere, the Government had not yet given their consent, it was hoped that before long their object would be attained. The Committee asked for re-appointment.

Report of the Electrical Standards Committee, presented by Mr. Glazebrook (Secretary).—Eighteen standard coils have been tested during the year, and certificates of their value issued. The attention of the Secretary was called to the fact that the paraffin in some of the coils showed a trace of green coloration round the edges. This has been shown to arise from the action of a small amount of acid, left in the paraffin, on the copper of the case and connecting-rods, and the Committee are considering how to deal with the difficulty. At present the insulation resistance of the coils is extremely high, amounting to as much as 8000 megohms. The Committee wish to express their sense of the great desirability of establishing a National Standardising Laboratory for Electrical Instruments on a permanent basis, and their readiness to co-operate in the endeavour to secure the same. The Committee apply for re-appointment, with the addition of the name of Mr. J. T. Bottomley.

Second Report on the Fossil Plants of the Tertiary and Secondary Beds of the United Kingdom, by J. S. Gardner.—Attention has been devoted exclusively this year to the fossil flowering or phanerogamous plants. The results point to the conclusion that while the Gymnosperms, to which the Coniferæ belong, are of the highest antiquity, there are no angiospermous plants in British rocks older than the Secondary, if we except the problematic plant known as *Spirangium*. Even so late as the Lias no indisputable Angiosperm has been discovered within our area, for the supposed Monocotyledons from the Rhætics, near Bristol, hitherto referred to the family of Pond-weeds under the name *Najadita*, are really cryptogamic plants of the moss tribe, closely allied to the river moss *Fontinalis*. This group had not previously been found fossil, and, so far as it goes, would indicate rather a temperate climate. It is important to notice that these conclusions are shared by Prof. Williamson, Mr. Carruthers, and by all botanists who have examined them, as well as by Mr. Brodie, the possessor of the specimens. The *Lilia*, *Bensonia*, and other supposed Monocotyledons of similar age are very imperfectly preserved, and doubtless referable to Cycads, a family which abounded then.

We have examined a large number of specimens of the anomalous Jurassic plant described by Carruthers as *Williamsonia*. Though there are still many difficulties in the way, our own examination of the specimens in London, Manchester, Cambridge, and elsewhere tends to confirm Saporta's view, referring them to the group of *Pandanaceæ*, so far as that there does appear to be vestiges, in some cases at least, of lignitic structure which may represent the areolæ or carpels. These rather minute cavities and the lignitic matter surrounding them fall away on exposure to the air, and only traces of them are visible. Should Saporta's contention be upheld, *Williamsonia* will be by far the most perfectly known of the Secondary Angiosperms, since all the organs of fructification and even of foliation are more or less known.

A still more definite Monocotyledon is the *Podocarya*, from the Inferior Oolite, originally figured by Buckland, and redescribed by Carruthers. Its resemblance to the fruit of *Williamsonia*, as interpreted by Saporta, is extremely striking, and on suggesting this to that author, he replied that he was in the act of preparing an important work on the very subject. The same work is to include an illustration of the most recent member of the group, obtained from the Grey Chalk of Dover, and which we thought advisable to communicate to him.

Next in point of age, among English Monocotyledons, to the *Podocarya* is the *Kaidacarpum*, from the Great Oolite, also described by Carruthers, and by him referred to the *Pandaneæ*. We have been able to ascertain that a second species, hitherto supposed to be of Cretaceous age from the Potton Sands, is a derived fossil, and undoubtedly Jurassic. A third species was originally figured, without any reference in the letter-press as to its age or locality, by Lindley and Hutton as *Strobilites Bucklandi*; this, however, is far more likely to prove a Jurassic than a Cretaceous fossil if found, and the genus should not be included in lists of plants of the latter age.

The oldest Monocotyledons thus appear to be referable to the *Pandaneæ*, a group of plants distributed in widely distant and remote oceanic islands, and whose fruits are still met with at sea in drifts of vegetable matter.

Next to these in antiquity are two very monocotyledonous-looking fragments from the Jurassic of Yorkshire, which have been fully described in the *Geological Magazine* for May and August. The one is apparently an unopened palm-like spathe, and the other a jointed cane-like stem. Mr. Brodie possesses an undoubtedly monocotyledonous leaf-fragment from the Purbeck of Swindon.

The *Aroideæ* have long been supposed to be a group of very high antiquity, but there are good reasons for believing that the supposed remains of aroideous plants from beneath the Tertiaries are, without exception, referable to other groups, and actually there are no known traces of them earlier than the Middle Eocene, when they become by no means uncommon.

In a similar manner the fruits once supposed to represent *palms* in the Palæozoic and Mesozoic rocks have been gradually removed or suppressed, and, unless the fragments of palm-like wood in the Gault at Folkestone are taken into account, there are no traces of palms in any of our Secondary strata. They, however, appear as low down in our Eocene as the Woolwich series.

The supposed liliaceous or *Dracæna*-like stems from the Wealden, so frequently mentioned by Mantell, are not easy now to identify; but it is very probable that certain stems of *Endogenites* in the British Museum are those intended, in which case they are of course cycadeous. The Wealden has, indeed, so far yielded no trace whatever of any more highly organised plants than ferns and Gymnosperms, and this, when we consider that Monocotyledons were undoubtedly in existence, is a fact that should be of great significance to speculative geologists. The sediments must represent the deposits of the drainage system of a large area, for they are of vast extent and thickness, varied in character, and abounding in remains of trunks and stems, fruits and foliage of plants. In them, therefore, if anywhere, we might reasonably expect to find at least the traces of reed and rush, but the swamps seem to have been tenanted only by Equisetum and ferns, and the forests by Cycads and Conifers.

Angiosperms are absent throughout the Neocomian and Gault of Britain; and it is only in the White Chalk that we meet with any indications of them.

Of the gymnospermous section of Phanerogams the records are very different. To refer here to the earlier Secondary Coniferæ and Cycadeæ would be quite beyond our province, and it is only those of the Cretaceous, as the last discoverable ancestors in our area of the Eocene flora, that are of immediate interest. These belong, excluding Cycads, chiefly to the newest section of the Coniferæ, the Pine family. We are able to make the following contribution to our knowledge of these:—*Pinites Andraei*, Coemans (Gault, Folkestone); *P. Valdensis*, sp.nov. (Wealden, Brook Point, Isle of Wight); *P. Carruthersi*, sp.nov. (Wealden, Brook Point, Isle of Wight); *P. cylindroides*, sp.nov. (Lower Greensand, Potton); *P. Pottoniensis*, sp.nov. (Lower Greensand, Potton). These are described and figured; the report then gives a list of thirteen species of British Cretaceous Coniferæ previously described.

Passing to the Tertiary forms the report refers to leaves in the basement bed of the London Clay at Colden Common, between Bishopstoke and Winchester: the blocks of clay in which the leaves occur are derived, but the plants are allied to the Alum Bay flora; there are no palms.

Much work has been done in collecting at Sheppey, but there are great difficulties in the way of determining the fruits.

A large series of leaves of *Smilaceæ* has been obtained from Bournemouth, by means of which the number of good species is now reduced to five.

The leaves of *Smilaceæ* are highly characteristic, and can be determined with a large degree of certainty; but it is quite improbable that such will be the case with very many of the families of Dicotyledons.

Fortunately fruits and even flowers are comparatively abundant at Bournemouth, and we consequently anticipate little difficulty in determining leaves belonging to such easily distinguishable fruits as *Alnus*, *Tilia*, *Acer*, *Carpinus*, the *Leguminosæ*, and many others, but the residuum with indeterminable fruits, or fruits that will not float, may be very large. We are thus brought to the question, whether any value beyond that of mere landmarks, or aids to the correlation of rocks, can be attached to the determinations of fossil dicotyledonous leaves arrived at when fruits are absent. Nearly every Tertiary and even many Cretaceous floras are said to comprise *Quercus*, *Fagus*, and *Corylus*, to select these as typical examples. Now, we very much doubt whether the fruits of these genera have been met with in any strata older than the Upper Miocene, we might almost say the Pliocene; whilst in the latter the fruits of at least two of them are very far from uncommon. Fossil hazel-nuts are well known to abound in forest beds such as the one at Brook, in the Isle of Wight, and at Carrickfergus. It does appear to us that it would have been wiser and more consistent, when arriving at these determinations, to have taken the absence of fruits into account, when these were such as would naturally have been preserved. The large proportion of fossil dicotyledonous leaves that have been referred without any hesitation to living genera, must strike every one, in comparison with the relatively few associated fruits that have been determined otherwise than as Carpolithes—a name which is a confession of failure. It will thus be seen that in our opinion the fossil Dicotyledons of our own Eocene must be dealt with in a manner different from that pursued by the majority of foreign writers on kindred subjects, and that a revision of much of their work is urgently needed.

Report on the Caves of North Wales, by Dr. H. Hicks, F.R.S.—The explorations have been confined to the caverns of Ffynnon Beuno and Cae Gwyn, in the Vale of Clwyd. Among the remains discovered in these two caverns up to the commencement of the work this year there were over eighty jaws belonging to various animals, and more than 1300 loose teeth, including about 400 rhinoceros, 15 mammoth, 180 hyæna, and 500 horse teeth. Other bones and fragments of bones occurred also in very great abundance. Several flint implements, including flakes, scrapers, and lance-heads, were found in association with the bones. The most important evidence, however, obtained in the previous researches was that bearing on the physical changes to which the area must have been subjected since the caverns were occupied by the animals. During the excavations it became clear that the bones had been greatly disturbed by water action, that the stalagmite floor, in parts more than a foot in thickness, and massive stalactites had also been broken and thrown about in all positions, and that these had been covered afterwards by clays and sand containing foreign pebbles. This seemed to prove that the caverns, now 400 feet above Ordnance datum, must have been submerged subsequently to their occupation by the animals and by man. One of the principal objects, there-

fore, which the Committee had in view this year was to critically examine those portions of the caverns not previously explored, so as to endeavour to arrive at the true cause of the peculiar conditions observed. When the explorations were suspended last year in the Cae Gwyn Cave it was supposed that we had just reached a chamber of considerable size, but after a few days' work this year it was found that what appeared to be a chamber was a gradual widening of the cavern towards a covered entrance. The position of this entrance greatly surprised us, as hitherto we had believed that we were gradually getting further into the limestone hill. The rise in the field at this point, however, proved to be composed of a considerable thickness of glacial deposits heaped up against a limestone cliff. A shaft, 20 feet deep, was opened over this entrance from the field above. The beds were carefully measured by Mr. C. E. De Rance, Mr. Luxmoore, and the writer, during the prosecution of the work. Below the soil, for about 8 feet, a tolerably stiff boulder-clay, containing many ice-scratched boulders and narrow bands and pockets of sand, was found. Below this there were about 7 feet of gravel and sand, with here and there bands of red clay, having also many ice-scratched boulders. The next deposit met with was a laminated brown clay, and under this was found the bone-earth, a brown, sandy clay with small pebbles and with angular fragments of limestone, stalagmite, and stalactites. On June 28, in the presence of Mr. G. H. Morton, of Liverpool, and the writer, a small but well-worked flint-flake was dug up from the bone-earth on the south side of the entrance. Its position was about 18 inches below the lowest bed of sand. Several teeth of hyæna and reindeer, as well as fragments of bone, were also found at the same place; and at other points in the shaft teeth of rhinoceros and a fragment of a mammoth's tooth. One rhinoceros tooth was found at the extreme point examined, about 6 feet beyond and directly in front of the entrance. It seems clear that the contents of the cavern must have been washed out by marine action during the great submergence in mid-Glacial time, and that they were afterwards covered by marine sands and by an upper boulder-clay, identical in character with that found at many points in the Vale of Clwyd and in other places on the North Wales coast. The bone-earth seems to diminish in thickness rather rapidly outwards under the glacial deposits, but it was found as far out as the excavations have been made. Here the bone-earth rests directly on the limestone floor, with no local gravel between, as in the cavern. It would be interesting to know how far the cave-earth extends under the glacial deposits, but this could only be ascertained by making a deep cutting through the terrace of glacial deposits, which extends for a considerable distance in a westerly direction. The glacial deposits here are undoubtedly in an entirely undisturbed condition, and are full of smooth and well-scratched boulders, many of them being of considerable size. Among the boulders found are granites, gneiss, quartzites, flint, felsites, diorites, volcanic ash, Silurian rocks, and limestone. Silurian rocks are most abundant. It is clear that we have here rocks from northern sources, along with those from the Welsh hills, and the manner in which the limestone at the entrance to the cavern in the shaft is smoothed from the north would indicate that to be the main direction of the flow. The marine sands and gravels which rest immediately on the bone-earth are probably of the age of the Moel Tryfaen and other high-level sands, and the overlying clay with large boulders and intercalated sands may be considered of the age of the so-called upper boulder-clay of the area. The latter must evidently have been deposited by coast-ice. Whether the caverns were occupied in pre- or only in inter-Glacial times it is difficult to decide, but it is certain that they were frequented by Pleistocene animals and by man before the characteristic glacial deposits of this area were accumulated. The local gravel found in the caverns, underlying the bone earth, must have been washed in by streams at an earlier period, probably before the excavation of the rocky floor of the valley to its present depth. From the Glacial period up to the present time excavation has taken place only in the glacial deposits, which must have filled the valley up to a level considerably above the entrances to the caverns. The characteristic red boulder-clay with erratic blocks from northern sources is found in this area to a height of about 500 feet, and sands and gravels in the mountains to the south-east to an elevation of about 1400 feet. The natural conclusion therefore is that the caverns were occupied by an early Pleistocene fauna and by man anterior to the great submergence indicated by the high-level marine sands, and therefore also before the deposition of the so-called great upper boulder-clay of this area. As there is no evidence against such a view, it may even be legitimately assumed that the ossiferous remains and the flint implements are of an earlier date than any glacial deposits found in this area.

Fourth Report on the Fossil Phyllopoda of the Palæozoic Rocks, by Prof. T. R. Jones.—This report tabulates 37 species with their geological range, critical remarks being given on most species. There are 28 species of *Ceratiocaris* from beds ranging from the Carboniferous limestone to the Lower Wenlock; three doubtful forms are recorded from the Upper Llandovery and Tremadoc. The other forms referred to are *Emmelezoaris* (four species), *Physocaris vesica*, and *Xiphocaris ensis*, all from the Ludlow beds.

Report on the Volcanic Phenomena of Vesuvius and its Neighbourhood, by Dr. H. J. Johnston-Lavis.—The report gave a description of the volcanic activity of Vesuvius during the past year, illustrated by photographs. The fourth sheet of the Geological Map of Monte Somma and Vesuvius (scale 1 : 10,000) has been completed, and was exhibited at the meeting; this distinguishes in great detail the lava flows of various dates. The present year has been remarkable for the chances it affords for studying the subterranean structure of the Campi Phlegrei and the volcanic region around Naples. The great main drain which is to convey the sewage of Naples to the Gulf of Gaeta will traverse the region west of Naples on a line running nearly east and west. Five borings have been made to test the ground to be cut through, in which observations on the water-level, temperature, and presence of volcanic gases were made. A deep well is in progress at Lago Fusaro. Five other borings on or near the renowned Starza or fore-shore of Puzzuoli, on the new works of Sir W. Armstrong, Mitchell, and Co., are interesting as being within a few hundred yards of the celebrated so-called Temple of Serapis. Two are on the shore, and three at varying distances out to sea; they fully confirm the opinions generally held as to the oscillations of the ground here. The new Cumana Railway from Naples to Baia and Fusaro traverses the rocky escarpment just west of Naples. This has hitherto been supposed to be composed of a moderately uniform mass of pelagonatised basic marine tuff; but under the middle of the Corso Vitt. Emanuele and the Via Tasso the edge of a trachyte flow was traversed for over 70 metres. Much interesting information is expected from this railway, which will require a number of cuttings and tunnels, and will have to traverse the hot hill behind Baia. A deep well, in progress at Ponticelli on the outskirts of Naples, towards Vesuvius, has already been carried to a depth of over 100 metres; in the lower half of this a series of leucitic lava-streams was traversed, showing the great distances to which the old flows from Monte Somma reached, and also that either great depression of land has taken place, or that Monte Somma once formed a volcanic island. The work in hand, in addition to watching the progress of the works mentioned above, and mapping the old lava-streams, includes a careful study of the ejected blocks of Monte Somma, both chemical and microscopical, and a comparison of these rocks with those of the ancient volcanic regions of the Fassathal in the Tyrol, which they greatly resemble.

Thirteenth Report on the Erratic Blocks of England and Wales, by Rev. Dr. Crosskey.—This describes boulders near Settle and Kendal, to which the attention of the Committee has been called by Prof. T. McKenny Hughes, which are perched on pedestals of limestone striated in the direction of the main ice-flow. The boulders have preserved the rock immediately beneath from denudation. Mr. Plant gives much information upon the boulders in the valley of the Soar near Leicester recently well exposed in deep excavations. Thousands of blocks here occur in the boulder-clay; about one half are from Charnwood Forest, the remainder from the Permian sandstones and Carboniferous rocks of the Ashby coal-field, with blocks of mountain limestone brought fifteen or eighteen miles from the north-west; the rest are from the east side of the Pennine Chain, forty to fifty miles distant to the north-east. Details of various other excavations in and around Leicester were given, from which it is inferred that the Charnwood district was the centre of local ice-action. Dr. Crosskey and Mr. F. W. Martin describe a group of boulders between Shifnal and Tong, the stones consisting of rocks from the Lake district with Criffel granites which have evidently travelled together to their present position.

Report on the Erosion of the Sea-Coasts of England and Wales, by C. E. De Rance and W. Topley.—The information here given referred in part to the East Anglian coast, for which

several returns had been received. The rate and mode of erosion of the chalk cliffs at the north-west part of the Isle of Thanet is described by Mr. R. B. Grantham; in places as much as 20 feet in width of cliff has been lost in five years, but the average loss is not so much.

Capt. T. Griffiths and Mr. H. W. Williams contribute a report on the north-west coast of Pembrokeshire, where the alterations are in places important, and in all are historically interesting.

A report by Mr. K. McAlpine, on Pembrokeshire, illustrated by numerous photographs, was also laid before the meeting.

The Twelfth Report of the Committee on the Circulation of Underground Waters, by C. E. De Rance.—During the thirteen years the investigation had been going on much valuable information had been obtained; the complete dependence of the supply of underground water on the annual rainfall and the character and porosity of the strata on which the rain fell, had been completely established, varying from one inch to twelve inches of rainfall annually absorbed on each square mile; one inch of rain giving 40,000 gallons per day for each square mile of surface exposed. The great value of underground supplies had been shown during severe droughts, the dry-weather flow of the streams and rivers being wholly dependent on underground supplies issuing as deep-seated springs. Large quantities of water could be obtained by deep wells in suitable situations, as was well shown by the Birmingham Corporation supply,—the Aston well yielding 3 million gallons a day, the Witton well 2½ million gallons, King's Vale a quarter of a million gallons, Perry well 2 million gallons, Selby Oak well 1¼ million gallons, giving a total supply from wells of 9 million gallons a day, the remaining supply being from streams yielding 7½ million gallons a day; giving a total supply of 16½ million gallons, of which only 12 are required at present. Large supplies of pure artesian well waters are obtained and used at Nottingham, Liverpool, and Birkenhead. The supplies to other cities were investigated, and the recent successful borings at Stafford commented on.

Report of the Committee, consisting of Mr. John Cordeaux (Secretary), Prof. A. Newton, Mr. T. A. Harvie-Brown, Mr. William Eagle Clarke, Mr. R. M. Barrington, and Mr. A. G. More, re-appointed at Aberdeen for the Purpose of Obtaining (with the consent of the Master and Brethren of the Trinity House and the Commissioners of Northern and Irish Lights) Observations on the Migration of Birds at Lighthouses and Light-vessels, and of reporting on the same.—The General Report of the Committee, of which this is an abstract, is comprised in a pamphlet of 173 pages,[1] and includes observations taken at lighthouses and light-vessels, as well as at several land stations, on the coasts of Great Britain and Ireland and the outlying islands. The best thanks of the Committee are due to their numerous observers for their assistance. Much good work has been rendered by those amongst them who have taken the trouble to forward a leg and wing of such specimens as have been killed against the lanterns, and which they have themselves not been able to identify. This has already led to the determination of several rare birds, which otherwise would have escaped notice. It is evident that, unless the birds can be correctly named, the value of this inquiry is materially diminished, and ornithologists may justly refuse to accept the accuracy of the statements. It is intended, in order to facilitate the sending of wings, to supply the light-keepers with large linen-lined envelopes, ready stamped, and inclosing labels for dates and other particulars. The best thanks of the Committee are also tendered to Mr. H. Gätke for the increased interest he has given to their Report by forwarding a daily record of the migration of birds as observed at Heligoland between January 1 and December 31, with the concurrent meteorological conditions under which the various phenomena occurred. Altogether 187 stations were supplied with printed schedules for registering the observations, and returns have been sent in from 125. About 267 separate schedules have been sent in to your reporters. The general results, as far as the special object of the inquiry, have been very satisfactory, and much information has also been accumulated respecting the breeding habits of sea-fowl on the outlying islands and skerries on the Scotch and Irish coasts, and altogether a great mass of facts and valuable data obtained which cannot fail to be of value to future inquirers. A special point of interest in the Report is the large arrival, with a north-east wind, of pied flycatchers in the first week in May 1885, observed at Spurn Point, Flamborough Head, the Isle of May, and Pentland Skerries. At Flamborough Head the flycatchers were accompanied by male redstarts in large numbers, both species swarming for two or three days. The immigration at this period was not exclusively confined to these two species. Mr. Agnew, writing from the Isle of May, at the entrance of the Firth of Forth, says, under date of May 3:—"An extraordinary rush of migrants to-day; have never seen anything like it in spring. To attempt to give numbers is simply useless. I will just give you the names in succession: fieldfares, redwings, ring-ouzels, blackbirds, lapwings, dotterels, rock-pigeons, hawk, meadow pipits, redstarts, whinchats, tree-sparrows, yellow wagtails, ortolan (obtained), robins, chiff-chaffs, wood-warbler, blackcap warbler, marsh-tit, whitethroats, and pied flycatchers." And on the 4th: "Stil increasing in numbers, but wind shifted this morning to E. for S.E." A noteworthy incident also of the vernal migration was the great rush of wheatears observed at the Bahama Bank vessel off the Isle of Man, and at Langness on the night of April 13, when many perished and were captured. On the same night, wheatears were killed at the Coningbeg and Rathlin Island Lighthouses on the Irish coast. On the 12th and 13th the rush was very heavy at stations on the west coast of Scotland. No corresponding movement was observed on the east coast of Great Britain on the same night; but at Hanois L.H., Guernsey, on May 10, at night at the north light, and on the Lincolnshire coast and Farn Islands on the 10th and 11th. These entries are sufficient to show the immense area covered by the migration of this species at or about the same period. On the east coast of England the first wheatears were observed at the Farn Islands on February 22. The autumnal migration is first indicated at Heligoland on July 6, and was continued with slight intermissions up to the end of the year. A similar movement affected the whole of the east coast of Great Britain during the same period, but was apparently less constant and persistent than at Heligoland. It has been remarked in previous Reports that the migration of a species extends over many weeks, and in some cases is extended for months. Yet it is observable that, at least on the east coast of England, year by year, the bulk or main body of the birds come in two enormous and almost continuous rushes during the second and third weeks in October and the corresponding weeks in November. In the autumn of 1885 it is again observable that the chief general movements which usually characterise the southward autumnal passage were two in number, and affected the stations over the whole coast-line both east and west of Great Britain. The first of these commenced about October 11, and was continued to the 20th. The second from November 8 to 12. It is worthy of notice that these two chief movements of the autumn were ushered in by, and concurrent with, anti-cyclonic conditions, preceded by, and ceasing with, cyclonic depressions, affecting, more or less, the whole of the British Isles. From this it appears not unlikely that birds await the approach of favourable meteorological conditions, of which, perhaps, their more acute senses give them timely warning, to migrate in mass. Whatever may be the cause which impels these enormous rushes, often continuous for days, it is one which operates over an immense area at one and the same time. The October rush reached its maximum on the 16th, at which date almost all the stations report extraordinary numbers of various species on the wing. As one out of many, we quote from the journal of Mr. James Jack, principal of the Bell Rock Lighthouse:—"Birds began to arrive at 7.30 p.m., striking lightly and flying off again; numbers went on increasing till midnight, when it seemed that a vast flock had arrived, as they now swarmed in the rays of light, and, striking hard, fell dead on balcony or rebounded into the sea. At 3 a.m. another flock seemed to have arrived, as the numbers now increased in density; at the same time all kinds crowded on to the lantern windows, trying to force their way to the light. The noise they made shrieking and battering the windows baffles description. The birds were now apparently in thousands; nothing ever seen here like it by us keepers. Wherever there was a light visible in the building they tried to force their way to it. The bedroom windows being open as usual for air all night, they got in and put the lights out. All birds went off at 6 a.m., going W.S.W. Redwings were most in number, starlings next, blackbirds, fieldfares, and larks." The rush in November chiefly took place in the night; at the Bell Rock the movement ceased at midnight of the 12th, and at

[1] "Report on the Migration of Birds in the Spring and Autumn of 1885." (McFarlane and Erskine, 19, St. James's Square, Edinburgh.)

the Longstone Lighthouse, on the Farn Islands, a little earlier—at 10.30 p.m., when the wind became strong from S.W. From each succeeding year's statistics we have come to almost similar conclusions regarding the lines of flight—regular and periodically used routes where the migratory hosts are focused into solid streams. Three salient lines on the east coast of Scotland are invariably shown, viz. (1) by the entrance of the Firth of Forth, and as far north as Bell Rock, both coming in autumn and leaving in spring; (2) by the Pentland Firth and Pentland Skerries, likewise in spring and autumn; and (3) by the insular groups of Orkney and Shetland, which perhaps may be looked upon as part of No. 2. On the other hand, three great areas of coast-line, including many favourably lighted stations, almost invariably, save in occasionally protracted easterly winds, and even then but rarely, send in no returns, or schedules of the very scantiest description. These areas are Berwickshire, the whole of the east coast south of the Moray Firth, and Caithness and East Sutherland. Each and all of these areas possess high and precipitous coast-lines, if we except the minor estuaries of the Rivers Tay and Dee, and a small portion of the lower coast-line of Sutherland, which face towards the east. On the east coast of England these highways are less clearly demonstrated. The Farn Islands, Flamborough Head, and the Spurn are well established points of arrival and departure; but south of the Humber as far as the South Foreland the stream appears continuous along the whole coast-line, and to no single locality can any certain and definite route be assigned. It cannot be said that the southerly flow of autumn migrants is equally distributed along the entire west coast of England. On the contrary, the schedules afford unmistakable evidence that the great majority of these migrants, so far as the English and Welsh coasts are concerned, are observed at stations south of Anglesey. But while the north-west section of the coast is thus less favoured than the rest, such is not the case with the Isle of Man, which comes in for an important share of the west coast migratory movement. The fact has already been alluded to that large masses of immigrants from Southern Europe pass through the Pentland Firth, and, along with migrants from Faroe, Iceland, and Greenland, pass down the west coast of Scotland, whence many cross to Ireland, and it seems most probable that the remainder leave Scotland at some point on the Wigtown coast, and pass by way of the Isle of Man to the west coast of Wales, and thus avoid the English shore of the Irish Sea. The schedules sent in from the coasts of Flint, Cheshire, Lancashire, and Cumberland show that in 1884–85 comparatively few migrants were observed, and that the great general movement did not affect them in any general degree. These remarks do not apply to migrants amongst the waders and ducks and geese, which, as a rule, closely follow coast-lines, and which are abundantly represented on the Solway and coasts of Cumberland and Lancashire. There is a much-used bird route along the north coast of the British Channel, and thence, from the Pembroke coast, across to Wexford, passing the Tuskar Rock, the best Irish station. The fact of a double migration or passage of birds, identical in species, across the North Sea in the spring and autumn both towards the E. and S.E., and to the W. and N.W., is again very clearly shown in the present report. This phenomenon of a cross migration to and from the Continent, proceeding at one and the same time, is regularly recorded on the whole of the east coast of England, but is specially observable at those light-vessels which are stationed in the south-east district; at the same time, it is invariably persistent, and regular year by year. Our most interesting stations are those on small islands or rocks, or light-vessels at a considerable distance from shore, and the regular occurrence of so many land birds, apparently of weak power of flight, around these lanterns, is a matter of surprise to those unacquainted with the facts of migration. No clear indication of the migration of the redbreast has yet been shown on the Irish coast; the records of its occurrences are few and scattered. The black redstart was recorded at several stations in the southern half of Ireland; specimens were forwarded from Mine Head, the Skelligs, and Rockabill. It is apparently a regular winter visitant to the Skelligs and Tearaght, generally appearing in October and November. The occurrences so far recorded by the Committee of the black redstart on the east coast of Great Britain, in the autumn, range between October 23 and November 3. In the spring of the present year, Mr. G. Hunt, under date of March 20, reports an extraordinary flight of rooks at Somerton, on the Norfolk coast, which he observed from 10.30 a.m. to 6 p.m. He says:—"I observed them flying just above the sand-hills, going due south, and as far as the eye could see both before and behind there was nothing but rooks. There could never for one moment in the day be less than a thousand in sight at one time; they kept in a thin wavering line. The coast line here runs due north and south." Mr. J. H. Gurney reports:—"I saw the rooks and grey crows on the same day in much smaller numbers as were seen at Somerton, which is fifteen miles further south. I again saw them on the 21st, 22nd, 25th, 26th, and 29th, but none after this date; with us, however, grey crows preponderated; the direction was to S.E. An enormous migration of these and many others is recorded from Heligoland, also from Hanover between March 19 and 25." In conclusion your Committee wish to thank H.R.H. the Master and the Elder Brethren of the Trinity House, the Commissioners of Northern Lights, and the Commissioners of Irish Lights for their ready co-operation and assistance, through their intelligent officers and men, in this inquiry. The Committee respectfully request their re-appointment.

Report of the Committee, consisting of Prof. Cleland, Prof. McKendrick, Prof. Ewart, Prof. Stirling, Prof. Bower, Dr. Cleghorn, and Prof. McIntosh (Secretary), for the Purpose of continuing the Researches on Food-Fishes and Invertebrates at the St. Andrews Marine Laboratory.—The Committee beg to report that the sum of 75*l.*, placed at their disposal, has for the most part been expended in the purchase of instruments and books permanently useful in the Laboratory, only a limited proportion having been disbursed for skilled assistance. Since the meeting of the Association at Aberdeen last year several structural improvements in the wooden hospital, now converted into the Laboratory, have been completed, and others are being carried out by the Fishery Board for Scotland. These changes will render the temporary building much more suitable for work. A small yawl of about 21 feet in length has also been added to the apparatus by the Fishery Board. The desiderata now are an increase in the number of good microscopes and other expensive instruments, and an addition to the nucleus of books which workers require always at hand. In this respect the Laboratory has been much indebted to the Earl of Dalhousie, who forwarded a complete set of Fishery Blue-Books, and to the Trustees of the British Museum, who sent such of their publications as bore on marine zoology. Collections of papers have also been forwarded by many observers, amongst whom Prof. Flower, the late Dr. Gwyn Jeffreys, and Prof. Alexander Agassiz are conspicuous. Most of the Continental and American workers in marine zoology and cognate subjects, as well as those of our own country, are indeed represented. The first work of the year was the examination of a fine male tunny, 9 feet in length, caught in a beam-trawl net near the mouth of the Forth, and the skeleton of which is now being prepared for the University Museum. Various interesting anatomical features came under notice, and its perfect condition enabled a more correct figure of its external appearance to be made (*vide Ann. Nat. Hist.*, April and May 1886, and "Fourth Report of the Fishery Board for Scotland," plate 8). The examination of various food- and other fishes in their adult and young conditions was systematically carried out, and notes on the following species will be found in the *Annals of Natural History*, and the "Report of the Fishery Board":—Weever (greater and lesser), shanny, sand-eel, halibut, salmon, common trout, herring, sprat, conger, ballan-wrasse, shagreen-ray, piked dog-fish, and porbeagle-shark. Special attention was also given to the "Mode of Capture of Food-Fishes by Liners," "Injuries to Baited Hooks and to Fishes on the Lines," "Shrimp-Trawling in the Thames," "Sprat-Fishing," and to the "Eggs and Young of Food- and other Fishes," "Diseases of Fishes," the "Effect of Storms on the Marine Fauna," and "Remarks on Invertebrates, including Forms used as Bait" (*vide* "Fourth Report of the Fishery Board for Scotland," 1886). The active work in connection with the development of fishes for the season may be dated from the middle of January, when one of the local trawlers captured a large mass of the ova of one of the food-fishes, viz. the catfish. The embryos in these eggs (which are the size of the salmon's) were well advanced, so that, with the exception of a few unimpregnated ova observed during the trawling experiments of 1884, the earlier stages have yet to be examined. The large size of the embryos of the catfish permitted a satisfactory comparison to be instituted between them

and the salmon, which had formerly been under examination, and the results, with drawings of both forms, are nearly completed, and will be communicated to one of the Societies during the winter. The first pelagic ova, viz. those of the haddock, made their appearance during the very cold weather in the beginning of February, and the examination of these, together with those of the cod and common flounder—both of which were unusually late—enabled Mr. E. E. Prince and the Secretary to extend considerably the observations of last year. Moreover, for the first time, the ova of the ling (*Molva vulgaris*) were examined, and the development followed to a fairly advanced stage. These were procured by a long-line fisherman of Cellardyke (who with others was supplied with suitable earthenware jars[1] and encouraged by a visit to the Laboratory), fertilised about 100 miles off the Island of May, and safely brought, after a considerable land journey, to St. Andrews. The fertilised ova of the plaice and lemon-dab were similarly brought by Capt. Burn, late of the Hussars, from the Moray Frith; for the Laboratory had then no boat suited for procuring a supply nearer home. No fish, however, has been more useful to the workers this season than the gurnard (*Trigla gurnardus*), the spawning period of which seems to have been somewhat later than usual. The first ova were procured about the middle of May, and the embryos of the last hatching (middle of August) still swarm in the vessels. Further observations were also made on the ova and young of the lumpsucker, Montagu's sucker, shanny, stickleback, sand-eel, *Cottus*, &c. Amongst others the ripe ovum of *Ammodytes tobianus* has been examined. It is colourless, translucent, and has a beautifully reticulated capsule. Mr. Prince is of opinion that, as suggested in the "Report of H.M. Trawling Commission," it most nearly resembles a pelagic egg. Moreover, the information necessary for filling up the gaps between the very early stages of the young food-fishes near the surface and their appearance off the shore as shoals of young forms more or less easily recognisable specifically has been considerably increased. Much of this knowledge has been obtained by the aid of a huge tow-net of coarse gauze—upwards of twenty feet in length—attached to a triangle of wood, ten feet each way, sunk by a heavy weight and kept steadily at the required depth in fathoms by a galvanised iron float, such as is used for the ends of herring-nets. Since the completion of the net, however, the services of the Fishery Board tender *Garland* have only once been available, and the yawl has been at our disposal only a few weeks. In these brief opportunities, however, the young of various fishes have been obtained at stages hitherto unknown, and some rare invertebrates and a new Medusa have been captured. Enough, in short, has been seen to indicate the value of this apparatus, and of certain modifications of the ordinary beam-trawl for work on the bottom. The hatching and rearing of the embryos of the common food-fishes have been attended with much greater success than last year or the previous one, and a large series of microscopic preparations (chiefly sections with the Caldwell and rocking microtomes) has been made by Mr. E. E. Prince, embracing the entire development of the food-fishes from the early ovum to a late larval stage. The study of these preparations is now being proceeded with; but in traversing a field so extensive as the embryology of these important Teleosteans a great expenditure of time and labour is required. It is hoped, however, that the results will be completed during the winter (*vide* for other observations the *Annals of Natural History* for April, May, June, and August 1886; NATURE, June 1886, &c.). Since the beginning of June, Dr. Scharff has been occupied with the investigation of the intra-ovarian egg of a number of Teleostean fishes. Among the ovaries examined were those of *Trigla gurnardus*, *Gadus virens* and *G. luscus*, *Gadus merlangus*, *Anarrhichas lupus*, *Conger vulgaris*, *Blennius pholis*, *Lophius piscatorius*, and *Salmo salar*. The researches were made on fresh ovaries and on spirit specimens. Most of those reserved for section-cutting were previously treated either with picro-sulphuric or weak chromic acid. Special attention was paid to the structural changes in the growing nucleus. The origin of the follicular layer surrounding the egg, as well as the origin and development of the yolk, will be dealt with in a paper to be published shortly. Considerable advancement has been made in the study of the development of the common mussel by Mr. John Wilson. Some of the very early larvæ are described in the report of last year, along with an account of the artificial methods employed. This year embryos were developed for forty days in vessels suitable for microscopic manipulation. Normal growth continued during the first fourteen days. At the end of this period the largest embryos had shell-valves ·128 mm. in length. They are transparent and almost semicircular, the dorsal (hinge-) line being nearly straight. The powerful velum could be wholly withdrawn within the valves. The alimentary system was conspicuously developed. In the beginning of June great numbers of young mussels were found swimming actively on the very surface of the sea close to the shore, and measuring ·134 mm. They differed from the most advanced of those artificially reared only in their being more robust, the stage reached being the same in both. At various periods somewhat later in the season many older, though still microscopic, mussels were captured with the tow-net in St. Andrews Bay from the shore seaward for 4 miles. Besides the careful study of their development, Mr. Wilson has also been engaged with the histology of the mussel (especially that of the generative organs) at various stages, up to the adult condition. The Committee beg to recommend a renewal of the grant (100*l.*) for the ensuing year.

[1] Containing about a gallon. These were partially filled with pure sea-water containing the fertilised ova, and simply tied over with porous cheese-cloth.

Report of the Committee, consisting of Prof. McKendrick, Prof. Struthers, Prof. Young, Prof. McIntosh, Prof. Alleyne Nicholson, Prof. Cossar Ewart, and Mr. John Murray (Secretary), appointed for the Purpose of Promoting the Establishment of a Marine Biological Station at Granton, Scotland.—The Committee report that the sum of 75*l.*, placed at their disposal, has been used to aid in defraying the expenses of carrying on the work of the Scottish Marine Station at Granton. Two reports on the work of the institution during the past year are given below; they have been sent in to the Secretary by Mr. J. T. Cunningham, the Superintendent, who has charge of the zoological investigations; and Dr. Hugh Robert Mill, who is responsible for the physical work:—

The biological work of the Station falls into three principal divisions: (1) Embryology and morphology; (2) faunology; (3) the accommodation of students and investigators. (1) Efforts to elucidate some facts bearing on the reproduction and development of Myxine formed the principal part of the work under this head during the autumn and winter. In the summer the aquarium had been arranged, and a large tank was specially devoted to the purpose of keeping specimens of the animal in confinement. After careful attention to the matter, it was found that the creatures refused entirely to feed while in captivity; they lived several months, but no signs of reproductive activity appeared, with one exception noted below. It was then determined to continue the examination of large numbers of specimens every month in the year in order to find if the ova were shed at any limited season. As almost nothing accurate was known on the whole subject, the first problem was to obtain ripe males and females. In November the testis in its immature condition was recognised, and it was subsequently found that with few exceptions all very immature specimens were hermaphrodite, containing immature testicular tissue at the posterior end of the generative organ. Microscopic examination of the largest ova obtained showed that the well-known polar threads belonged to the vitelline membrane, and were developed in tubular depressions of the follicular epithelium. In December, January, February, and March, females were obtained which had just discharged their ova, the collapsed capsules, still quite large, being present in the ovary. At the end of January two females were obtained in which the polar threads were so far developed as to form projections at the ends of the inclosing follicle. One specimen with eggs in this condition was taken from the aquarium. No perfectly ripe ova were ever obtained. In February moving spermatozoa were discovered in hermaphrodite specimens, but the total quantity of milt present was quite insignificant. The greater number of the specimens examined were obtained from fishermen's lines baited for haddock; some were taken by baited traps. In March dredging was carried on off St. Abb's Head, with a view to obtain deposited fertilised eggs of Myxine, but none were found. It has thus been shown that Myxine deposits its eggs in the months of December to March, and that the females are taken on the hook immediately after the eggs have been shed. But no method has been discovered of obtaining adults in the ripe condition, or of obtaining the fertilised ova and embryos. The research and its results are described in a paper in the *Proceedings* of the Royal Society of Edin-

burgh, and more fully in a paper which will appear in the next number of the *Quarterly Journal of Microscopical Science*. At the beginning of the present year the systematic examination of the ova of all species of fish which could be obtained was commenced. The pelagic ova of the cod, haddock, whiting, and gurnard had been examined in the previous spring, and those of a large number of additional species have now been figured and described at successive stages of development. The results of this work are now being published in full by the Royal Society of Edinburgh, and will appear as a memoir in the Society's *Transactions*. (2) The faunological investigations have been carried on as time permitted since the opening of the Station, and have, since June last, been receiving particular attention. A Report on the Chætopoda, in the preparation of which Mr. G. A. Ramage is giving his assistance, will appear in the coming autumn; a Report on the Sponges is being prepared by Mr. J. Arthur Thomson; and miscellaneous notes on other classes will be incorporated with these special Reports. (3) The following is a list of those who have carried on studies at the Station:—

	Name	Began	Left	Subjects
1885.	Dr. Kelso, Edinburgh ...	Aug.	Sept. 26	Teleostean ova
	And. D. Sloan, Edinburgh	Aug. 8	April 1886	Cœlenterates
	A. H. W. Macdonald, Edinburgh	Oct. 5	Nov. 1885	General
	G. L. Gulland, Edinburgh	Oct. 6	Nov. 1885	Crustacea
1886.	G. A Ramage, Edinburgh	June 3	—	Chætopoda, &c.
	M. M. Kay, Edinburgh ...	July 24	—	General
	Miss Macomish, London ...	Aug. 2	—	Mollusca
	J. Arthur Thomson, Edinburgh	Aug 9	—	Sponges, &c.

The yacht is kept up in the same condition as at the opening of the Station, and the number of men is unaltered. The ark at Millport is again in use this summer, and is in the charge of Mr. David Robertson. Mr. Cunningham worked there for one week in June, having found at Millport a particularly favourable opportunity for the study of Teleostean ova. Many other naturalists have taken part in the *Medusa's* dredgings in the Clyde district during the present summer. The services of Alex. Turbyne, the keeper of the Station, in making excursions in trawlers to procure fish ova, have been most valuable. All those interested in the Station are greatly indebted to Mr. Robert Irvine, of Royston, for the friendly assistance which he has always been ready to afford on every occasion. Preserved specimens of marine animals and plants are still sent out to applicants, and some attention is being paid to the question of oyster-cultivation in the Firth of Forth.

J. T. CUNNINGHAM, B.A., F.R.S.E.

Physical marine research has, from the commencement, formed one of the distinctive features of the Scottish Marine Station. During last year work has been carried on in this direction by Dr. H. R. Mill and Mr. J. T. Morrison; other gentlemen have occasionally made use of the facilities of the Station. Regular meteorological observations are continued twice daily, and include the temperature at surface and bottom of the water. An elaborate set of experiments with Mr. John Aitken's new forms of thermometer-screen were completed last year by Mr. H. N. Dickson, who has discussed the results in connection with those obtained by him with the same apparatus on Ben Nevis. Experiments with various anemometers are still in progress. Atmospheric dust is being collected on several islands in the Firth of Forth, by means of large funnels and carboys, which are periodically emptied and the contents forwarded to Mr. Murray for examination. Monthly trips along the Firth of Forth for the observation of temperature and salinity have taken place regularly from river to sea; preliminary results have been communicated to the Royal Society of Edinburgh from time to time, and a complete discussion of salinity is nearly ready for publication. It shows remarkable relationships between salinity and configuration, which have suggested new definitions of the words *river*, *estuary*, and *firth*. Special attention has been devoted to the relation of salinity and temperature to tide in the estuary of the Forth. Besides the observations of the scientific staff of the Station, thermometer readings are taken by volunteer observers at different parts of the Forth river-system and in the adjacent parts of the North Sea. The *Medusa* has made regular trips on the Clyde since April last at intervals of two months. Temperature and salinity observations are made in all parts of the estuary and firth from Dumbarton to the North Channel, and in all the connected lochs. These trips have yielded results of great interest and novelty. They are communicated in several papers to various Sections of the present meeting. The temperature of two deep fresh-water lakes—Loch Lomond and Loch Katrine—has been observed at all depths once a month since November 1885, in continuation of Mr. J. Y. Buchanan's work. Daily temperature observations have been established on a number of rivers and at several points on some. The Station has charge of observations on the Thurso, in the north of Scotland, the Forth and Teith, and the Tweed; and it has also been the means of inducing independent observers to undertake similar work on the Tummel (a tributary of the Tay), the Tay, and the Derwent, in Cumberland. These are all salmon rivers, and the observers being interested in fishing have already succeeded in showing some connection between temperature and the movements of salmon. In consequence of experience gained in physical marine investigations the apparatus used for the purpose has been progressively modified and improved—the Scottish thermometer-frame and water-bottle may be pointed to as special instances. The Station has, since September 1885, been able to advise and assist several public bodies in starting observations of temperature and salinity, the National Fish Culture Association of England, the Dundee Harbour Trust, and the Fishery Board for Scotland being amongst the number. Thermometers have been lent to several naturalists for use on short scientific voyages. The collection of all existing records of sea and river temperature round the coast of Scotland is proceeding, and promises, when completed, to be of great value in showing the different sea-climates of the east and west coasts—a question of much importance in relation to the distribution of marine species.

HUGH ROBERT MILL, D.SC., F.R.S.E.

Report upon the Depth of Permanently Frozen Soil in the Polar Regions, its Geographical Limits and Relations to the Present Poles of Greatest Cold, by a Committee consisting of Lieut.-General J. T. Walker, C.B., F.R.S., General Sir J. H. Lefroy (Reporter), Prof. Sir W. Thomson, Mr. Alex. Buchan, Mr. J. Y. Buchanan, Mr. John Murray, Dr. J. Rae, Mr. H. W. Bates (Secretary), Capt. W. J. Dawson, R.A., Dr. A. Selwyn, and Prof. C. Carpmael.—The inquiry referred to the Committee necessitated reference to residents in many distant regions, and time must elapse before any large harvest of observations can be hoped for; nevertheless, the Committee are in a position to quote several valuable communications, especially one from Mr. Andrew Flett, adding materially to what was previously known on the subject of the extension of permanently frozen soil, or ground ice, in America. It will be convenient to arrange the data now available, in their order of latitude.

1. Lat. 71° 18′ N., long. 156° 24′ W.—At the wintering station of the United States Expedition of 1881-82, under Lieut. P. H. Ray, United States America, that officer found the temperature of the soil 12° F. at 28 feet from the surface, and the same at 38 feet.

2. Lat. 68° N., long. 135° W.—At Fort Macpherson, on Peel River, Mr. Andrew Flett, who passed twelve years there, reports:—"The greatest depth of thawed-out earth I came across round that post was 3½ feet, October 10, 1865. The greatest depth of frozen ground was 52 feet 3 inches, September 27, 1867, near the mouth of Peel River. The bank had fallen in; at the bottom the perpendicular cliff, which I tried with a boat pole, was frozen as hard as a rock. A black sandy soil. The surface was not above two feet thawed-out. The cliff was measured with the tracking line." This account leaves it doubtful whether the frost may not have entered the soil from the face of the cliff. On the other hand it is evident that it extended to a greater depth from the surface than was measured.

3. Lat. 67° N., long. 142° W., on the Youcon.—The same gentleman writes:—"I spent twelve years on the Pelly or Youcon River, on the west side of the Rocky Mountains. Round old Fort Youcon ground ice is found at 6 feet; this I have seen in the river banks in September where they had caved in; but no particular notice has been taken as far as I know by any one, unless it be Chief Factor Robert Campbell, now residing in Merchiston, Strathclair, P.O., Manitoba."

4. Lat. 65° N., long. 120° W., on the Mackenzie River, about ten miles above the mouth of Bear River.—The same gentleman writes:—"I have seen many landslips on the Mackenzie, which

more frequently takes place in rainy weather—July, August, and sometimes September: but I never examined them particularly excepting one, which we came near being buried by in camp. This was about August 15, 1876. By a pole, I found the bottom of the slide frozen hard, a grey clay and gravel mixed, *from where the earth broke off was not over 6 feet.* The surface soil sandy. Some way back from the river bank the country is muskeg more or less, and by removing the moss by hand we came to hard frozen ground in August." The sentence printed in *italic* is somewhat ambiguous. It is understood to mean that the bank was not much more than 6 feet high, and was hard frozen at that depth; the depth to which the frost extended is therefore unknown.

5. Lat. 64° 20′ N., long. 124° 15′ W., on Mackenzie River. —The face of a cliff from which a recent land-slide had occurred, was measured by the present reporter in June 1844. The soil was frozen to a depth of 45 feet from the surface. (*See* "Magnetic Survey," p. 161.)

6. Lat. 62° 39′ N., long. 115° 44′ W., at Fort Rae, on Great Slave Lake.—Capt. Dawson, R.A., observed the temperature of the soil monthly at his station of circumpolar observation, 1882–83. The following table contains his results in degrees Fahr.:—

Months	1 Foot	2 Feet	3 Feet	4 Feet
1882	°	°	°	°
September	40·6	37·9	36·1	34·5
October	32·5	32·7	32·5	32·3
November	23·9	29·1	30·9	31·3
December	15·8	24·6	28·8	30·8
1883				
January	8·3	19·9	25·7	28·5
February	11·1	21·2	24·5	26·3
March	9·5	20·8	22·7	24·8
April	18·9	25·2	24·3	25·3
May	34·0	32·0	33·8	30·5
June	43·5	36·5	32·4	31·5
July	48·0	41·0	37·0	34·5
August	47·3	41·9	38·5	36·5

The mean temperature of the air at 5 feet 10 inches above the surface, in the same months, was as follows:—

1882		1883	
September	44·40°	February	−10·41°
October	32·59	March	−7·71
November	9·30	April	19·30
December	−15·20	May	36·30
		June	51·49
1883		July	61·11
January	−26·80	August	56·50

We learn from this table that the soil is frozen at a depth of 4 feet from November to June inclusive, and is at the lowest temperature at that depth in March. It further shows that, like the waters of the Scottish lakes, as proved by the observations of Mr. J. Y. Buchanan and Mr. J. F. Morrison in Loch Lomond and Loch Katrine last winter, the mean temperature of the soil reaches its minimum about the time of the vernal equinox. The rise of earth-temperature in February above that recorded in either January or March is remarkable. It does not appear, from the conver ence of the lines when projected, that temperatures below 32° F. extend lower than 11 or 12 feet. Capt. Dawson writes:—"There are two reasons why these earth-temperatures are above what is probably the average in that latitude. (1) The ground had a slope of 1/16 to the south-west; and (2) it was fully exposed to the rays of the sun; now, in most places, the ground is either covered with thick moss or shaded by brushwood, and its surface-temperature on the hottest day is not likely to exceed 70° F., whereas earth exposed to the rays of the sun may easily reach a temperature of 120° F." Fort Rae is situated on a long arm or inlet of Great Slave, baving a depth of 10 or 12 feet of water.

7. Lat. 62°, long. 129° 40′, Jakutsk, Siberia.—The great depth of permanently frozen soil in this part of the valley of the Lena has long been well known; but the following extract, translated from a recent paper by Dr. Alex. Woeikof, of St. Petersburg, entitled "Klima von Ost-Siberien," contains infomation on the influence of local conditions which will make it of value to observers, and we therefore reproduce it.

"The further north," he remarks, "the longer is the duration of cold in valleys in comparison with that on higher ground. The effect extends to a part of autumn and spring, and is observable in the mean temperature of the year."

The following observations of earth-temperatures are a proof:—

	At Depth				Limit of
	20 ft.	50 ft.	300 ft.	381 ft.	Frozen Soil
Jakutsk[1]	13°·6	17°·1	25°·0	26°·6 Fahr.	620 feet.
Mangan mine	22°·1	25°·2	...	...	269 ,,
Schelou mine	22°·1	25°·7	...	...	298 ,,

Thus, on heights in the vicinity of Jakutsk (these are heights on the left bank of the Lena, near Jakutsk) the earth temperature is from 8°·1 to 8°·6 F. higher than it is in the town and valley at the same depth, and it is even lower at 300 feet in the former than at 50 feet in the latter locality. The total depth of frozen soil is, according to Mittendorf ("Sibirische Reise," Bd. i.) more than twice as great in the valley as it is on the heights: and observe that these lesser heights are in winter relatively colder than higher isolated mountains. Mittendorf also states that no frozen soil was found at 60 metres above the level of the river at the mouth of the Maja, in Aldan, but that it was found four miles and a quarter up the stream at three metres above the level of the river, and that about 28 miles further, in the mountains, there is a deep hollow from which aqueous vapour is constantly rising.

Kuppfer asserts that in Bergrivier Nertschinsk, in the Trech Swjatitilei mine, frozen soil was found at a depth of 174 feet, but that in Wossdwischenst mine, which lies 230 feet higher, the frozen soil ceased at 50 feet. Even in Altai it is acknowledged that many valleys are colder than the neighbouring heights.

Dr. Woeikof sums up a number of observations in the following sentences, which apply to the greater part of East Siberia, but more particularly to the north-east portion.

(1) As the greater cold coincides with calms and light winds, the valleys and lower grounds are colder than the heights.

(2) The temperature of isolated mountains is relatively higher than that of lesser elevations.

(3) The lowering of temperature in the valleys is so lasting and considerable that the mean of the year is also lowered, as is proved by the observations of earth-temperature.

(4) The depth of the frozen soil is greater in valleys than on the neighbouring heights, probably also than it is on the higher mountains.

(5) In the tundras of the far north (answering to the barren grounds and muskegs of the North-West Territory of Canada), the winter is warmer than in the valleys of the forest-zone. Probably because the stronger currents of the air do not permit the cold stratum to remain so long stagnant.

8. Lat. 61° 51′, long. 125° 25′, Fort Simpson, on Mackenzie River.—The summer's heat was found in October 1837 to have thawed the soil to a depth of 11 feet, below which was 6 feet of ground ice (Richardson), making the depth of descent of the frost 17 feet. The result is anomalous; at other posts in the same region the summer thaw is much more superficial. Thus, it will be observed above, that in the month of October, at Fort Rae, the soil was at a nearly uniform temperature, but slightly above the freezing-point, from the depth of 1 foot to 4 feet. Franklin found a summer thaw of only 22 inches at Great Bear Lake, and the writer was informed that it was only 14 inches at Fort Norman (lat. 64° 41′). Fort Simpson is situated on an island of deep alluvial soil, bearing timber of large size, and possessing an exceptional climate.

9. Lat. 57°, long. 92° 26′, York Factory, Hudson's Bay.—Sir J. Richardson has stated that the soil was found frozen to a depth of 19 feet 10 inches in October 1835, the surface being thawed to a depth of 2 feet 4 inches.

10. Lat. 55° 57′, long. 107° 24′, Lake à la Crosse.—It is stated that no frozen soil was found in sinking a pit to a depth of 25 feet in 1837, and that the earth was only frozen to a depth of 3 feet in the winter of 1841. Both records are anomalous, and call for verification.

11. Lat. 53° 40′, long. 113° 35′, at Prince Albert, on the Saskatchawan.—Mr. W. E. Traill, who was in charge of this post in 1872, reports that a settler in the neighbourhood came to frozen ground at a depth of 17 feet, but did not learn whether they passed through the frozen strata, or, if such was the case,

[1] M. Schergin's shaft.

what was the thickness of it. The same gentleman, writing from Lesser Slave Lake (lat. 55° 33′), remarks that he has never come across any indication of perpetual ice during the twenty-two years he has passed in the North-West Territory.

12. Mr. Andrew Flett, writing from Prince Albert, April 21, 1886, says:—"Hundreds of wells have been sunk in this settlement; one I had sunk myself, beginning of July 1881, 27 feet deep—saw no frozen earth. As far as I have noticed on this prairie land, when there is a good fall of snow when the winter sets in, the frost does not penetrate so deep as when there is no snow till late, and in some years very light snow. I had a pit opened on the 9th inst. (April); the surface was thawed 3 inches; we got through the frozen earth at 4 feet 7 inches. On the 11th inst. I saw a grave dug in the churchyard at Emmanuel College, one mile from my place, 5 feet deep, and had not got through the frost. My place is on higher ground, loam soil."

13. Mr. W. Ramsay, settled on the South Saskatchawan, thirty-five miles from here, sunk a well 40 feet, May 27, 1884—no frost.

14. Mr. Jos. Finlayson, three miles from here, sunk a well beginning of July 1882, 46 feet. He saw no frost.

15. Mr. J. D. Mackay, on the same section as the above, sunk a well 27 feet, July 15, 1884, found particles of frozen earth at 7 feet deep.

16. Mr. W. C. Mackay, my next neighbour half a mile west of this, sunk a well about June 20, 1884, found particles of frozen earth at $5\frac{1}{2}$ feet.

17. Lat. 53° 32′, long. 113° 30′, Fort Edmonton, on the Saskatchawan, 2400 feet above the sea.—Dr. James Hector, on March 5, 1858, found the soil frozen to a depth of 7 feet 6 inches (*Journal R. G. S.* vol. xxx. p. 277).

18. Lat. 51° 14′, long. 102° 24′.—At Yorkton, Mr. J Riaman, when digging a well last summer (1885), found the frost at a depth of 19 and 20 feet, and continuing for a depth of 30 inches. In this case, therefore, the total depth to which frost descended was about 22 feet. Mr. J. Tarbotton, of Yorkton, in communicating the last observation, remarks:—"The depth to which frost penetrates during the winter, varies, I find, with the character of the winter itself and with the nature of the locality. I made observations in an open unprotected spot, where there was little or no snow, and found frost to the depth of 5 feet 9 inches. This occurred last July, and the frost was then about 2 feet deep (*i.e.* had descended to 7 feet 9 inches). But in the bluffs near my house, I dug a cellar, at the same time, going down between 8 and 9 feet, encountering no frost at all.

"This year, however, when digging another well in April, in almost the same place, I encountered frost at 2 feet, and the ground continued solid until I had gone down from $4\frac{1}{2}$ to 5 feet from the surface. From this, and from the information I obtained from others, I am safe in saying that the frost penetrates here on an average 5 feet, except when we have had a great depth of snow in the beginning of winter, in which case it does not penetrate nearly so far. The bluffs referred to are groves of poplar from 3 to 6 inches in diameter, on the edge of an open plain."

Prof. Charles Carpmael, Director of the Meteorological Service of Canada, to whom most of the above reports were addressed, remarks:—"We can easily imagine that at a depth of 17 feet at Prince Albert, there might be no frost at all in winter, but owing to the slow travelling downward of the wave of cold, it might have reached a depth of 17 feet in the early summer.

"It is easily seen that the annual mean temperature of the air might be considerably below the freezing-point without the occurrence of permanently frozen soil, for in winter the soil is often covered deep in snow, so that the temperature of the soil might be but little below 32°, although the temperature of the air were 30° or 40° F. below zero. Again, the heat which had entered the soil in summer would only be removed by slow conduction, whereas the summer heat would not only travel downwards by conduction, but be carried into the soil by percolation of the warm water through the surface."

19. Lat. 50° 30′, long. 103° 30′, the Bell farm, near Indian Head.—Frozen soil is said to have been met with in the summer of 1884 at a depth of $12\frac{1}{2}$ feet; details are wanting.

20. Lat. 49° 53′, long. 97° 15′, city of Winnipeg and the neighbourhood.—Mr. Ch. N. Bell reports that frozen soil has been found as under in various cemeteries.

Brookside Cemetery on the open prairie close to the city, soil rich black loam, varying in depth from 1 to 2 feet, subsoil heavy grey clay.

		On the Higher Ground Ft.	In.	On the Lower Ground Ft.	In.
December 23, 1884	... Frozen to	0	10	2	2
January 3, 1885	... ...	1	0	3	0
March 21, ,,	... ...	1	4	3	6
May 6, ,,	... ...	4	4	5	0
June 25, ,,	... None down to	6	0	6	0
January 14, 1886 ...	...	0	10	1	6

A further communication of June 1, 1886, states that the frost only descended 3 feet 6 inches on the higher ground in the winter of 1885-86, and had at that date disappeared. It descended 5 feet in the lower ground, but had almost disappeared.

At. St. John's Cemetery in the city, "I am advised by the clergyman," says Mr. Bell, "that frost has been found at from 5 to 8 feet depth"; careful investigation will be made there this year.

St. Boniface, a suburb of Winnipeg to the east.—The frost penetrates from 5 to 8 feet, according to the season, varying locally under the conditions of the exposure, tillage, dryness, and heat or frost cracks. During the summer of 1885 frost was found at a depth of 5 feet, and down to 7 feet, when the work was stopped. This was in July or early in August. The locality was probably exposed to the action of the sun.

21. Lat. 49° to $49\frac{1}{2}$°, long., in the valley of the River Pembina to the extreme south of the North-West Territory.—Dr. Alfred Selwyn, Director of the Geological Survey of Canada, who has two sons settled in this region, states that those gentlemen have had several wells sunk, the deepest about 40 feet, and have never seen any permanently frozen ground. There is similar negative evidence from Brandon, a little further north.

It would be premature to draw any general conclusions from the observations thus far collected. There is want of proof of the existence of permanent ground ice beyond the district of Mackenzie River in the North-West, but frozen soil has been shown to exist at a depth of 17 feet at Fort Simpson, at Prince Albert, and at Yorkton, and it may be questioned whether the wave of summer heat has time to descend to such a depth before it is overtaken by the refrigerating influence of the early winter. It certainly exists also in the neighbourhood of Hudson's Bay, on the eastern side, and it is evident that under favourable conditions frost, without being permanent, may in some cases last in the soil all the year round over a wide area, and in other years disappear.

At whatever level we locate the maximum of absorbed heat, it must be remembered that when the winter sets in, and freezes the surface, which it does rapidly to the depth of a foot or two, the heat will then be abstracted in both directions, and its rate of descent checked.

Report of the Committee, consisting of Sir Joseph D. Hooker, Sir George Nares, Mr. John Murray, General J. T. Walker, Admiral Sir Leopold McClintock, Dr. W. B. Carpenter, Mr. Clements Markham, and Admiral Sir Erasmus Ommanney (Secretary), appointed for the Purpose of Drawing Attention to the Desirability of Further Research in the Antarctic Regions.—Your Committee, after having given full consideration to the great importance of effecting a further exploration of the Antarctic Polar Sea, desire, in the first place, to express their opinion that it would be most essential, before approaching Her Majesty's Government with the view of urging the expediency of equipping such a naval expedition as would be required for the carrying out an exploration of such magnitude, interest, and importance, that the requirements for its success and a plan of operations should be most carefully considered, and the results embodied in a written form for the approval of the Council of the Association and for the information of the Government. Furthermore, in order to obtain the co-operation which the matter requires from eminent men in science, your Committee feel it necessary for their body being enlarged by the addition of influential members of the Association, and of other bodies representing the various branches of science interested in the investigation of this comparatively unknown region, and especially of the Royal Geographical Society. Your Committee have to point out that our knowledge of the South Polar region is chiefly confined to the grand discoveries effected by that celebrated expedition under the command of Capt. Sir James C. Ross, conducted between the years 1839 and 1843 with sailing-ships. Since that period the facilities for effecting a more complete research have been greatly augmented by the application of steam propulsion to vessels better adapted for ice-navigation.

This has been proved by continuous experience in the Arctic seas during the late half-century. For the above reasons your Committee deem it desirable to defer making their report, with a view to giving more definition to the objects sought to be obtained and to the best means of obtaining them, as also to expand this Committee, in order to elicit to the fullest extent the opinions and to secure support from those conversant with the various branches of science which are to be investigated during an exploration which, from its very important and serious nature, eminently merits the favourable consideration of this great and enterprising maritime nation.

NOTES

THE 59th annual meeting of the Association of German Naturalists and Physicians will take place at Berlin from the 18th to the 24th inst. General meetings will be held on the 18th, 22nd, and 24th, the sections, of which there are thirty, meeting at other times when and where they wish in the various places offered them for that purpose. At the same time there will be an exhibition of scientific apparatus, instruments, and educational objects. On the morning of each day a journal will be issued containing information of interest to members, and as much as possible of the proceedings at the various meetings of the preceding day. The Physical Section is under the Presidency of Dr. von Helmholtz and Dr. Kirchhoff. Amongst the papers to be read are the following:—The microscope as an aid to physical investigation, by Dr. Lehmann; the determination of the electro-chemical equivalents of silver, by Dr. Köpsel; electrical discharges, by Dr. Goldstein; on Palmieri's investigations into the development of electricity in the condensation of steam, by Dr. Kalischer. The Presidents of the Chemical Section are Drs. Hoffmann and Landolt. In this Section there will be papers on silver oxydul, by Herr von der Pforten; a new synthesis of naphthaline derivatives, by Dr. Erdmann; and on a peculiar phenomenon of reaction, by Herr Liebreich. In the Botanical Section there will be papers on Goethe's influence on botany, and on the reception of water by the external organs of plants. In the Zoological Section papers will be read on dual eyes in insects, on the origin of the frontal ganglion in Hydrophilus, on freshwater Bryozoa, the Protozoa of Kiel Bay, on the boundaries of zoo-geographical regions from the point of view of ornithology, the fauna of North German lakes, and on the old Peruvian domestic dog. In the Section for Geography and Ethnology there will be several papers on Africa, especially on the Congo region; one on the Kurds, others on South Polar exploration, on the Goajira Indians, and on the importance of the Xingu for the ethnology of the northern part of South America. A great majority of the sections are occupied with medical subjects. One of these will be devoted to the discussion of the condition of Europeans in different climates, their diseases, acclimatisation, &c. The last section of all is devoted to scientific education.

SIR HENRY ROSCOE has given notice that in the next session of Parliament he will call attention to the Report of the Departmental Committee on the National Science Collections, and will move a resolution.

IN reply to a question by Sir John Lubbock in the House of Commons on the 9th inst., the Chancellor of the Exchequer stated that the appointment of a Minister of Education, as recommended by the Committee of 1884, had not yet come under the notice of the Government, nor could he hold out any hope that it would be likely to come very soon under its notice. Lord Randolph Churchill said he suspected the proposal would involve an increased charge upon the public revenues, "and every alteration, reform, or modification of a department which would involve an increased charge possesses in my eyes an incurable defect."

EARTHQUAKES have continued at Charleston during the past week, but the shocks are decreasing greatly in frequency and violence. One occurred on Saturday night and one on Sunday, but no harm was done by either. The Mexican Government has been officially informed that Tequisixtlan was shaken by an earthquake at 4.30 on the morning of September 3. The movement was from east to west. A Naples correspondent of the *Times* writes that the shock of the 28th ult. was severer than any which has been felt for some years. The panic was therefore great, and was increased by superstition. There were two shocks—one was horizontal, the other vertical, but they followed each other in such rapid succession that they appeared to be one shock, and for many hours after the replica was expected with much apprehension. The shocks occurred about 11 p.m., and were felt severely at every place in the Bay of Naples, and in the Island of Capri, which has no volcanic element in its formation. Similar reports were received from Puglia, Calabria, and Sicily, where the shock was very severe. At Forio, in the Island of Ischia, it was felt, and created a panic. Vesuvius has long been in a state of comparative repose Prof. Palmieri says that at 4 p.m. on the 28th it showed signs of renewed activity by frequent thunders, and by throwing masses of lava into the air.

MR. POND, the Government Analyst of New Zealand, has proved by actual experiment that the dust thrown out during the recent volcanic eruptions is of a highly fertilising kind. He obtained samples of the dust from three different places, and sowed a quantity of clover and grass seeds in each. The soil was kept moistened with distilled water, so that no manurial elements might be imparted by the water used. In all cases the growth was almost as vigorous as in rich volcanic soil. The rapid growth of the plants and their colour show that the dust is a benefit to the soil on which it has fallen.

THE programme for the autumn meeting of the Iron and Steel Institute, which is to be held in London on October 6, 7, and 8 next, has just been issued. The Council of the Institute has arranged to hold the meeting in London this year, for the second time in the history of the Society, with a view to affording Members the opportunity of studying the mineral resources, &c., of the colonies, as illustrated by what is shown at the Exhibition, and of coming into contact with colonists and Indians who are interested in mineralogical operations. That being so, perhaps the most interesting paper in the list is one on the iron-making resources of our colonies, prepared by Mr. Gilchrist (whose name is associated with the well-known basic process) and Mr. Edward Riley. Among other papers to be read there is one on the chemical composition and mechanical properties of chrome steel, by M. Brustlein; another on combustion with special reference to its application in the arts, by Mr. F. Siemens; another on the treatment of high-class tool steel, by Mr. A. Jacobs, of Sheffield; and one on modifications of Bessemer converters for several charges, by Mr. John Hardisty, of Derby.

THE Paris Academy of Sciences has issued in separate form the text of the discourses pronounced at the Museum of Natural History on the occasion of M. Chevreul's centenary, August 31, 1886. The speakers were M. Fremy, Director of the Museum; M. Jules Zeller, President of the Institute; M. Janssen, on behalf of the Academy of Sciences; M Broch, Corresponding Member of the Institute; Colonel Le Mat, in the name of the Washington National Institute; M. Ressmann, Italian Plenipotentiary; M. Gilbert Govi, President of the Neapolitan Academy of Sciences; M. de Bouteiller, on behalf of the Paris Municipal Council and the General Council of the Seine; M. Chaumeton, President of the Association of French students; MM. Nadault de Buffon, Dehérain, Leroy, Auguste Vitu, Gerspach, and René Goblet. The brochure is printed in uniform size and type, with the weekly *Comptes rendus* of the Academy.

THE Paris newspapers have published a congratulatory communication from the Academy of Sciences in Berlin to M. Chevreul, from which the following is an extract:—"He who would form a complete idea of your so busily occupied life should follow the entire course of your creative activity, which has been directed to all departments of chemistry. He must follow all the innumerable detailed researches which have enabled you to determine the nature of various minerals and of a large number of salts, as well as the composition of many organic matters. He should study your chemico-physiological works, by which you have made such great advances towards the knowledge of the most important secrets of the animal organism as well as your labours on the most varied questions of public hygiene. He ought to follow the excursions which enabled you to fix the laws of the contrasts among colours, and to class them systematically and scientifically. He ought to study your lectures on the chemical principles of dyeing. He should finally imagine himself at the period when misty ideas of the most false and fantastic kind threatened to surround and obscure the mind, and when, with the record of history in your hands, you dissipated the mists by making your countrymen recognise in the delusions of the past the errors of the present time. Having thus represented in all its extent the activity that you have shown throughout your long life, we hold that your name should be inscribed in one of the first places on the list of the great men who have carried the scientific glory of France to the extremities of the earth."

IN a long communication to the *Times* of September 7 from its Correspondent with the Grenada Eclipse Expedition, it is stated that a botanical garden is in course of formation under Mr. Elliott, with a view to the development of the resources of the island. Mr. Elliott, the article goes on to state, "has made frequent excursions into the high woods for the latter purpose, and the results of his botanical exploration of the island, which will soon be made known, are very satisfactory, many valuable woods having been found. It is hoped by means of the botanic garden to encourage the planters of the Windward Islands to cultivate the minor products of this fertile region, and especially to improve their fruits by exhibiting the finest kinds in the gardens brought from other regions and giving information showing how it can be done. No more healthy sign of the progress of the colony could be afforded than in the enterprise of the Colonial Government in establishing such a garden and the interest taken in it by the planters of the island. So much for applied botany. During the stay of the Expedition pure botany has been studied with much success by Mr. George Murray, of the British Museum, the naturalist attached to the Expedition, who arrived a fortnight before the observers of the eclipse to gain the necessary time for finding good working grounds. His special mission is an inquiry into the life-history of certain marine *Algæ* called the *Siphoneæ*. The forms of this group are well enough known to European botanists, but the development and life-phenomena of most of the genera composing it have not yet been investigated. He has been greatly gratified to find an abundant supply of material for his special research, though the island is poor in *Algæ* owing to the small rise and fall of the tide, which exceeds a foot only at spring tides. The operation of examining this material is conducted in a well lighted and very convenient room set apart for the purpose by His Excellency the Governor, who has in this instance, too, shown the greatest sympathy with the object of the work, and an unfailing helpfulness towards its accomplishment. So far as the examination of the material collected has gone, it promises to yield an answer to the question of the nature of the reproduction and development of the types investigated, but whether the information will result in fixing definitely the position of the *Siphoneæ* as a group or in the breaking up of the group and the incorporation of certain genera into other orders, already better known, cannot, of course, yet be determined. The land and marine fauna are also engaging Mr. Murray's attention to the extent of collecting these, and the reptiles and small mammals have in this department of work been kept particularly in view."

MR. JOHN TAYLOR, a pupil of Dr. Marshall Ward, who went out some time ago as botanist to the Bahamas Government, is evidently pursuing his work under difficulties. While he and his companions were on shore at Acklin Island, 30 miles from Long Cay, on August 13, the cook, a Haytian, who was left in sole charge of the vessel, made off with it under cover of darkness, and up to the 19th no trace of ship or cook was found. Mr. Gardiner had on board nearly all his scientific books, and all the instruments, &c., necessary for a month's good work. He lost everything of that kind he had, including his Zeiss microscope; besides all his manuscript scientific diary, and list of the Bahamas flora, not to mention a sum of money, bedding, &c. His total loss he estimates at 75*l.*; we are sure the Bahamas Government will not allow Mr. Gardiner to sustain a loss which to him must be serious.

THE translation of an English botanical book into German is so much a reversal of the present fashion that it is of some interest to know that a translation of Dr. Maxwell Masters' "Vegetable Teratology" has been made by Mr. Udo Dammer, and published by Haessel of Leipzig. Many additional notes have been added by various German and Italian botanists, as well as by Dr. Masters, and some additional woodcuts provided.

THE celebrated waterfall of Teverone, which Horace calls "*præceps Anio,*" has been employed to put in operation two dynamos of 100 horse-power for the illumination of the city of Tivoli. Others are being fitted up. The motive power, which is to be utilised by a company from the designs of M. Cantoni, is equivalent to several thousand horses. The illumination of Rome is contemplated, as well as the distribution of force to a distance from the station. The excavations and canals are conducted under the house of Mæcenas, which is described as situated at *udum Tibur*, now Tivoli.

TWO more of the Paris theatres are now illuminated by incandescent light—the Palais Royal by Edison, and the Variétés by Woodhouse. With the Opera and the Eden Theatre this brings the number up to four. Every evening the Industrial Exhibition at the Palais des Champs Elysées is lighted also by electricity.

WE have received a copy of a paper read by Mr. H. C. Russell before the Royal Society of New South Wales, on "Local Variations and Vibrations of the Earth's Surface," in which he records his own experiences on this subject in the hope that other astronomers will do the same, and thus by united action assist in the work of tracing these vibrations and changes. Mr. Russell's observations took place at Lake George, and were made chiefly by means of an automatic recorder of the height of the water in the lake. Although the instrument used has not the extreme sensitiveness to minute vibrations which Mr. Darwin's reflecting mirror and similar instruments have, yet it was so placed that all such changes became magnified by the relatively enormous extent over which it extends its sensitive part, if this expression may be used; for any change in gravity, or the direction of the vertical, is not seen as it affects the base of a small instrument a few feet square, but as it affects a surface 20 miles long and 5 to 6 miles wide. Barometric and wind changes, too, so difficult to see in other instruments, at once became evident here by their effects on such a large body of water, and the lake-gauge for these reasons is not only capable of showing changes quite as minute as the Cambridge pendulum apparatus, but also of keeping a perfectly satisfactory record of

these changes, so written that many, if not all, the causes can be traced in the curves which they produce. Various tables and diagrams are appended to the paper.

COLONEL GILDER has started from Winnipeg on his expedition to the Arctic regions, with the object of reaching the North Pole.

THE deaths are announced of M. Paul Soleillet, the explorer of Shoa in North-East Africa, and of Herr Robert Flegel, the explorer of the Niger and Binué.

THE celebrated traveller and botanist, Dr. Schweinfurth, lectured recently at Berlin on the Kew Botanical Gardens, which he characterised as the finest in the world. Kew, he said, is the Botanical Foreign Office for all nations, for it is the centre of all botanical news from all parts of the world.

WE have received "An Account of the Progress of Astronomy in the Year 1885," compiled for the Smithsonian Institution by Prof. W. C. Winlock. The investigations reviewed comprise (among others) Prof. Pritchard's photometric researches as consigned in the Oxford "Uranometria," M. Dunér's catalogue of stellar spectra of the third type, Drs. Gill and Elkin's determination of southern star-parallaxes, Prof. Bakhuysen's of the rotation-period of Mars, Prof. de Ball's of the nutation constant, Prof. Peters' of the orbit of 61 Cygni, and Prof. Langley's inquiries into the temperature of the moon's surface. M. Faye's theory of the origin of the solar system, with Prof. G. H. Darwin's criticisms upon it in NATURE, are prominently dealt with. We hear with pleasure of the progress towards completion of Prof. Rowland's photographic map of the normal solar spectrum. The amount of detail contained in it may be judged of from the one fact that 120 lines are visible between H and K, the original negatives showing 150. The most striking astronomical events of the year, *i.e.* the outburst of new stars in Andromeda and Orion, the photographic discovery of a nebula in the Pleiades, and the meteoric shower of November 27, are chronicled in due and interesting detail. Seven comets were observed in 1885, of which five were seen for the first time, the others being expected returns of Encke's and Tuttle's. Nine minor planets were discovered. The Report concludes with a useful bibliography of astronomical works published in 1885.

IN March, 1884, Prof. Holden offered to observe at the Washburn Observatory the 303 fundamental stars for the southern zones of the "Astronomische Gesellschaft." The offer was accepted; the work was begun May 2, 1884, and finished December 25, 1885. The results are contained in vol. iv. of the "Publications of the Washburn Observatory," now before us. When Prof. Holden was appointed to the Lick Observatory in October 1885, 468 observations were still wanting to complete the series. These were very creditably supplied, before the end of the year, by his assistants, Mr. Updegraff and Miss Lamb. In all, 6444 observations were made with the Repsold meridian-circle; each star of the 303 was completely observed six times; and instrumental constants were determined for each night. No pains were spared to secure accuracy. The probable error of a single bisection of Polaris was estimated at not above $0''.1$ for poor seeing, and $0''.05$ under the most favourable conditions. A list of corrections to standard star-catalogues (p. 69) forms a valuable addition to the contents of the volume.

WE have to acknowledge the receipt of the Calendars for the Session 1886-87 of the University Colleges of Dundee and Bristol, and of the Durham College of Science of Newcastle-on-Tyne.

THOSE interested in natural history will be glad to hear that Mr. Quaritch issues this week vol. iii. (the "Quadrupeds") of the Memorial Edition of "Bewick's Works," which he is publishing—to be complete in five volumes.

THE additions to the Zoological Society's Gardens during the past week include a Spring-bok (*Gazella euchore* ♂) from South Africa, presented by Capt. John Hewat, C.M.Z.S.; two Talapoin Monkeys (*Cercopithecus talapoin*) from West Africa, presented by Mr. R. E. Dennett; two Red-headed Finches (*Amadina erythrocephala* ♂ ♀) from South Africa, two Saffron Finches (*Sycalis flaveola* ♂ ♀) from Brazil, presented by Mr. H. B. James; a Leadbeater's Cockatoo (*Cacatua leadbeateri*) from Australia, presented by Mr. J. Davis; a Roseate Cockatoo (*Cacatua roseicapilla*) from Australia, presented by Mr. G. H. Hawtayne, C.M.Z.S.; a Herring Gull (*Larus argentatus*), British, presented by Mr. E. Penton, jun., F.Z.S.; a West African Python (*Python sebæ*) from West Africa, presented by Major A. Morton Festing; a Smooth Snake (*Coronella lævis*) from Hampshire, presented by Mr. W. H. B. Pain; a Leonine Monkey (*Macacus leoninus* ♂) from Arracan, deposited; two Crested Pigeons (*Ocyphaps lophotes*), a Geoffroy's Dove (*Peristera geoffroii*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

THE INNER SATELLITES OF SATURN.—Writing in the *Astronomische Nachrichten*, No. 2743, Prof. Asaph Hall states that he has now finished the reduction and discussion of the observations of Titan and the five inner satellites of Saturn made at Washington since the mounting of the 26-inch refractor in 1873. These observations have been made with the filar micrometer, and most of them are observed angles of position and distances. The average probable error of a single observation for the position of a satellite is $\pm 0''.27$. A remarkable result of the discussion is that the Washington observations of the five inner satellites can be satisfied within the limits of their probable errors by circular orbits. At the beginning of this discussion Prof. Hall hoped that the observations would determine the positions of the lines of apsides with such accuracy that the motions of these lines would be known, and that thus we might obtain data for a new determination of the mass of the Ring, and of the figure of the planet. But the resulting circular orbits for the inner satellites make the position of a line of apsides indeterminate, and for the present the mass of the Ring remains unknown.

On account of the difficulty of making good micrometrical measurements of the inner satellites of Saturn, astronomers have revived the old method of observing their conjunctions with the ends of the Ring, or with some other marked feature in the Saturnian system. A series of observations of conjunctions with the ends of the Ring was made at Toulouse in 1876 and 1877 by MM. Tisserand and Perrotin; and in order to test the old method of observing, Prof. Hall has compared these measures with his elements of the five inner satellites. The result at which he arrives is that the probable errors of a single residual are larger for the Toulouse observations ($= \pm 0''.41$ in the mean) than in the micrometrical measurements at Washington. Prof. Hall therefore concludes that observations of these satellites with filar micrometers are among the best we have, and since they are definite measurements, and are made in very different positions, a result deduced from them is more likely to be free from constant errors. He suggests that probably the best way to effect an improvement in such measurements is to devise some new arrangement of the wires of the filar micrometer.

THE INVENTION OF THE SEXTANT.—Dr. J. L. E. Dreyer points out, in the *Astronomische Nachrichten*, No. 2739, an historical error which has crept into several astronomical works, although it was refuted some fifty years ago by Prof. Rigaud in a series of papers communicated to the *Nautical Magazine*. In the books referred to, it is stated that the principle of the construction of the sextant was communicated to John Hadley by his brother, a Capt. Hadley, who had in his possession a sextant given to him by Capt. Godfrey, brother of Thomas Godfrey, of Philadelphia, the real inventor of the instrument. But it appears there never was such a Capt. Hadley. The brothers of John Hadley were—one a barrister, the other a physician; and he himself was not an instrument-maker by profession (as has been asserted), but, as an amateur, occupied himself with mechanical pursuits, and was the first to bring the polishing of reflecting-telescopes to any perfection. On May 13, 1731, John Hadley

communicated to the Royal Society a description of his reflecting octant; and, after some hesitation, Halley declared himself satisfied that Hadley's idea was quite different from that of Newton, who had invented an instrument founded on the same principle. It is no doubt true that Thomas Godfrey, a glazier of Philadelphia, had invented an instrument of this kind about the year 1730; but the first intelligence of his invention did not reach England before the month of May 1732, in a letter from James Logan to Halley. Godfrey's instrument was made of wood by Edmund Woolley, a carpenter, about November 1730, and had been tried on board the ship *Truman*, of which John Cox was master. The first model of Hadley's octant had, however, been constructed by his brother George about the middle of the summer of 1730. The thanks of those interested in the history of astronomy are due to Dr. Dreyer for the effort which he has made to correct the errors on this point which are found in Poggendorff's "Biographisch literarisches Handwörterbuch," in Wolf's "Geschichte der Astronomie," and elsewhere.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *SEPTEMBER* 19–25

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on September 19

Sun rises, 5h. 43m.; souths, 11h. 53m. 42·7s.; sets, 18h. 5m.; decl. on meridian, 1° 25′ N.: Sidereal Time at Sunset, 17h. 59m.

Moon (at Last Quarter September 21) rises, 20h. 39m.*; souths, 4h. 7m.; sets, 11h. 44m.; decl. on meridian, 15° 12′ N.

Planet	Rises	Souths	Sets	Decl. on meridian
	h. m.	h. m.	h. m.	° ′
Mercury ...	4 55	11 29	18 3	6 0 N.
Venus ...	3 50	10 45	17 40	9 56 N.
Mars ...	10 44	15 11	19 38	18 12 S.
Jupiter ...	7 7	12 52	18 37	3 43 S.
Saturn ...	23 34*	7 37	15 40	21 32 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich)

Sept.	Star	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image
			h. m.	h. m.	° °
20	130 Tauri	6	23 34	0 30†	68 229
21	26 Geminorum	5½	22 46	23 26	23 273
25	B.A.C. 3345	6	1 53	2 41	61 206

† Occurs on the following morning.

Sept.	h.	
22	12	Saturn in conjunction with and 3° 29′ north of the Moon.
23	—	Sun in equator.

Variable Stars

Star	R.A.	Decl.		
	h. m.	° ′		h. m.
U Cephei	0 52·2	81 16 N.	Sept. 21,	19 4 *m*
ζ Geminorum	6 57·4	20 44 N.	,, 24,	0 57 *M*
U Monocerotis	7 25·4	9 32 S.	,, 21,	*M*
U Cancri	8 29·3	19 17 N.	,, 25,	*M*
δ Libræ	14 54·9	8 4 S.	,, 21,	2 11 *m*
U Coronæ	15 13·6	32 4 N.	,, 22,	1 45 *m*
U Ophiuchi	17 10·8	1 20 N.	,, 22,	4 27 *m*
			and at intervals of	20 8
β Lyræ	18 45·9	33 14 N.	Sept. 18,	21 30 *M*
			,, 22,	2 0 *m*
η Aquilæ	19 46·7	0 43 N.	,, 25,	5 0 *m*
S Cygni	20 3·1	57 40 N.	,, 24,	*M*
U Cygni	20 16·1	47 32 N.	,, 20,	*M*
T Cephei	21 8·0	68 2 N.	,, 20,	*m*
δ Cephei	22 24·9	57 50 N.	,, 22,	22 0 *M*

M signifies maximum; *m* minimum.

Meteor Showers

The following are amongst the showers of the period:—Near θ Cassiopeiæ, R.A. 14°, Decl. 50° N.; near α Arietis, R.A. 31°, Decl. 18°; and near Polaris, R.A. 68°, Decl. 87° N.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

University College, London.—We notice from the prospectus of the Engineering Department that the examination for the Gilchrist (Entrance) Engineering Scholarship of 35*l.* per annum is to be held on the 28th and 29th inst. Candidates must be under nineteen, and the subjects of examination are:—(1) Mathematics; (2) any two or more of the following—(*a*) mechanics, (*b*) mechanical drawing, (*c*) examination on some subject connected with engineering, (*d*) French or German, (*e*) the use of tools. The examination is intended to be of such a standard as can be passed by lads from school who have begun to acquire some knowledge of mechanical pursuits. The appliances of the engineering laboratory (under Prof. Alexander B. W. Kennedy) have been very much extended during the past year, mainly through a grant from the Gilchrist Trustees, and are now very complete in the direction both of experiments in elasticity and the strength of materials, and in the economic work of engines and boilers. Laboratory work is so arranged that students go through a systematic course of experimental instruction in these and other connected subjects during the session.

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, September 6.—M. Émile Blanchard in the chair.—On presenting to the Academy a copy of a volume issued on the occasion of M. Chevreul's centenary, August 31, 1886, M. Berthelot remarked that this seemed a suitable occasion for reviving the old custom of celebrating Academic solemnities by the publication of special scientific and literary essays. The present work, in the preparation of which MM. Ch. Richet, G. Pouchet, E. Grimaux, E. Gautier, Dujardin-Beaumetz, E. Demarçay, and Berthelot had co-operated, has been executed with rare taste and care by the editor, M. Alcan, and by him dedicated to M. Chevreul on behalf of himself and his fellow-contributors.—Fluorescence of the compounds of manganese subjected to electric effluvium in vacuum, by M. Lecoq de Boisbaudran. In the experiments here described the author has aimed especially at determining the effects due to the presence of manganese. The fluorescence of some of its compounds is an extremely sensitive reaction, by means of which imponderable traces of this metal may be detected in natural or artificial substances that might otherwise be supposed free from its presence.—Paralytic ataxy of the heart, by M. Mariano Semmola. In this communication the author resumes the results of his further observations on cardiac disorders, already reported in the *Transactions* of the International Medical Congress, seventh session, London, August 1881.—Remarks in connection with three Italian essays submitted to the Academy, by M. Govi. The first of these papers deals with an episode in the life of Galileo, showing that the hostility of the Jesuits to the Florentine philosopher was not due to the letter addressed by him to his brother in 1606, announcing the expulsion of the Order from Venice. The second describes a curious plano-convex lens executed by Torricelli some time between 1644 and 1647, and recently discovered in the Cabinet of Physics attached to the University of Naples. The third refers to an unpublished letter written by Volta in 1785 on Lavoisier's pneumatic theory, which, although not accepted without reservations, is defended against the assumptions of an Englishman named Lubbock, who had essayed to transform oxygen into a new principle called by him the "sorbile principle."—On certain differential equations of the first order, by M. Roger Liouville. It is shown that the differential equation—

$$y' + a_1 y^3 + 3a_2 y^2 + 3a_3 y + a_4 = 0,$$

is reducible to the quadratures if its coefficients $a_1, \ldots a_4$ and their derivatives $a'_1, \ldots$ satisfy the equation—

$$a_1 L' + KL^{\frac{5}{3}} - 3[a'_1 + 3(a_2^2 - a_1 a_3)] L = 0,$$

where L represents the combination

$$L = a_2 a'_1 - a_1 a'_2 + a_1 (a_1 a_4 - a_2 a_3) + 2a_2 (a_2^2 - a_1 a_3,$$

and K an arbitrary constant which may vanish.—Note on the

theory of dissociation, by M. G. Chaperon. It is argued that the theory of dissociation may be expressed with simplicity by means of certain cycles, which are easily formed, if the possibility be admitted of separating at a constant temperature several mixed gases or vapours without the expenditure of convertible labour or of heat.—On the conjugation of the Paramecia, by M. E. Maupas. Fresh observations on *Paramecium caudatum* have led the author to the determination of a fact of great physiological importance, which had hitherto escaped his notice, and which seems to foreshadow in these organisms the fecundating processes of the higher animals.—On the family of the Polyclinians, by M. Lahille. The Polyclinians of Roscoff—studied, for the first time, in 1872 and 1873, by M. Giard—are here divided into two distinct families—Polyclinidæ and Aplididæ.—On the affinities of the Eocene ferns of West France and Saxony, by M. Louis Crié. The already-determined affinities of the Tertiary flora of these two regions are here considerably enlarged by a comparative study of their respective ferns.—Note on the telluric currents, by M. J. J. Landerer. The author's further observations on meteorological phenomena connected with electricity lead to the general inference that the great telluric current of the globe has its origin in the difference of the negative potentials. The constancy and amplitude of the *régimes* of the winds whence it results insure both its normal direction and permanence.—On the discovery made in Belgium of a grave contemporary with the mammoth and rhinoceros, by M. Nadaillac. In a cave recently explored by MM. Marcel de Puydt and Sohest near Spy, in the province of Namur, were discovered two skulls of the Neanderthal type associated with the remains of *Rhinoceros tichorhinus* and *Elephas antiquus* (?). It thus appears that the Neanderthal race had already penetrated to the Meuse valley in the remotest times. From the relics found in the undisturbed soil of this cave it appears that they could make flint implements, utilise the tusks of the mammoth, manufacture earthenware baked in the fire, that they buried their dead, and in a word possessed the first rudiments of civilisation.

BERLIN

Chemical Society, July 12.—C. Scheibler, Vice-President, in the chair.—Ferd. Tiemann gave an account of some reactions of substituted amidoximes.—C. Scheibler discussed the important question for the sugar industry, whether a definitely characterised strontium dihydrate ($SrO,2H_2O$) exists as such, or whether the substance having the percentage composition of a dihydrate is not rather a monohydrate (SrO,H_2O) containing a higher hydrate mixed with it. He described his experiments on the action of carbon dioxide on the hydrates of the alkaline earths at different temperatures and containing varying amounts of water. He finds that the facts agree with the latter view.—H. Noerdlinger has studied the oxidation products obtained by the action of nitric acid on myristic acid: the chief products are succinic and adipic acids, besides smaller quantities of glutaric, pimelic, suberic, oxalic, and carbonic acids.—R. J. Friswell and A. G. Green described their researches on the constitution of diazoamidobenzene, from which it is concluded that the constitutional formula $C_6H_5 . N : N . NH . C_6H_5$ usually assigned to it is the correct one.—M. Rosenfeld described lecture experiments for the demonstration of the volumetric decomposition of hydrochloric acid and of the sublimation of sulphur.—J. Bongartz gave an account of compounds which aldehydes, ketones, and ketonic acids give with thioglycolic and thioacetic acids.—R. Otto discussed the conditions under which the whole of the arsenic can be removed from hydrochloric acid by hydrogen sulphide, and he showed that the last traces of arsenic can be precipitated when the addition is made of a certain quantity of a substance which gives an insoluble precipitate with the hydrogen sulphide. Since crude hydrochloric acid always contains such substances (*e.g.* ferric chloride, chlorine, &c.), it can readily be freed from arsenic by means of hydrogen sulphide.—P. Klason gave an account of a new method for the estimation of sulphur and of the halogens in organic compounds by burning them in a current of oxygen.—H. Kiliani has isolated the lactone of levulosecarboxylic acid, and has more closely examined the pentoxypimelic acid resulting from the oxidation of dextrosecarboxylic acid, and also its lactone.—E. Fischer has obtained a base named isoglucosamine, $C_6H_{13}NO_5$, by the reduction of phenylglucosazone with zinc dust and acetic acid, aniline and ammonia being simultaneously produced; isoglucosamine is isomeric with glucosamine, and closely resembles the latter in its properties, and probably bears the same relation to levulose as glucosamine does to dextrose.—J. Tafel described a new method of preparing primary amines, which consists in the action of sodium amalgam and glacial acetic acid on the alcoholic solution of the substances produced from ketones or aldehydes and phenylhydrazine; the reaction takes place according to the equation—

$$R : N_2H . C_6H_5 + 4H = RH . NH_2 + NH_2 . C_6H_5.$$

—E. Erlenmeyer offered an explanation of the remarkable isomerism occurring in the cinnamic acid and acrylic acid series.—K. Heumann and Th. Heidlberg are experimenting with a view to ascertain the influence exerted on the shade of certain dyes by the introduction of substitution groups and elements: in the present communication they describe the effect produced by the introduction of chlorine.—W. Staedel and H. Bauer gave an account of their experiments on the methylation of metanitraniline; on the demethylation of tertiary aromatic amines, and also on a convenient method of preparing azo-compounds.—G. Ciamician and P. Silber had a paper on the constitution of certain di-substitution derivatives of pyrroline.—K. Elbs and G. Steinike have studied α-naphthylphenylketone.—W. Kelbe has found ordinary cymene and an aromatic hydrocarbon of the formula C_9H_{12} in rosin-spirit.—R. Anschütz and P. N. Evans have found that antimony pentachloride boils under diminished pressure without appreciable decomposition.—A. G. Ekstrand gave the results of his research on the naphthoic acids; he has prepared and described the various nitro-derivatives.

BOOKS AND PAMPHLETS RECEIVED

"American Journal of Mathematics," vol. viii. No. 3.—"18th and 19th Annual Reports of the Trustees of the Peabody Museum," vol. iii. Nos. 5 and 6 (Cambridge, Mass.).—"Field and Other Experiments Conducted on the Farm and in the Laboratory of Sir J. B. Lawes, June 1886."—"A History of the Theory of Electricity," vol. i., by Isaac Todhunter and Karl Pearson (University Press, Cambridge).—"Industrial and High Art Education in the United States," by J. E. Clarke (Washington).

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THURSDAY, SEPTEMBER 23, 1886

EXTERIOR BALLISTICS

Exterior Ballistics in the Plane of Fire. By James M. Ingalls, Captain First Artillery, U.S. Army, Instructor. (New York: D. Van Nostrand, 1886.)

CAPTAIN INGALLS has succeeded in presenting within the limits of 128 pages, for the most part a very good and complete account of the various methods now in use for calculating range tables and solving important problems relating to trajectories of shot. The subject of ballistics is divided into three parts—interior ballistics, which treats of the motion of the shot within the bore of the gun, of which very little is known; exterior ballistics, which deals with the motion of the shot after leaving the muzzle and till it strikes an object; and the remaining part treats of the penetration into an object struck. The author confines his attention to exterior ballistics. His book is purely mathematical, but well adapted to the wants of the artillerist. It is in reality a second edition.

In Chapter I. theoretical resistances are calculated for various forms of heads of elongated shot; and Chapter II. is devoted to the consideration of experimental resistances, where reference is made to the experiments of Robins (1742); Hutton; Piobert, Morin, and Didion (1839-40); Virlet (1856-58). Then follows a brief notice of experiments made with the Bashforth chronograph at Shoeburyness. The dates of the publication of the first and final reports of these experiments are correctly given 1870 and 1880 respectively, but there is no notice of an intermediate report (84/B/1879), printed by Government and circulated in 1879, giving coefficients of the resistance of the air for elongated shot for all velocities between 430 and 2250 f.s. (131 and 686 m.s.). But the first publication of the results of these experiments will be found in the *Transactions* of the Royal Society for 1868 for velocities 900 to 1600 f.s. (p. 441). Mention is then made of some rather meagre experiments said to have been made by Mayevski in 1868-69 with both spherical and elongated shot. In the latter case the assistance derived from the results of the English experiments is fully and candidly acknowledged by Mayevski, whose results it appears were not published till 1872. And lastly, Mayevski's and Hojel's discussion of Krupp's experiments made at Meppen (1881) are noticed. Some confusion is caused here from the intermixture of dates of experiments and dates of their publication, but it must be evident that the publication of Bashforth's results was not anticipated by any experimenter here named for the whole range of velocities 100 to 2800 f.s. for elongated shot, and for velocities 850 to 2150 f.s. for spherical shot, excepting in the latter case the results of Robins, Hutton, and Didion.

Capt. Ingalls then proceeds to explain the methods of determining the coefficients of resistance. He passes over the ballistic pendulum, which is however not yet quite obsolete, and first deals with the case where velocities of a shot are measured at two points a known distance apart. He gives a table of the 16 determinations of Didion's ρ' for spherical projectiles by Mayevski (published 1872), and then he passes on to Mayevski's and Hojel's formulæ for elongated shot (1882). It does not appear why priority is given to these recently published results. These being disposed of, the author gives a very good account of the manner in which Bashforth obtains his coefficients from the observed times occupied by the shot in passing over a succession of equal spaces (pp. 31-35). Capt. Ingalls has expressed the law of resistance from these English results in terms of the powers of the velocity, upon which he remarked that the expressions deduced by Mayevski and Hojel from the Krupp experiments give a less resistance. He does not consider at all the nature of the experiments from which these contradictory results were obtained, but at once comes to the desired conclusion that "This is undoubtedly due to the superior centring of the projectiles in the Krupp guns over the English, &c." But we very much doubt this statement. Further on, the experiments given in the 'Annexe à la Table de Krupp" are calculated by (1) Krupp's tables; (2) by Ingalls' tables (reduced by using $c = \cdot 907$, as had before been done in the *Proceedings* of the R. A. Institution, Woolwich, 1885); and (3) by Mayevski's formulæ. The agreement between calculation and experiment is apparently about equally satisfactory in all three cases. But the calculation by Ingalls' tables supposes a reduction of over 9 per cent. in Bashforth's coefficients used in calculating these tables. We have not had an opportunity of examining the Krupp experiments of 1881, from which Mayevski's formulæ of 1882 are said to have been derived, unless they be those contained in Krupp's Paper xxx., which gives the particulars of a few "expériences pour déterminer la résistance de l'air aux grandes vitesses." In that case we are informed that no less than *six* chronoscopes were used in *pairs* to measure velocities at 30, 130, and 1000 metres from the gun. This amounts to a confession that the particular instrument used was not to be relied upon. We have not space to pursue the question further, but we must direct particular attention to the group formed of rounds 7-10, fired July 5, 1881, stated as follows:—

Round		v at 30 m. No. 301	No. 302		v at 130 m. No. 292	No. 293		v at 1000 m. No. 114	No. 115
7	...	896·4	892·5	...	855·9	850·9	...	—	—
8	...	903·8	894·5	...	852·7	862·7	...	—	—
9	...	907·4	887·2	...	857·6	856·7	...	438·1	—
10	...	907·4	911·4	...	854·1	834·7	...	—	—
Means	...	903·8	896·4	...	855·1	851·3	...	438·1	—
		900·1 m.s.			853·2 m.s.			438·1 m.s.	

This is one of the six groups of rounds of which the experiment consists. It will be observed that there are large differences in some of the velocities measured at the same point by two different instruments, and that there is only a single velocity measured by one chronoscope at the distant station. It is quietly assumed that this one velocity of 438·1 m.s. determined from round 9 is perfectly correct and applies equally to all four rounds 7, 8, 9, and 10! All the results deduced from this group depend entirely upon this *single* velocity. Chronoscope No. 115 never came into action at all, and No. 114 only once. It is difficult to imagine a worse experiment. From all this it appears that it is necessary to be very cautious in

adopting the results of so-called experiments. It must be evident that the efforts of the Krupp party are directed to spread an impression that his system of constructing guns and projectiles has some mysterious property of reducing the resistance of the air. Now it is perhaps fortunate that the English experiments were made when the Service guns did not shoot quite so steadily as they do now, because all the observations were made near the gun when the motion of the shot was most nearly in the direction of motion. Although one gun was an extremely good one—we will suppose that the average of the four guns gave coefficients slightly above those due to perfectly steady motion in direction of the axis of the shot. Let us consider now what actually takes place on a long range. The elevation we will suppose 10°, and also that the shot leaves the muzzle with perfect steadiness. The tendency of the shot is to preserve the parallelism of its axis, but the curvature of the trajectory soon causes the axis of the shot to be inclined to the direction of motion. The resistance of the air then acts *obliquely* on the shot, and so tends to place the axis in the direction of motion. If it succeeded in accomplishing this feat at any instant, all would be out of order the next moment. In this way the axis is kept *nearly* in the direction of motion. Our shot would perhaps fall at an angle of 12°, making 22° as the angle through which the axis of the shot had been turned during its flight, by the *oblique action* of the resistance of the air. This oblique action of the air causes other disturbances, as "drift," &c. Thus if in the English experiments the shot moved with their axes at times slightly inclined to the direction of their motion they would give coefficients more nearly corresponding to the conditions of their motion on long ranges than if they had been obtained from shot moving with the axis exactly in the direction of their motion.

Afterwards Capt. Ingalls treats of the general properties of trajectories, the rectilineal motion of shot, and the calculation of tables. He explains the methods of calculating trajectories adopted by Euler, Bashforth, Niven, and Siacci. Numerous examples are given to illustrate and explain these methods, and examples taken from Bashforth's treatise are worked out by approximate and other methods.

The work concludes with three ballistic tables adapted for the calculation of trajectories by Siacci's approximate method. Table I., for spherical shot, is based upon Mayevski's coefficients (1872); Table II., for elongated projectiles, is based on Bashforth's coefficients; and Table III. is said to be copied from Didion, who copied from Euler. This table is given by Otto for every minute up to 87° 0′, which is its most complete form.

OUR BOOK SHELF

Illustrations of the Indigenous Fodder Grasses of the Plains of North-Western India. (Roorkee: Nature-printed at the Thomason Civil Engineering College Press, 1886.)

THIS is an atlas of forty plates, the representations in which are most natural and life-like, the characteristic habit of each species being effectively shown. About half the plates are accompanied by diagrams of the spikelets or florets. Of the 40 selected species, 7 belong to Andropogon, 7 to Panicum, 3 to Eleusine, 3 to Eragrostis, and 2 each to Aristida, Cenchrus, and Paspalum. The 14 remaining genera, represented each by 1 species, include, amongst others, Saccharum, Setaria, Sorghum, and Sporobolus. All the species shown are extra-British, excepting *Cynodon Dactylon*, Pers. [and *Panicum Crus-Galli*, L.]. Of these grasses none perhaps is of greater current interest than *Sorghum halepense*, Pers., known amongst English-speaking peoples as "Johnson grass," respecting the drought-withstanding capacity of which very favourable reports continue to be received from Australia and from the Western United States. Mr. J. F. Duthie, under whose careful supervision the work has been published, states in a short introduction that "the increasing demand for reliable information concerning the various grasses used in this country, either as fodder or forage, has induced me to collect materials for the preparation of a work embodying all the available information on this very important subject." This admirable atlas is a contribution in the direction indicated, and the descriptive letterpress, which Mr. Duthie promises to have ready by next cold season, will be welcomed by those—and their number is rapidly increasing—who are interested in the economic study of the Gramineæ.

W. FREAM

Exercises on Mensuration. By T. W. K. Start. (London: Sampson Low and Co., 1886.)

A WRITER who invariably mis-spells "hypotenuse" speaks of squaring two numbers and "subtracting the results," and treats of the area of a triangle before the area of a rectangle, does not deserve success. Yet so unsuited for non-technical schools is the scope of most of the existing books on mensuration, that a little manual like this of 32 pp. has an excellent chance in the struggle for existence. We hope the present edition may be rapidly sold, and followed by a second edition thoroughly revised. T. M.

Lectures in the Training Schools for Kindergartners. By Elizabeth P. Peabody. (Boston: D. C. Heath and Co., 1886.)

IN these eight lectures, which have been addressed during the past nine or ten successive years to training classes for Kindergarten teachers in Boston and elsewhere, Miss Peabody explains the system of Froebel, and the principles on which it rests. The very first sentence of the first lecture shows the serious view entertained by Miss Peabody of the duties of such teachers: "Whoever proposes to become a Kindergartner according to the idea of Froebel, must at once dismiss from her mind the notion that it requires less ability and culture to educate children of three, than those of ten or fifteen years of age. It demands more."

Le Mouvement scientifique et industriel en 1885. Causeries scientifiques. Par Henry Vivarez. (Paris: Librairie Centrale des Sciences, 1886.)

THIS volume is a republication of a number of sketches on scientific subjects contributed weekly to the journal *La Gironde,* with a view to keeping the readers of that periodical *au courant* with the progress of science in its various branches. They are therefore popular, and are made as entertaining as possible. The writer has the gift, so common amongst his countrymen, of rendering the most technical and abstruse subject clear and interesting. The "Causerie" is peculiarly a French device in journalism: hitherto it has been mainly devoted to literature and the drama. M. Vivarez has applied it with much success to science. It would be absurd to speak of this as a work of science, but it certainly is a work in which the latest results of science are explained and illustrated for the million.

LETTERS TO THE EDITOR

The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

British Association Sectional Procedure

As one who has attended fourteen consecutive meetings of the British Association, with seven years' experience as a Sectional Secretary, as one, moreover, who has a high opinion of the benefit these meetings may confer upon aspiring learners and isolated workers, I venture to submit a few observations on the details of the conduct of the business of the Sections—a subject, I think, of general interest and importance.

My remarks apply directly to Section A, and only touch Sections B and G incidentally. I have no means of knowing anything about the conduct of the business of other Sections. But as regards Section A every one must feel how great a burden the papers are becoming, and how impossible it seems to deal with them adequately on the present system. The papers sent in are good and interesting almost without exception—there is no fault in the quality of the papers, it is their superabundance in proportion to the time that constitutes the evil. It is disheartening to a man who has taken much care and trouble in some piece of work, and in preparing an account of it to lay before the Section in order to get advice and encouragement for further labours, to come on at the end of a long day when every one is tired—the seniors many of them absent, and to find it expedient to abstract even his abstract, and give a hasty and barely intelligible account of what he has done: the next paper being hurried on without any adequate discussion in order to try and get through the list. I speak with the greater freedom because I have no personal grievance whatever: what I say merely represents a feeling which is certainly prevalent. Moreover, I by no means imply that any one is to blame. The effective conduct of the business of a Section in which titles of papers are liable to be handed in at the last moment is a most difficult matter, and one can only grow wise by experience.

This year the experiment has been tried of sitting on only four complete days, and of putting 35 papers down for one of them in order to avoid a Wednesday sitting if possible. My impression of the general feeling is that this has been a failure, and has resulted in a dull meeting. There are several objections to not sitting on a Saturday—not the least of which is that the break between Friday evening and Monday morning is rather a long one, and the temptation to go away is strong.

In the days when I received the traditions of the Secretaryship from Mr. J. W. L. Glaisher, a Saturday sitting was the universal rule, and it was devoted to pure mathematics. At Bradford this pure mathematical day was a most memorable and brilliant one. In those days, moreover, joint sittings with other Sections were unheard of. Electricity was then almost unknown to Section G, and the border region between physics and chemistry was less active. Nevertheless the press of papers was even at that time considerable, and a long Wednesday sitting was a frequent occurrence.

At the present time joint sittings, or semi-joint sittings, with B and G, and even with C and D, are liable to have to be provided for; and these joint sittings and interdiscussions are surely likely to be among the most fruitful and instructive periods of the whole meeting, and everything should be done to encourage them. But if time is to be found for any such discussion or joint meeting, and if at the same time the papers accepted are to be adequately treated, so as really to encourage, and not discourage, research, then I may take it as evident that it is quite hopeless to attempt to avoid sitting on both Saturday and Wednesday. Saturday is in many respects a favourable day for a sitting of A, for so many of the other Sections are then free that a good audience is frequently obtained, and opportunity for a junction of Sections is afforded. It is true that the innovation of Saturday excursions is supposed to militate against this arrangement; but Saturday excursions should not begin till middle day; moreover, if excursions are to interfere with scientific business, there ought to be no question as to which should give way.

Assuming this granted, I proceed to indicate a plan for the classification of papers so as to get a convenient list on each day, and to endow each day with a character of its own. This is an old practice, but it seems in some danger of being discarded, and it is a most useful one. Let us consider the days in order.

On Thursday there are, of course, the Presidents' addresses; not only the address in A, but also those in B and G, which are likely to draw off a considerable number of physicists. Indeed, these Sections considerately postpone their commencement till 12 o'clock, for this most desirable interchange of members. Those least affected by either of these Sections are, perhaps, the astronomers; and therefore it is convenient to fill up the rest of Thursday with papers on astronomy, tides, terrestrial magnetism, and with reports. The day thus acquires a dignified and substantial character.

Friday is a day to be devoted to pure physics, *i.e.* to papers interesting to theoretical physicists as distinguished from those concerned with celestial or terrestrial applications. The day is suitable for mathematico-physical papers, for reports on theories of light and electricity, and such like.

On Saturday the pure mathematicians should take their innings as of old: they should be the main Section for that day, and should not consent to be shunted off into some small and unknown room. The rest of the Section may either meet as a sub-section to clear off a residue of papers on general or applied physics, if any were left over from Thursday; or, if opportunity offers, it may hold a conjoint meeting and discussion with some other Section, such as B or D in that Section's room.

Monday is a day devoted to meteorology; and the growing importance of this branch claims that it should have the time fairly to itself, and not be swamped by a multitude of other papers crowded into it from other days. But inasmuch as a number of members take only a general interest in meteorology they are set partially free for attendance at other Sections, and accordingly it is becoming customary for Section G to take electrical and other communications of a physical interest on this day, and for Section B to read its papers on physical chemistry.

Tuesday is another day for pure physics, like Friday; but inasmuch as the physical aspect of chemistry is manifestly growing in importance, it is probably feasible to take papers having a more chemical bearing, as well as all those which were omitted from Friday's list. Section B would doubtless be able to take its technical papers of less general interest on this day, as it did at the late meeting.

Finally, Wednesday is a most useful day, not only for clearing up arrears, but for a class of papers often very interesting, and yet such as should not be allowed to interfere with the more serious business of the meeting at times when the *dii majores* are likely to be present in force. These are papers on minor and semi-technical points—new batteries for instance, telegraphic and observatory details, rheostats, commutators, and all manner of things, not by any means unimportant, but yet involving no serious difficulty or novelty of principle. Some of these might be taken in the sub-section on Saturday. If, by reason of discussion on other days, there are arrears of work to be dealt with, then a sub-section to deal with them may be appointed for Wednesday. Splitting of Sections is undesirable (though it is much better than destroying the whole object of the meeting by undue haste), but if it has to be done, Saturday and Wednesday are the days for doing it; partly because the work to be accomplished is by that time known, partly because few other Sections are then meeting; but mainly because the important days of general interest, Tuesday and Friday, are thus left uninterfered with, and with their interest undissipated; while on Monday, when the interest is more special, and to a less extent on Thursday also, many members may be expected to wish to attend B or G, and an A sub-section in addition is quite undesirable. The possibility of meeting at 10 on Saturday and Wednesday is a feature which enables a good deal of work to be got through, before excursions on the one day and committee-meetings on the other put an end to the sitting. Let me summarise these suggestions.

Thursday.—President's address in A. Reports and papers on astronomy and earth physics. At 12 o'clock Presidents' addresses in B and G.

Friday.—Reports and papers on pure and more mathematical physics.

Saturday.—Reports and papers in pure mathematics. Also possible joint sitting with some other Section as arranged; or sub-section for minor experimental details.

Monday.—Reports and papers on meteorology and observational physics. Physical chemistry in B. Electricity in G.

Tuesday.—Reports and papers on pure and more chemical physics. Technical chemistry in B.

Wednesday.—Arrears and papers on minor or semi-technical experimental details. Simultaneous sub-section, if necessary, for clearing off arrears without haste.

It may be felt that this means a hard week's work. Well, it does. Attendance at these meetings, if attentive, is no child's play. But if any diminution is necessary, I submit that it is better to shorten each day's sitting than to lessen the number of days. One is fresh enough at 10, when the committee work begins, but pretty tired and hungry at 3. If interest dwindles, and papers begin to hurry themselves off without discussion, or to drone themselves dismally through, it is far better for the Section to rise at 2, instead of constraining itself to continue the process till the allotted hour. On the other hand, if interesting discussions arise, and attendance is good, it is very well to be able to continue the sitting till 3 or even longer. Though, indeed, Thursday is the only day on which a sitting may happily be continued beyond 3 without being disturbed by a committee meeting.

I have now said my say. I offer no apology for treating the subject, because my single aim in doing so has been to endeavour to do something to promote the usefulness and success of these meetings. OLIVER LODGE

University College, Liverpool, September 13

The Geological Age of the North Atlantic Ocean

WHILE the interest attaching to Sir William Dawson's Presidential address at Birmingham is still fresh, I wish to be allowed to offer a few observations on that part of it which deals with the geological age of the North Atlantic Ocean. The President in referring to those writers who, like Mr. Crosby in America, Mr. Mellard Reade and myself in Britain, maintain that the North Atlantic and the American continent have in the main changed places in Palæozoic times, makes the following statement. Admitting the correctness of the facts as to the swelling out of the Palæozoic sediments in the direction of the Atlantic seaboard, he endeavours to account for these very striking phenomena thus: "I prefer, with Hall, to consider these belts of sediment as in the main the deposits of northern currents, and derived from Arctic land, and that, like the great banks of the American coast at the present day, which are being built up by the present Arctic current, they had little to do with any direct drainage from the adjacent shore." Now, in reading this passage it occurs to me that Sir W. Dawson must have felt he had a very questionable case when he attempted to support it by such an hypothesis. To liken the great sheets of sediment which spread themselves sometimes over half the North American continent south of the Great Lakes to the banks heaped up along the Atlantic coast is a point of analogy in which, probably, he will find few to concur. The Palæozoic sediments are certainly not banks, but sheets originally spread over the sea-bed, and distributed according to certain recognised laws of increase and decrease of thickness.

But, putting this point aside, I may be allowed to ask, How can we suppose the existence of a northern current bringing sediment from the Arctic regions, and spreading it over Eastern America, unless there was at the same time a coast-line to guide the current in taking a southerly direction; and if such a coast-line existed, must it not have lain along the eastern American shore, because the American continent itself was then submerged? If we examine a current-chart of the globe, we find that all the N.–S. oceanic currents flow along the continental shores and take their directions from them. If America and the Atlantic, south of the Arctic regions, were both oceanic in Palæozoic times, then the current would not have been southerly, but westerly or easterly, according to circumstances, certainly not flowing from north to south; therefore this explanation for the distribution of the Palæozoic strata cannot, I venture to say, bear the test of examination.

Again, the President states: "It is further obvious that the ordinary reasoning respecting the necessity of continental areas in the present ocean basins would actually oblige us to suppose that the whole of the oceans and continents had repeatedly changed places." Now, as regards the North Atlantic, this is an objection which is purely imaginary; because the evidence goes to show that it remained in the condition of a continent all through the Palæozoic ages, with, of course, ever-varying margins; and it is only so far (as a writer in the *Athenæum*, September 4, has properly pointed out) that I have argued in favour of its continental condition. But undoubtedly the arguments in favour of the interchange of ocean and continent during Palæozoic times, as applicable to North America, would be found to apply more or less strictly to other oceans and continents, owing to the wide distribution of the formations of this period over the present continental areas. Northern and Central Africa and Greenland may prove exceptions; but apart from these tracts, Palæozoic strata appear to have been distributed (prior to denudation) over by far the greater portions of the continents, and the sediments must have been derived from the adjoining continental areas, which are now covered by the waters of the ocean.

The question between the President and myself is mainly this: Did the sedimentary strata of the Palæozoic period of North America come from lands lying around the Arctic Circle, or from others occupying the position of the North Atlantic? American geologists have a favourite theory that the Arctic regions have been the originating lands, but I venture to repeat that if it be allowed as a general principle that the originating lands lay in the direction towards which the sediments thicken, and opposite to that in which the limestones are most developed, the conclusion is inevitable that the Atlantic was in the main a land-surface in Palæozoic times. All the Palæozoic formations of North America point to this conclusion, as I have on former occasions attempted to show,[1] and this, regardless of the question whether or not there was also land along the Arctic Circle. Throughout the Silurian, Devonian, and Carboniferous epochs marine limestones were in course of formation mainly over the regions west of the Mississippi, and sediments mainly east of that line and chiefly in the Appalachian region. The general direction of the swelling out of the sediment is (if I mistake not) rather south of east than north of east. Thus, the "Potsdam beds" appear to swell out towards the E.S.E.; the "Hudson beds," S.E., and S.S.E.; the "Hamilton beds" of the Devonian, towards the E. or E S.E.; and different members of the Carboniferous series swell out N.E., E., and S.E. On the whole, and as a general result, the centre from which the sediments appear to have been chiefly distributed seems to have lain around the point intersected by the parallel 30° N. lat. and the meridian of 60° W. long., except in the Carboniferous period, when the originating lands appear to have lain in the region of the first Atlantic cable, between Newfoundland and the British Isles, and which lands were probably continuous with those of the Arctic continent.

I wish, in conclusion, to take this opportunity of adding a few words in reference to the Archæan rocks. I am much disposed to concur in the view of Sir W. Dawson—that the fundamental gneissose beds of the Archæan period may have had a different origin from the metamorphic strata of succeeding periods, and that they may not have been originally sediments. This observation does not, however, apply to the schists, limestones, and quartzites which succeed them, and which sometimes include beds of gneiss, as in Scandinavia. From this point of view, the birthday of the Atlantic continent may not have dated farther back than the commencement of the Palæozoic age—represented in Britain by the Cambrian, and in America by the Potsdam, sandstone. As a continent it remained till the close of that age. To what extent it survived the terrestrial movements which closed that epoch I am not prepared to say.

Dublin, September 15 EDWARD HULL

Earthquake at Sea

CAPTAIN H. J. OLSEN, commanding the brig *Wilhelmine* of Drammen, reports that, on the 1st inst., being by dead reckoning in lat. 50° 10′ N., long. 1° 40′ W., he observed, between 3.30 and 4 p.m., three rumblings at short intervals, during which the ship was felt to tremble violently, so that both the bulwarks of the cabin and plates on the table clattered. The wind was north-west, with a gentle breeze, and the ship was on the starboard tack. H. MOHN

Det Norske Meteorologiske Institut,
Christiania, September 15

Peripatus

NATURE for July 29 (p. 288) mentions that *Peripatus* has been taken at Demerara. It may interest some of the readers of

[1] *Scient. Trans.* Roy. Dublin Soc., vol. iii. 2 ser. p. 305 (1885), and "Contributions to the Physical History of the British Isles," p. 27 *et seq.*

NATURE to know that in January 1881 I captured a single specimen of *Peripatus* in the low, damp woods at Breves, on the island of Marajó, mouth of the Amazon. The specimen is now in the entomological collections of Cornell University, Ithaca, N.Y. JOHN C. BRANNER

Bloomington, Ind., U.S.A., September 2

THE RECENT EARTHQUAKE IN GREECE

I FORWARD the inclosed copy of a report made by the master of the steamship *La Valette* in reference to the earthquake which occurred in Greece last month, in case you may not have received the report and might wish to publish it.

W. J. L. WHARTON, Hydrographer

Admiralty, September 20

Report made by the Master of the s.s. "La Valette" to the Superintendent of the Ports, Malta, furnishing certain particulars in connection with the earthquake which occurred on August 27

On the 27th inst., at 11.30 p.m., whilst in lat. 36° 18′ N. and in long. 21° 32′ E., or at a distance of 50 miles W. ½ S. from Cape Matapan, I felt, all of a sudden, a very strong shock, which made the ship tremble, especially the engines, for the space of about 11 seconds. The ship was proceeding at the rate of 10 knots an hour, and with such shaking lost her course. The engineer thought that the screw had been lost. After the shaking was over all was right again. At midnight in the direction west-north-west, in lat. 36° 17′ N., long. 21° 27′ E., I observed on our right something like a mass of thick black smoke, which, like a cone, was rising up perpendicularly from the horizon, and at intervals changing into a reddish colour. In the meanwhile a perfect calm prevailed, with heavy sea from west at intervals. At 4 a.m. of the 28th, when the ship was in lat. 36° 12′ N., and in long. 20° 43′ E., the wind commenced blowing from north-west, which made the horizon a little clear. At 10 a.m. the mate, who was on watch on the bridge, reported to me that he had observed in the sea several stripes of a dark yellowish colour about one quarter of a mile long in the direction from north to south, which looked like shallows. The sea continued always heavy from west with very little wind. As the ship had a cargo of cattle, which suffer greatly from heat, I could not lose time in measuring the depth of the aforesaid stripes; therefore I tried to avoid them. During the navigation I thought proper to take precautions, as when I was at Alexandretta my owners informed me by telegraph of the report made by Capt. Tomlinson, of the steamer *Transition*. (Signed) CAPT. L. AQUILINA

Malta, August 29, 1886

THE TOTAL SOLAR ECLIPSE OF 1886

WE suppose that if, some months ago now, when the question of sending out an Expedition to Grenada during the rainy season was first discussed, any one had prophesied that out of a party of eight seven would see the eclipse and record results, the general feeling would have been that such a view would have been too sanguine. This, however, is what has happened, and so far as the securing of observations and photographs goes the Expedition must be pronounced a success.

With regard to the total result, however, no one is yet in a position to speak with certainty, for some of the photographs taken are not yet developed, and others, though developed, have not been submitted to any examination. On this point, however, we need not lay any great stress, for such photographs, though invaluable as records, do not help yet so much as such pictures will certainly be made to do hereafter in the matter of solar theory, for the reason that they are not large enough and not detailed enough.

Has, then, solar theory been advanced by the eye observations? From the sketch of the work done which appeared in yesterday's *Times*, from the pen of a Correspondent in Grenada, and which we reproduce, we think it has certainly. Prof. Tacchini's observation that the prominences seen most prominently during the eclipse were not the prominences seen by the ordinary method, and that the latter only reveals part of a very complicated phenomenon, is valuable in itself, but taken in connection with the fact that the eclipse prominences and the parts of the prominences not seen by the ordinary method are probably downrushes, wholly or partially, it is difficult to overrate its importance. These eclipse prominences, which Prof. Tacchini calls "white" prominences, are high and filamentous, and that distinguished observer, we know, does not hesitate to express his belief that the "comet" seen in the eclipse of 1882 was really one of them. If this be so, then the meteoric downpours of consolidating and consolidated materials are already *en évidence* with a vengeance, and these are the parts of the solar economy we want most to lay hold of just now.

That part of the *Times* Correspondent's letter which refers to the results obtained runs as follows:—

"The Green Island party was the only one doomed to disappointment. At Carriacou, Boulogne, Hog (or Fantôme) Island, and Prickly Point the eclipse was seen and results secured, although at these places even it was touch and go, the sky being cloudy everywhere. Carriacou was most highly favoured. During the totality the sky was cloudless, though the sun was covered one minute after the rim re-appeared. At Fantôme Island the last 40 seconds, and at Prickly Point the first 50 seconds, were lost. At Boulogne the clouds were still more persistent, and cut off 70 seconds of the totality, although Mr. Turner secured some observations during the four minutes before and the five minutes after. The presence of cloud during totality is a more serious matter than it might appear at first sight, for not only is the time reduced during which precious facts may be recorded, but pre-arranged programmes are interfered with, and it may be necessary to change them in order to meet the altered conditions. This requires a rapid and wise decision.

"Before I attempt to give any summary of the general results obtained, it may be remarked that the kinds of work attempted as a rule by eclipse expeditions are four in number, and are very distinct both in their methods and results from each other. We have first of all new facts, or new views of facts, which experience shows us are always obtained at such times, though they are not sought for as such. Next comes the testing of views which have been put forward to explain and harmonise the results previously obtained, and this part of the attack becomes very important when there are rival hypotheses in the field, the superiority of one of which can be established by a few critical observations. The third kind of work is the testing of the new methods of obtaining facts, the introduction of new instruments, or of new or improved ways of using old ones. Only in this way can a complete and perfect system of eclipse observation be built up. Finally we have the application of the ordinary methods of obtaining records, which for the most part are photographic. Astronomers not only want to study the phenomena of each eclipse to get at the physical and chemical structure and nature of the sun's atmosphere, but they want to note the changes from eclipse to eclipse, in order to see which phenomena are liable to variation, and the extent and period of such variation if it exists.

"Now in the eclipse observations secured in Grenada and Carriacou a distinct advance has been made along all the four lines to which reference has been made. New

facts have been acquired, old views have been satisfactorily tested, new instrumental methods have been studied, and records of the general phenomena have been secured. I will as briefly as possible go over each of these points in turn.

"First as to the new facts. For these we have to refer to the work of Prof. Tacchini at Boulogne. No one was more competent than he to note the prominences and other appearances visible during the eclipse. This he did with a 6-inch, and so soon as the clouds permitted after the eclipse he observed the spectrum of the prominences by the ordinary method. He found that the prominences seen under these two different conditions and by means of such different methods were not the same. He also noted that the prominences seen during the eclipse itself had the same characters as the so-called 'white' prominences which he observed in 1883 at the Caroline Islands. These appear whiter and dimmer as the distance from the photosphere increases. These observations have been very closely examined by Prof. Tacchini and Mr. Lockyer, with the result that both these solar observers are now prepared to ascribe these new phenomena to the descent of relatively cool material.

"It is difficult to over-estimate the importance of this result from the point of view of solar theory. The determination of the direction of the currents in the solar atmosphere is indeed so important that it was included in the programme of the observations to be made by Mr. Turner with his 4-inch finder, but no certain results were secured by this means, as the structure of the corona was apparently unusually complicated. In the spectroscope, however, one long streamer was observed to be much brighter near the limb. This is not absolutely conclusive evidence, but it has its value.

"To return, however, to Prof. Tacchini's other observations. He found that the prominences which were visible both during totality and by the ordinary method presented very different appearances, so that we are driven to the conclusion that by the latter we only see part of the phenomena. This entirely accords with Mr. Lockyer's recently published views, in which it is suggested that the metallic prominences seen near spots are really mixed up and down rushes, with probably an excess of the cooler descending material. Thus, for instance, the metallic prominences observed by the ordinary method after the eclipse were found to be only the central portions of those observed during totality, the part visible only during totality forming a whitish fringe round the more incandescent centre. Another very important observation was made. The 'flash' of bright lines, attributed by Prof. Young to the existence of a thin stratum which was supposed to contain all the vapours the absorption of which is registered by the Fraunhofer lines, was found to be due solely to the great reduction in the intensity of the light reflected by the earth's atmosphere allowing the spectrum of the higher regions to be seen the moment the lowest stratum of the corona was covered by the moon. This is carrying the unveiling of the spectral effects by the increasing darkness recorded in the Egyptian eclipse to its furthest limit, and it harmonises all the observations of this kind made since the eclipse of 1870.

"So much in the way of new facts and new ideas. We next come to the second kind of work, the testing of old ones. In this connection we have to refer to Mr. Turner's work at Boulogne and Mr. Perry's at Carriacou. Mr. Lockyer, before the eclipse of 1882, had been driven by a long series of experiments and observations to conclude that the lower part of the atmosphere was composed of successive strata giving different spectra, and that the sole cause of the difference was temperature. A test was possible during an eclipse, for then these lines of any substance seen to brighten when a higher temperature is employed in the laboratory should be seen shortest and brightest. The test was perfectly sharp and definite. It was applied during the eclipse of 1882, and the lines appeared as predicted.

"So far, then, the hypothesis which had enabled a prediction to be made which was subsequently verified was worthy of confidence. But this was a reason for repeating the observations to put the hypothesis on a wider basis. Mr. Turner did this, and found that the facts observed this year were the same as those recorded in 1882. It remains now for those who oppose Mr. Lockyer's views to give a more simple and sufficient explanation of those facts than he has done. Mr. Perry was to have extended the test further, but he failed to make the critical observation, as a large number of lines were seen, and those only for a short time, for the clouds came up directly after totality.

"Capt. Darwin was charged with a test of a different order. It was stated, after the eclipse of 1871, that the light of the corona was in all probability strongly photographic; and in 1875 the evidence in this direction was greatly strengthened, and some attempts were made to utilise this quality to obtain photographs of the corona without an eclipse. The efforts failed. More recently Mr. Huggins has tried the same methods with great precautions, and he has obtained appearances on his plates which resembled the corona, so that some thought that success had been achieved. The natural thing to do was to test the method during the progress of the eclipse to see if the appearances in question, due to atmospheric glare according to some, to the corona according to others, really resembled the corona when revealed by totality. Capt. Darwin's work seems to leave no doubt that the effect is due to glare only, and that the corona has nothing to do with it.

"Next as to new methods of attack. This year the only new method applied has been a change of the photographic manipulator, with a view of obtaining a much larger number of photographs and increasing the size of the images at the same time, by using larger lenses of longer focus and secondary magnifiers. Along this line success has not been complete, because the photographs have not been actually taken, as this new work was undertaken by Mr. Lockyer and his party at Green Island, and was clouded out. In spite, however, of this want of photographs, Mr. Lockyer will not hear of want of success. He holds that the problem has been solved.

"I have given an account of the work at Green Island, including the results of the rehearsals, and your readers will have been able in a large measure to form an opinion of their own. The improvement consists essentially in using four plates in one slide. The difficulty always has been in getting the slide in and out of position, so that the more plates we can work in one slide the more the difficulty and consequent loss of time are evaded. Another advantage lies in the use of a secondary magnifier, as by this means not only is the photographic image of the sun enlarged, but a system of cross wires can be introduced which permits of a perfect orientation of the picture obtained—that is, the exact east and west points on the circumference can be determined with the utmost precision, and from this the position of the various phenomena with regard to the sun's equator and poles. It can be easily imagined that on this point there must be no uncertain sound.

"We next come to the photographic record obtained by old methods—that is, methods dating in the case of photography of the corona from 1852, and in the case of spectrum photography from 1875. About twenty photographs of the corona have been obtained in all, and five photographs of the chromosphere and lower regions of the corona. Mr. Maunder obtained seven of the corona, and could have obtained more, at Carriacou. Captain Darwin obtained six, and Dr. Schuster, we believe, five, at Prickly Point. Of the photographs seven spectra, two with the

solar spectrum on the same plate—the only ones worth anything, have also been secured by Mr. Maunder. But we must not build too much on this, for, as I have said before, these photographs have not yet been developed; but if only one good one has been received, the laboratory work it should set going will take at least one or two years before the teachings of the precious record are exhausted. The so-called 'measurement' of such photographs is worth next to nothing.

"Among the records obtained on this occasion must be classed the disk observations, now for the first time included in the ordinary routine of eclipse work. The point of a disk observation is that an observer is by its aid able to observe the outlying solar appendages under the best conditions, so far as the sensitiveness of the eye is concerned. For ten minutes before totality the observer is blindfolded, and at the moment of the totality he is led to a small aperture through which, the bandage over his eyes having been removed, he sees a black disk some 40 feet away, which shuts off the moon and the brighter interior portion of the solar atmosphere. The eye, therefore, being thus shielded, is in the best position to pick up faint streamers extending beyond the borders of the disk, and to note their positions and extension. Streamers were thus noted at Grenada, extending far beyond the limits seen in the ordinary way, but the air was so saturated with aqueous vapour and incipient cloud, even where substantial clouds did not make their appearance, that the failure of any of the observers to see the equatorial extension observed by Prof. Newcomb in the clear sky of Wyoming, at an elevation of 7000 feet, in 1878, by no means proves that the extension was not there. The question of the continual existence of an extension of matter of some sort or other in the plane of the sun's equator must be held to be still *sub judice*.

"Capt. Archer at Fantôme Island, and Capt. Maling at Prickly Point, made disk observations fairly accordant. The former had greatly improved the disk provided him by surrounding it with concentric rings of wire, so that distances from the centre could be measured with the greatest accuracy.

"The records obtained by Prof. Thorpe regarding the intensity of the light of the corona were sufficient in number to suggest that when they are reduced a value will be obtained to be placed side by side for purposes of comparison with those previously obtained in 1870 and 1878. In this connection it may be remarked that the darkness of an eclipse must not be taken as a measure of the dimness of the corona, for, if the totality be longer, more of the brighter portion of the solar atmosphere will be covered. This was certainly the darkest eclipse seen since eclipse expeditions have been in vogue. This shows the importance of Prof. Thorpe's work, for if successful it will give us the luminous intensity per unit of surface of different regions of the solar atmosphere, as well as the intensity of the total light emitted.

"The preceding sketch of the results obtained has of necessity been of the most general character. Not till all the observations are published in detail, as they doubtless will be at no very distant date by the Royal Society, and not till they have been discussed by those competent to discuss them, can a final verdict as to their value be given. We have of set purpose dealt only with the conclusions which lie on the surface."

NOTES

THE death of Alessandro Dorna, Director of the Astronomical Observatory of Turin, took place on August 19 last, at the age of sixty-one years.

THE annual Congress of the Sanitary Institute of Great Britain commenced on Tuesday in York. Sir T. Spencer Wells, the President, commenced his inaugural address by expressing the hesitancy with which he accepted the position of President of the Congress, a hesitancy induced by the knowledge that he could not presume to appear before a body of sanitary experts as an instructor. Having referred to questions which had been dealt with in regard to sanitary science by his predecessors in the Presidential chair, he observed that it now remained to be considered how sanitary improvements might be carried still further by the co-operation of investigators, legislators, and administrators. As to the work of investigation, it had hitherto for the most part been personal, and the waste of labour had been enormous. The Institute must develop into something grander and more powerful. The Colleges of Physicians and Surgeons had done much, but it was rather for individual than collective good. Why should we not have a College of Health? The President then reviewed the work which those whom he called the "advanced guard of sanitary science" had accomplished, in lessening the death-rates of our population, and in benefiting the public health by prolonging life. Much of this he attributed to the coincident progress made in the science and art of medicine and surgery. He claimed for the medical profession a considerable share in the gain to the State of increasing numbers of more healthy subjects. We could not be far wrong if we put the average duration of human life in Great Britain half a century ago at about thirty years; now, according to the healthy life table, it was forty-nine years. Formerly it was calculated that a twenty-third part of the population was constantly sick, and the products of all that labour for the time necessarily withdrawn. A great deal of this sickness had been altogether prevented, and the duration of that which comes in spite of sanitation was lessened. He then dealt with the progress which had been made, since the Sanitary Institute had come into existence, in the moral and physical condition of our population. Dealing then with the various subjects to which the Institute had given attention, he divided them into five groups: (1) those relating to the training and health of the population; (2) to their social comfort and well-being; (3) to the prevention of disease; (4) to the care of the sick; and, lastly, those relating to the disposal of human refuse and remains. As to teaching the public on sanitary matters, it could never be done without elaborate organisation and legislative authority.

ONE of the tasks undertaken by the authorities of the British Museum since printing has taken the place of handwriting in the Catalogue is the publication of certain important sections of the Catalogue in separate parts. Thus the entries under America, Cicero, Luther, London, and many others have already appeared. The last of these is one of special scientific interest: it is a reprint of that part of the Catalogue which is classified under the head Academies. The definition of academies for the purpose is "Learned and Scientific Societies." The entries fill five parts, making a thick folio volume of about one thousand pages. In the great written Catalogue, which is well known to all readers, twenty-eight volumes were given to this one subject. The headings have been thoroughly revised throughout, and the names of a number of societies have been expunged, to be placed under more appropriate headings. Thus, agricultural societies, schools, political clubs, &c., which had crept into the Catalogue by degrees in course of time, have all been omitted. As it is, the total number of entries is about 32,000. "London" is the longest sub-heading; it fills nearly 200 pages, with about 6500 entries. Paris, St. Petersburg, and Berlin have about 3000 entries each; Vienna and Amsterdam about 1000. Towns are used for sub-headings, and under these are arranged alphabetically the names of the societies issuing the publications. The old sub-headings of countries have been abolished. Formerly the sub-headings would read thus:—

"Academies, &c.—Great Britain and Ireland,—London, Royal Society." The towns are now arranged alphabetically, regardless of countries. Only completed series are fully entered; works in progress are, according to the rule of the Museum, catalogued with the date of the first volume, and the words "in progress." The work covers the greater part of the scientific literature of the world; when the Catalogue of "periodical publications" is finished, there will be little relating to science which cannot be found under appropriate heads in one or the other. It seems like looking the gift-horse in the mouth, but we cannot refrain from observing that the value of these five volumes would be enormously increased if some approximation to a subject index could be added to them. It would be a simple task to have headings Chemistry, Microscopy, Geology, &c., under which were given the names of the towns where societies on these subjects are to be found. The student would then have before him at a glance the names of all the societies on the globe working at any particular subject. Instances will present themselves to every student in which the first name of a society, and that by which it has to be sought in the Catalogue, does not always indicate the sphere of work of the society. The price of the Catalogue unbound is, it should be added, a sovereign.

THE small launch *Volta*, which is propelled by the electric current, in a method invented by Messrs. Stephens and Co., of Millwall, left Dover on Monday morning last week on her voyage across the Channel. The hull of the *Volta* is 37 feet long and nearly 7 feet beam, built of galvanised steel plates. She has a very light appearance in the water. Her bow is about 2 feet above the water-line, and from this point down towards the stern she gradually reduces the depth of her gunwale. Her deck is nearly or quite on a level with the water. Below the deck, which is securely fastened down, are placed the electric accumulators, all coupled together with the coils. They are little square boxes about 6 by 12 inches, and are wedged in closely together so as to prevent shifting, and to fill the whole of the space below the deck. The propelling power consists of sixty-one accumulators, and a pair of Reckenzaun electromotors, also placed beneath the floor, so that the whole of the boat is available for passenger accommodation. The motive-power is under complete control, and the speed can be regulated to whatever rate is required. The speed of the launch is regulated by a main switch, and there are special switches for going astern, the whole of the apparatus being easily managed by one man. The power of the motors may be varied at will from 4 horse-power to 12 horse-power, whilst the screw-propeller, which is coupled direct to the motor-shaft, makes from 600 to 1000 revolutions per minute, according to the position of the switch handle. The *Volta* returned to Dover shortly before 8 o'clock, having completed a voyage which is regarded as a great scientific success. When the boat arrived at Calais it was found that the amount of electricity remaining in the accumulators warranted the return journey being attempted. When the voyage was completed, the current from the accumulators was still powerful, notwithstanding that during the last half-hour of the journey the launch had been driven at the rate of 14 miles an hour, and rushed through the water at such a rate as nearly to throw it over her bow. The total distance traversed was about 50 miles. During the voyage the speed was varied at will by means of the switch. The experiment is regarded by all those on board as a success far in advance of anything they expected. An incident occurred on the passage which illustrates the noiselessness of the little vessel. About mid-Channel the pilot observed a sea-gull floating asleep on the water. The boat was steered close to the bird, which was caught by the neck by one of those on board, and brought alive to Dover.

INTELLIGENCE has been received from Lieut. Schwatka, who was sent to Alaska in command of an exploring Expedition by the proprietors of the *New York Times*. On the way to Mount St. Elias, which dominates the range to which the same name has been given, the party crossed a river, the existence of which had been hitherto unknown. At a distance of eight miles from the mouth it is a mile in width, and its current flows at the rate of ten miles an hour. This is thought to be the largest river that enters the Pacific Ocean, and the glacial mud it brings down with it discolours the waters of Icy Bay for some miles out to sea. The river has been named Jones River, after Mr. George Jones, of New York, one of the promoters of the Expedition. To the east the explores saw a glacier twenty miles wide, which extended for fifty miles along the base of the St. Elias Alps. Assuming the land beneath it to be flat, the thickness of this glacier is about 1000 feet. It was named after Prof. Agassiz. Another glacier, to the westward, was named after Prof. Guyot. After three days' marching, Lieut. Schwatka and his party came upon a third glacier, which they named in honour of Prof. Tyndall. From this point they resolved to make a final dash as far as they could go into the heart of this grand but desolate icy region. At the end of twenty hours' labour they came in sight of the south side of the great mountain to which belongs the icy girdle along which they had been travelling. They saw before them glaciers rising, sometimes perpendicularly, to heights varying from 300 to 3000 feet. The Tyndall glacier, comparatively safe so far, was safe no longer. Enormous crevasses, some as much as 30 feet across, now became frequent; and the bands of ice between them were so narrow, that, in places, the explorers appeared to themselves to be walking on a bridge like that of a house roof, with a chasm hundreds of feet deep on each side. These and other difficulties, such as are familiar to Alpine climbers, had been surmounted until a height of 7200 feet above the level of the sea had been attained. As nearly the entire journey was above the snow-level, this ranks among the best climbs on record. The Lieutenant telegraphs that he hopes, by renewing his attempts upon the mountain on its northern and eastern sides, to make further contributions to geographical science, and perhaps to ascend the mountain to a greater height; but the probability is that Mount St. Elias will long remain an unscaled peak. Mr. Seton Karr states that the whole region is vastly superior to any other mountainous district with which he is acquainted. One incident of the journey was the discovery of three peaks, ranging from 8000 feet to 12,000 feet in height, which were severally named after President Cleveland, Mr. Secretary Whitney, and Capt. Nicholls.

LAST autumn a few science classes were started as a preliminary experiment in rooms belonging to the Royal Victoria Hall, at the nominal fee of 1*s*. on first entrance and 1*s*. 6*d*. per class for the session. The success achieved has encouraged the promoters to extend the scheme, and this year it is intended to hold classes in mathematics, chemistry, animal physiology, drawing, arithmetic, geometry, electricity, political economy, and English literature. Some of these classes will be in connection with South Kensington. Last year they were welcomed in the most enthusiastic way by the comparatively small number who knew of their existence; no pains were taken to advertise them, as it seemed likely the numbers would exceed the available accommodation. This has now been improved, and it is hoped that a very useful branch has been added to the work at the Hall. The entertainments, concerts, and lectures which go on in the large hall are in no way interfered with thereby; in fact the lectures gain by the existence of systematic instruction to which they lead up. Those at present announced are: October 5, Mr. W. L. Carpenter, on "What may be done with a New Lantern"; October 12, Dr. W. D. Halliburton, on "The Germs of Disease"; October 19, Prof. Judd, on "A Piece of Pumice-Stone."

THE Canadian salmon on view in the Canadian Section of the Colonial and Indian Exhibition which were hatched out last April in the building are thriving well.

THE Indian fish lately imported into the Aquarium of the Colonial and Indian Exhibition from Calcutta seem thoroughly at home in their artificial existence. There are two species on view, viz., the *Sacchobranchus fossilis*, or scorpion fish, and the *Ophiocephalus striatus*, or walking fish. A large consignment of German carp has just arrived, together with some Chinese goldfish and specimens of *Siluris glanis*.

THE first field-meeting of the County of Middlesex Natural History and Science Society was held on Saturday, the 18th inst., at Hampstead and Highgate. Between 60 and 70 members assembled at Hampstead Heath Station at 2.30 p.m., and were conducted thence by the Rev. F. A. Walker, D.D., and Mr. Clement Reid, F.G.S. Passing along the top of the Vale of Health to "Jack Straw's Castle," the party proceeded along the northern side of the Heath and reached "The Spaniards" about 4.30. At different points on the route the geology and physical features of the district were explained by Mr. Reid. At "The Spaniards" the party were unexpectedly met by Mr. Goodwin, of Highgate, who had obtained the kind permission of Lord Mansfield for the members to walk through the Park. Mr. Goodwin conducted the microscopical section to the ponds, where they were richly rewarded. Leaving the Park, the party proceeded to Highgate Schools, where they were received by Dr. McDonall, the head master, entertained at tea by the honorary secretary, Mr. Klein, and they afterwards inspected the collections of insects, mostly collected and arranged by Dr. Walker. At 7 o'clock the members assembled in the theatre of the Highgate Institute, where, after some short notes by Mr. Lloyd, the honorary secretary of the Institute, on "Highgate and Highgate Worthies," Mr. Mattieu Williams read a short paper upon "Some Peculiarities of London Atmosphere," which was followed by a discussion, in which Messrs. W. L. Carpenter, R. Hammond, and others took part. The Rev. Dr. Walker then made some interesting remarks on the different orders of insects represented in the collections of the Highgate Schools.

Science reports that Captain Dutton, of the United States Geological Survey, has recently been engaged in studying Crater Lake in Oregon, which he has found to be probably the deepest body of fresh water in the country. Boats were transported over a hundred miles of mountain road from Ashland, and had to be lowered 900 feet to the water. The steepness of the wall of the lake was very great. The depths ranged from 853 to 1996 feet, the average being about 1490 feet. The descent to the lake is partly over talus, covered with snow above, and rocky broken ledges lower down.

THE works for deepening the Seine to a depth of 3 metres have been finished. The river can now be navigated by vessels of about 1000 tons burthen, which are supplied with movable masts and chimneys for the bridges.

WE have received the report of the Otago Acclimatisation Society for the past year. The operations of the Society have been almost entirely confined to pisciculture, and apparently must be so for some years owing to the spread of poisoned grain over the country, and to the increase of the natural enemies of the rabbit, which are also the natural enemies of birds. But in pisciculture much has been done, and much more remains to be done, for the salmon and the herring are not yet numbered amongst New Zealand fishes. An experiment, which has so far been successful, for the introduction of the *Salmo salar* has been made; similarly in the cases of Loch Leven trout, *Salmo fontinalis*, and brown trout. Brown trout have been acclimatised with such success, that the Society is in a position to supply an almost unlimited demand for ova, as well as to provide liberally for the requirements of New Zealand streams. The late secretary, Mr. Arthur, had begun the collection of a series of data from which he hoped to gain some information respecting the sea-fish of New Zealand, and ultimately to arrive at something definite in regard to the nature and habits of some of the most important of them. The collection and tabulation of these returns and the important investigations of Mr. Arthur are being continued by Mr. Thomson. It is perhaps scarcely necessary to add that Sir James Maitland has seconded the efforts of the Society in acclimatising *Salmonidæ* in every possible way. The only wonder is that important public work of this nature should be left wholly to private endeavour.

A "transportable electric lighthouse" has been lately invented by M. Beduwe, a builder in Liége. The idea is, to furnish the light in any place on short notice; and it is thought the apparatus might prove useful in public works, cases of accident, gatherings in public places, *fêtes*, &c. The constituent parts are (1) a telescopic system of copper tubes bearing the light; (2) a three-cylinder steam-engine to drive either a Gramme machine, or a suction and force pump; (3) a vertical boiler on the tubular system; and (4) a reservoir for water. The whole is mounted on a four-wheeled carriage. The light is raised by hydraulic force. Further details may be found in *Le Génie Civil* of September 4.

THE first number of a monthly scientific journal made its appearance at Rio de Janeiro on July 25 in connection with the Philotechnic Institute of that city. It bears the title of *Revista Philotechnica*, and takes the place of the recently defunct *Revista Polytechnica*. Its object will be the practical and experimental study of the sciences, and of their application especially to the development of the arts and industries in Brazil. The first number contains papers on practical astronomy, by F. Behring; on building materials, by F. de Sá; and on practical chemistry, by Ad. Uchõa, chief editor.

A NEW YORK telegram states that several detonations and tremors occurred at Summerville between Saturday night and Monday night, and three shocks of earthquake, two of which were accompanied by detonations, were also felt there early on Tuesday morning. At Charleston three shocks of earthquake occurred on Monday night, one of which, at about daybreak, shook the houses to such an extent that many of the occupants ran terrified into the streets. One of the shocks was accompanied by detonations.

THE additions to the Zoological Society's Gardens during the past week include a Malbrouck Monkey (*Cercopithecus cynosurus* ♀) from West Africa, presented by the Rev. H. R. Moolenaar; a Black-backed Jackal (*Canis mesomelas*) from West Africa, an Algerian Tortoise (*Testudo mauritanica*) from North Africa, presented by Mr. A. T. Marsh; two Elegant Galidias (*Galidia elegans*) from Madagascar, presented by Mr. Burt. C. Muller; two Black Rats (*Mus rattus*) from Sark, Channel Islands, presented by Mr. W. F. Collings; a Bateleur Eagle (*Helotarsus ecaudatus*) from Lamoo, East Africa, presented by Dr. W. Somerville; a Wild Duck (*Anas boschas*), European, presented by Mr. K. Lawson; a Barn Owl (*Strix flammea*), European, presented by Mrs. E. Holloway; a Common Marmoset (*Hapale jacchus*), two Black-eared Marmosets (*Hapale penicillata*) from South-East Brazil, a Common Otter (*Lutra vulgaris*), British, two Ariel Toucans (*Ramphastos ariel*) from Brazil, deposited; a Common Crowned Pigeon (*Goura coronata*), two Auriculated Doves (*Zenaida auriculata*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

Photographic Determinations of Stellar Positions.—Dr. B. A. Gould, in a paper presented at the Buffalo meeting of the American Association for the Advancement of Science on August 20, 1886, gives some interesting particulars with regard to his photographic work at Cordova. He states that no northern stars were photographed there except the Pleiades and the Præsepe. On the Pleiades plates all but one of Bessel's stars are found, which fall within the limits of the field; the missing one being of the magnitude 9½, whilst there are depicted on the plates other stars of the magnitudes 10, 10½, and 11. About seventy southern clusters have been repeatedly photographed at Cordova, also more than a hundred double stars, whilst the total number of photographs which Dr. Gould has on hand for measurement is about 1300, only a few having been preserved in which the images are not circular. In addition to these classes of objects, special attention was given for many years to taking frequent impressions, at the proper seasons, of four stars selected, on account of their large proper motions, as likely to manifest appreciable annual parallax. All but one of these four stars—β Hydri—have been included in the lists observed and discussed by Drs. Gill and Elkin at the Cape. Still, it will be a matter of much interest to apply the photographic method of investigation to the same problem, even if for no other purpose than a comparison of the results of the two methods. With regard to the progress made in the measurement of the Cordova photographs, Dr. Gould states that the measurements thus far completed are those of the double stars, the four stars with large proper motion, the Pleiades, the Præsepe, and the clusters Lacaille 4375 and κ Crucis. The corresponding computations have been made as yet only for a portion of the Pleiades plates, but it is expected that all these will be completed at a comparatively early date. The results deduced from the Pleiades photographs will be looked for with much interest, especially as Dr. Elkin has recently executed at Yale College a heliometric triangulation of the principal stars of the group, and the comparison of the results will be a severe test of the photographic method for the determination of stellar positions. But astronomers expect good work from Dr. Gould, and they are not likely to be disappointed. Dr. Gould's paper is published in the *Scientific American Supplement*, No. 556.

Gore's Nova Orionis.—Rev. T. E. Espin announces in *Circular* No. 9 of the Liverpool Astronomical Society that, observing on the night of September 14, he found the *Nova* to have a magnitude of 9·2. The star, he says, appeared very red. The small comes *f* was estimated as of 9·7 magnitude.

Heliometric Observations of the Pleiades.—We learn from *Science*, vol. viii. No. 187, that at the recent meeting of the American Association Dr. Elkin communicated a paper upon a comparison of the places of the Pleiades as determined by the Königsberg and Yale College heliometers. The results given were provisional, but they show unquestioned change of position with reference to η Tauri since 1860. Most of the brighter stars of the group, as shown by Newcomb in his "Catalogue of Standard Stars," go with η Tauri, but among the smaller stars there are unquestioned departures from this community of proper motion.

Gould's "Astronomical Journal."—Our readers will be glad to learn that there is a prospect of the publication of this valuable periodical being resumed. The American Association at the recent meeting passed a unanimous resolution congratulating Dr. Gould on the proposed revival of the *Journal*, and expressing its best wishes for his success.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 SEPTEMBER 26—OCTOBER 2

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on September 26

Sun rises, 5h. 54m.; souths, 11h. 51m. 17·4s.; sets, 17h. 48m.; decl. on meridian, 1° 19′ S.: Sidereal Time at Sunset, 18h. 10m.

Moon (New on September 27) rises, 3h. 42m.; souths, 10h. 37m.; sets, 17h. 19m.; decl. on meridian, 7° 12′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	5 42	11 49	17 56	0 34 N.
Venus	4 12	10 50	17 28	6 44 N.
Mars	10 45	15 3	19 21	19 32 S.
Jupiter	6 49	12 30	18 12	4 18 S.
Saturn	23 8*	7 11	15 14	21 28 N.

* Indicates that the rising is that of the preceding evening.

Sept.	h.	
26	17	Venus in conjunction with and 0° 34′ north of the Moon.
28	3	Mercury in superior conjunction with the Sun.

Variable Stars

Star	R.A. h. m.	Decl. ° ′		h m	
Algol	3 0·8	40 31 N.	Sept. 29,	3 43	*m*
			Oct. 2,	0 31	*m*
λ Tauri	3 54·4	12 10 N.	,, 2,	5 16	*m*
ζ Geminorum	6 57·4	20 44 N.	Sept. 29,	2 48	*m*
T Geminorum	7 42·5	24 1 N.	Oct. 1,		*M*
δ Libræ	14 54·9	8 4 S.	Sept. 28,	1 45	*m*
U Coronæ	15 13·6	32 4 N.	,, 28,	23 27	*m*
U Ophiuchi	17 10·8	1 20 N.	,, 28,	1 20	*m*
				21 28	*m*
W Sagittarii	17 57·8	29 35 S.	,, 28,	0 0	*M*
U Sagittarii	18 25·2	19 12 S.	,, 28,	6 0	*m*
			Oct. 1,	6 0	*M*
R Lyræ	18 51·9	43 48 N.	Sept. 28,		*m*
S Vulpeculæ	19 43·7	27 0 N.	,, 29,		*m*
δ Cephei	22 24·9	57 50 N.	Oct. 2,		*m*

M signifies maximum; *m* minimum.

Meteor Showers

The *Aurigids*, R.A. 85°, Decl. 50° N., the *Aquarids*, R.A. 33°, Decl. 2° S., and meteors from the following radiants have been observed at this time:—From Musca, R.A. 46°, Decl. 26° N.; near ι Aurigæ, R.A. 70°, Decl. 32° N.; and near α Cephei, R.A. 315°, Decl. 62° N.

Stars with Remarkable Spectra

Name of Star	R.A. 1886·0 h. m. s.	Decl. 1886·0 ° ′	Type of spectrum
T Arietis	2 41 57	17 1·9 N.	III
D.M. + 8° 443	2 47 38	8 52·1 N.	III.
ρ Arietis	2 49 23	17 52·1 N.	III.
α Ceti	2 56 18	3 38·5 N.	III.
ρ Persei	2 57 50	38 23·9 N.	III.
D.M. + 57° 702	3 2 40	57 28·2 N.	IV.

THE BRITISH ASSOCIATION

SECTION G

MECHANICAL SCIENCE

Opening Address by Sir James N. Douglass, M.Inst.C.E., President of the Section

. . . I propose to address you on a subject with which I have been practically connected for nearly half a century, that is, the development of lighthouses, light-vessels, buoys, and beacons, together with their mechanical and optical apparatus. . . .

During the last century a very considerable increase has occurred in the number of lighthouses and light-vessels on the various coasts of the world, which have been required to meet the rapid growth of commerce. Only during the last twenty-five years can accurate statistical information be obtained, and it is found that in the year 1860 the total number of coast lights throughout the world did not exceed 1800, whereas the present number is not much less than 4000. . . .

Concurrently with the enormous increase in the number of coast lights during the last fifty years, very great improvements have been effected from time to time in their efficiency. In 1759 Smeaton's lighthouse on the Eddystone was illuminated by 24 tallow candles, weighing ⅖ lb. each. The intensity of the light of each candle, I find, from experiments made with similar candles prepared for the purpose, to have been about 2·8 candle units each; thus the aggregate intensity of radiant light from the 24 candles was only about 67 candle units. No optical apparatus, moreover, was used for condensing the radiant light of the candles, and directing it to the surface of the sea. The con-

sumption of tallow was about 3·4 lbs. per hour; therefore, the cost of the light per hour, at the current price of tallow candles, would be about 1*s.* 6¾*d.*, sufficient to provide a mineral oil light, at the focus of a modern optical apparatus, to produce for the service of the mariner a beam of about 2400 times the above-mentioned intensity.

The introduction of catoptric apparatus for lighthouse illumination appears to have been first made at Liverpool, about 1763, and was the suggestion of William Hutchinson, a master mariner of that port. The invention by Argand, in 1782, of the cylindrical wick lamp, provided a more efficient focal luminary than the flat wick lamp previously employed, and was soon generally adopted, for both fixed and revolving lights. In 1825 the French lighthouse authorities effected another very important improvement in lighthouse illumination by the introduction of the dioptric system of Fresnel in conjunction with the improvements of Arago and Fresnel on the Argand lamp, by the addition of a second, third, and fourth concentric wick.

Coal and wood fires, followed by tallow candles and oil, have been referred to as the early lighthouse illuminants. In 1827 coal gas was introduced at the Troon Lighthouse, Ayrshire, and in 1847 at the Hartlepool Lighthouse, Durham, the latter for the first time in combination with a first-order Fresnel apparatus. The slow progress made with coal gas in lighthouses, except for small harbour lights, where the gas could be obtained in their vicinity, was chiefly due to the great cost incurred in the manufacture of so small a quantity as that required and at an isolated station. In 1839 experiments were made at the Orford Low Lighthouse, Suffolk, with the Bude light of the late Mr. Goldsworthy Gurney. This light was produced by throwing oxygen gas into the middle of a flame derived from the combustion of fatty oils. The flame was of the dimensions of that of the Fresnel four-wick concentric burner. An increased intensity over that of the flame of the large oil burner was obtained, but it was not found to be sufficient to justify the increased cost incurred. In 1857 a trial was made by the Trinity House, at Blackwall, under the advice of Faraday, with one of Holmes's direct current magneto-electric machines for producing the electric arc light for a lighthouse luminary, and the experiment was found to be so full of promise for the future that a practical trial was made during the following year.

At the South Foreland High Lighthouse, on December 8, 1858, the first important application of the electric arc light, as a rival to oil and gas for coast lighting, was made with a pair of Holmes's machines, and thus were steel magnets made to serve not only, as in the mariner's compass, to guide him on his path, but also to warn him of danger. In 1859 the experimental trials at the South Foreland were discontinued, but they were sufficiently encouraging to lead to the permanent installation of the electric light at Dungeness Lighthouse in 1862. In 1863 the electric arc light was adopted by the French lighthouse authorities at Cape La Hève.

In 1871, after practical trials with a new alternating current machine of Holmes, two of such machines were supplied to a new lighthouse on Souter Point, coast of Durham, and in the following year the electric arc light, with these machines, was established in both the High and Low Lighthouses at the South Foreland, where it still shines successfully. The early experience with the electric light at Dungeness was far from encouraging. Frequent extinctions of the light occurred from various causes connected with the machinery and apparatus, and the oil light had, at such times, to be substituted. As no advantage can counterbalance the want of certainty in signals for the guidance of the mariner, no further step in the development of the electric light was taken by the Trinity House until the latter part of 1866, when favourable reports were received from the French lighthouse authorities of the working of the Alliance Company's system at the two lighthouses of Cape La Hève. Complaints were also received from mariners, in the locality of Dungeness, of the dazzling effect on the eyes when navigating, as they are there frequently required to do, close inshore, thus being prevented from rightly judging their distance from this low and dangerous point. Therefore, in 1874, the electric light was removed from Dungeness, and a powerful oil light substituted. In 1877 the electric arc light was installed at the Lizard Lighthouses on the south coast of Cornwall, and arrangements are now being made for establishing it at St. Catherine's Lighthouse, Isle of Wight, and at the High Tower, on the Isle of May, Firth of Forth. I have mentioned that the first machines of Holmes at the South Foreland were direct current, the machines provided by him for Dungeness being also of the same type. The French lighthouse authorities, however, adopted for their lighthouses at Cape La Hève the "Alliance" alternating current magneto-electric machines, and, in consequence of the less wear and tear of these machines with greater reliability through their having no commutator, Holmes was required to supply alternating current machines for Souter Point and the South Foreland. Those machines have been running at these stations fourteen years and fifteen years respectively. They have during this period required only a very trifling amount of repair, and are still in excellent order, but the time must soon arrive for replacing them by more powerful machines.

In 1876 a series of trials was made by the Trinity House at the South Foreland, with various dynamo-electric machines, for the purpose of ascertaining the then most suitable machine for adoption at the Lizard. The results were decidedly in favour of the Siemens direct current machine, and machines of this type were accordingly installed at the Lizard Station in 1878. In consequence of irregularities in their working, and because, at the time, Baron de Méritens, of Paris, had perfected a very powerful alternating current machine, it was resolved to send one of the latter machines to the Lizard for trial, where it has worked most satisfactorily for several years. The experience gained at the Lizard suggested that, for the St. Catherine's Station, where it had been resolved to adopt the electric arc light, the De Méritens machines should be employed, and they were accordingly ordered; but, as arrangements were then being made for experiments at the South Foreland for testing the relative merits of electricity, gas, and oil as lighthouse illuminants, it was determined that these machines should first be sent there for the experiments. In 1862 a practical trial was made by the Trinity House at the South Foreland of the Drummond or lime light, but the results were not so satisfactory, after experience with the electric arc light, as to encourage its adoption. In the meantime the successful development of the electric arc light for lighthouse illumination very soon acted as a keen stimulus to inventors of burners for producing gas and oil luminaries for the purpose; in 1865 the attention of lighthouse authorities was directed to the gas system of Mr. John R. Wigham, of Dublin, which system was tried in that year by the Commissioners of Irish Lights at the Howth Bailey Lighthouse, near Dublin, and in 1878 he introduced at the Galley Head Lighthouse, county Cork, his system of superposed gas burners. At this lighthouse four of his large gas burners and four tiers of first-order annular lenses, eight in each tier, were adopted. By successive lowering and raising of the gas flame at the focus of each tier of lenses, he had previously produced the first group flashing distinction. This light shows, at periods of one minute, from ordinary annular lenses, instead of the usual long flash, a group of short flashes, varying in number between six and seven. The uncertainty, however, in the number of flashes contained in each group is found to be an objection to the optical arrangement here adopted. In the meantime the attention of the Trinity House, the Commissioners of Northern Lights, and the French lighthouse authorities was being directed to the question of substituting mineral oil for colza as a lighthouse illuminant. In 1861 experiments were made by the Trinity House for the purpose of determining the efficiency and economy of mineral oils in relation to colza for lighthouse illumination; but, owing to the imperfectly-refined oil then obtainable and its high price, the results were not found to be quite so satisfactory as to justify a change from colza oil, at that time generally used. In 1869 the price of mineral oil, of good illuminating quality and safe flashing-point, having been reduced to about one-half the price of colza, the Trinity House determined to make a further series of experiments, when it was ascertained that, with a few simple modifications, the existing burners were rendered very efficient for the purpose, and a change from colza to mineral oil was commenced. It was found, during these experiments, that the improved combustion effected in the colza burners, in their adaptation for consuming mineral oils, had the effect of increasing their mean efficiency, when burning colza, 45¾ per cent. A further advance was made during these experiments by increasing the number of wicks of the first-order burner from four to six, more than doubling the intensity of the light, while effecting an improved compactness of the luminary per unit of focal area of 70 per cent.

With coal fires no distinctive characters were possible beyond the costly ones of double or triple lighthouses. There are at present not less than 86 distinctive characters in use throughout

the lighthouses and light-vessels of the world; and, as their numbers increase, so does the necessity for giving a more clearly distinctive character to each light over certain definite ranges of coast. This important question of affording to each light complete distinctive individuality is receiving the attention of lighthouse authorities at home and abroad, and it is hoped that greater uniformity and consequent benefit to the mariner will be the result.

During the old days of sailing-vessels, when the duration of voyages was so uncertain, sound-signals, as aids to the mariner, were but little demanded. The seaman on approaching the coast in fog trusted entirely to his lead, and, when he found circumstances favourable for doing so, he anchored his vessel until the atmosphere cleared. But, since the application of steam to navigation, with keener competition in trade, these conditions have been entirely changed. The modern steam-vessel is expected to keep time with nearly the same degree of precision as a railway train, and it is evident that, even with the utmost care and attention on the part of her commander, this requirement cannot possibly be fulfilled, and collisions and strandings must occur, unless efficient sound-signals for fog be carried by each vessel, and powerful signals of this class be provided at lighthouse and light-vessel stations.

These circumstances have led to a rapid development of fog-signals, both ashore and afloat, there being now about 700 of these signals, of various descriptions, on the coasts of the world. We therefore find, as might have been naturally expected, that coast fog-signals have been made, by lighthouse authorities, the subject of careful experiment and scientific research; but, unfortunately, the practical results thus far have not been so satisfactory as could be desired, owing (1) to the very short range of the most powerful of these signals under occasional unfavourable conditions of the atmosphere during fog; and (2) to the present want of a reliable test for enabling the mariner to determine at any time how far the atmospheric conditions are against him in listening for the anxiously expected signal. In 1854 some experiments on different means of producing sounds for coast fog-signals were made by the engineers of the French lighthouse department, and in 1861-62 MM. Le Gros and Saint-Ange Allard, of the Corps des Ponts et Chaussées, conducted a series of experiments upon the sound of bells and the various methods of striking them.

In 1863-64 a Committee of the Elder Brethren of the Trinity House made some experiments at Dungeness upon various fog-signals. In June 1863 a Committee of the British Association memorialised the then President of the Board of Trade, with the view of inducing him to institute a series of experiments upon fog-signals. The memorial, after briefly setting forth a statement of the nature and importance of the subject, described what was then known respecting it, and several suggestions were made as to the nature of the experiments recommended. The proposal does not appear to have been favourably entertained by the authorities to whom it was referred, and the experiments were not carried out.

In 1864 a series of experiments was undertaken by a Commission appointed by the Lighthouse Board of the United States, to determine the relative powers of various fog-signals which were brought to the notice of the Board.

In 1872 a Committee of the Trinity House visited the United States and Canada, with the object of ascertaining the actual efficiency of various fog-signals then in operation on the North American continent, about which very favourable reports had reached this country. Among other instruments, they witnessed the performance of a Siren apparatus, patented by Messrs. A. and F. Brown, of New York. One of these instruments was, in 1873, very kindly sent to the Trinity House by the United States authorities, and tested with other instruments in the experimental trials at the South Foreland in 1873-74. This investigation was carried out at the South Foreland by the Trinity House, with the object of obtaining some definite knowledge as to the relative merits of different sound-producing instruments, and also of ascertaining how the propagation of sound was affected by meteorological phenomena. These experiments were extended over a lengthened period, in all conditions of weather; and the well-known scientific and practical results obtained, together with the ascertained relative merits of sound-producing instruments for the service of the mariner, are of the highest scientific interest and practical importance.

The investigation at the South Foreland was followed up by the Trinity House by further experiments, in which they were assisted by the authorities at Woolwich, with guns of various forms, weight of charges, and descriptions of gunpowder. The powders tested were (1) fine grain, (2) larger grain, (3) rifle large grain, and (4) pebble. The result placed the powders exactly in the order above stated; the fine grain, or most rapidly burning powder, gave indisputably the loudest sound, while the report of the slowly-burning pebble powder was the weakest of them all. Experiments were also made with the object of ascertaining the relative value of the sound produced by the explosion of varying quantities of gun-cotton. Here again the greater value of increased rapidity of combustion in producing sound was clearly demonstrated. It was found that charges of gun-cotton yielded reports louder at all ranges than equal charges of gunpowder, and further experiments proved that the explosion of half a pound of gun-cotton gave a result at least equal to that produced by 3 lb. of the best gunpowder. These results led the Trinity House to adopt this explosive as a fog-signal for isolated stations on rocks or shoals where previously, from want of space, nothing better than a bell could be applied. It is also applied with success to light-vessels. But, wherever the Siren can be installed, it is found to be the most efficient fog-signal yet known, chiefly in consequence of the prolongation that can be given to its blasts, and the ease with which it can be applied, with any amount of motive-power available, to the production of any desired combination of high and low notes for distinctions corresponding with those of white and red, or short and long, flashes of light, and thus affording the required individuality of each station. The experience, however, with the most powerful fog-signal is not at present to be considered altogether satisfactory. With Siren blasts absorbing about 150 H.P., or nearly 5,000,000 foot-pounds, per minute during the time they are sounding, the signal is occasionally not heard, under some conditions of fog and wind, beyond 1 mile, while at other times it is distinctly heard above 10 miles. . . .

In 1881 it was considered by the lighthouse authorities of this country that the time had arrived when it was absolutely necessary that an exhaustive series of experimental trials should be made, on a practical scale, for the exact determination of the relative merits (both as regards efficiency and economy) of the three lighthouse illuminants, electricity, gas, and mineral oil, which, by the process of natural selection, may be regarded as the fittest of all those at present known to science. After many unforeseen difficulties had been overcome, this question of universal importance was, in July 1883, referred by the Board of Trade to the Trinity House, who accepted the responsibility of carrying out the investigation.

A Committee was formed of members of the Corporation, who secured the friendly co-operation of the Scotch and Irish Lighthouse Boards, and many distinguished scientific men at home and abroad. I had the honour of acting, in my official capacity as Engineer-in-Chief to the Trinity House, in making the arrangements for exhibiting the experimental lights, and in reporting to the Board from time to time, as in all other matters referred to me professionally.

These investigations were carried out in full view of all who were in any way interested in the subject. The whole arrangements were open to public inspection, and, in their desire to arrive at a wise and just decision on so important a question, the Trinity House Committee courted the fullest inquiry. Many members of scientific Societies, especially those connected with engineering, were invited, and visited the station. The French lighthouse authorities, who rendered much kind assistance in obtaining observations, sent their representatives to view the arrangements, and officers from the lighthouse services of Germany, Denmark, Norway and Sweden, Russia, Italy, Spain, Brazil, the United States, and Canada visited the station and witnessed the experiments.

In order to obtain, with uniformity and method, a consensus of comparative eye-measurements—in addition to the measurements of the Committee and their officers at their different stations ashore and afloat, to those of the coastguard men at nine stations between Dungeness and the North Foreland, and to the more precise scientific measurements of the experts—special observation-books were prepared, and widely distributed to shipping associations and port authorities, with a view to their securing the co-operation of masters of vessels, pilots, and others navigating in the vicinity of the South Foreland.

The South Foreland Station is especially adapted for lighthouse experiments generally, because of the existing facilities for observations on land and sea. The land in the neighbour-

hood has no hedges and few trees, and affords facilities for observations at distances of between 2 and 3 miles. The station is provided with surplus steam power for driving experimental machines for electric lights, and it is easily accessible from London.

Three rough timber towers of sufficient strength to withstand, without tremor, the effects of heavy gales were erected at the rear of the High Lighthouse, 150 feet apart. These towers were marked in large letters, A, B, and C. A tower was devoted to electricity, B to the gas system of Mr. Wigham, and C to such gas or oil lamps as might be proposed to, and approved by, the Committee for trial during the experiments. A lantern of the usual first-order dimensions, but with an additional height in the glazing for the passage of beams from superposed optical apparatus of the first order, was provided for each tower. The optical apparatus in each lantern was, in the outset, special in relation to the illuminant to be used for producing fixed and flashing lights. For the electric arc lights, optical apparatus of the second order of Fresnel was adopted, the apparatus having a focal distance of 700 mm. The dimensions of this apparatus are greater than optically required for the largest electric arc light yet tried for lighthouse illumination, but the internal capacity is found to be only just sufficient for the perfect manipulation of the light by a light-keeper of possibly robust build. For the large gas and oil flames in the A and C lanterns the apparatus adopted was of the usual first-order size, having a focal distance of 920 mm.

The lanterns were partially glazed on opposite sides, north and south, the southern arc being chiefly for observation from the sea. To the northward the land is better adapted for observations on shore, and here three observing-huts were erected at the respective distances of 2144, 6200, and 12,973 feet; each hut was provided with accommodation for two watchers, and a chamber fitted with a large plate-glass window in the direction of the experimental lights, and special apparatus for their photometric measurement. The third hut proved to be practically of but little value for photometry, the distance being too great; it, however, afforded an accurately known distance for eye-measurements, and a barrack and starting-point for watchers endeavouring to determine the vanishing distance of each light during hazy weather. In this they were further assisted by white painted posts, placed throughout the whole track to the experimental lighthouses, at distances of 100 feet apart, the distance of each post from the lights being plainly marked on it in black figures. For the more exact examination and measurements of the intensity of each luminary and that of the beam from each optical apparatus, a photometric gallery was erected in a convenient position, 380 feet long by 8 feet wide, and provided with all the necessary appliances.

During a period of over twelve months the experimental lights were exhibited, and watched by numerous observers, trained and untrained, scientific and practical. During that period a vast amount of valuable evidence was collected, by the aid of which the Committee were subsequently enabled to state their conclusions with definiteness. During these investigations intensities were shown in a single oil and gas luminary about three times greater than the electric arc luminary first adopted at Dungeness in 1861, while, with a single electric arc luminary, there was shown a practically available focal intensity about fifteen times greater than that of the Dungeness luminary, and the highest yet shown to be practically available for the service of the mariner.

With gas and oil the highest intensity of a single luminary and optical apparatus was tripled by the use of three superposed luminaries and optical apparatus, and although optical arrangements were made for triple electric luminaries, and experiments were carried out with these at comparatively low intensities, it was soon found that all the electromotive force available at the station could be conveniently applied with efficiency and permanency in one compact focal luminary, and its optical apparatus. This fact demonstrated that the electric arc has the most important requisites of a lighthouse luminary; viz. maximum intensity and minimum focal dimensions, and in all states of the atmosphere, from clear weather to thick fog, an incontestable superiority over the utmost accumulative efforts of its rivals—gas and oil. It was therefore considered to be unnecessary to incur additional cost for exhibiting the electric arc light, under the same conditions of accumulative powers as its rivals, for showing a maximum intensity. With the best gas and oil luminaries it was found that, where gas of the ordinary commercial quality is employed, there is no appreciable difference, either in the intensity or focal compactness of the luminary, but when the richest gas, from cannel coal, and mineral oil are used, there is found to be a superiority in the maximum intensity of this luminary over oil of about 45 per cent., and in focal compactness of about 10 per cent.; but in haze and fog, when the maximum intensity only is required, this difference was found to effect no appreciable gain in penetrative power, therefore the question of merit between these illuminants was found to resolve itself into one of economy only, and in this respect mineral oil at the present market prices was found to have a considerable advantage.

The relative penetrability per unit of light of the best gas and oil flames in haze and fog is so nearly identical that the question is of no practical importance in lighthouse illumination. But, with regard to the relative atmospheric absorption of these lights and the electric arc light in certain impaired conditions of the atmosphere, the electric arc light is found to compare somewhat unfavourably. The general result of the photometric measurements of the three illuminants showed (1) that the oil and gas lights, when shown through similar lenses, were equally affected by atmospheric variation; (2) that the electric light is absorbed more largely by haze and fog than either the oil or the gas light; and (3) that all three are nearly equally affected by rain. Experiments made in the photometric gallery at the South Foreland with the electric arc light have shown that the loss by atmospheric absorption is by no means so great as was previously supposed. It would have been most interesting and instructive to have obtained data for exactly determining the relative coefficients of atmospheric absorption of the electric arc, gas, and oil luminaries, but the necessary observations and measurements for effecting this would have prolonged the time too much, and added too much to the cost of the investigation, especially when it is remembered that with the electric arc light there is for coast illumination such an enormous preponderance of initial intensity at disposal that a small percentage of penetrating efficiency is of no practical importance.

In 1836 Faraday showed by actual experiment that the penetrating power of a light in atmosphere impaired by such obstruction as fog, mist, &c., is but very slightly augmented by a very considerable increase in the intensity, and M. Allard, late Engineer-in-Chief to the French Lighthouse Board, has more recently shown after long experimental and practical research, that, in an atmosphere of average transparency, a beam of light equal to 6250 becs (Carcel) would penetrate 53 kilometres, yet when augmented to twenty times that intensity, or 125,000 becs (Carcel), it would only penetrate 75·40 kilometres; showing that, in the average condition of atmospheric transparency, 2000 per cent. of increased intensity only gives 42 per cent. longer range.

The South Foreland experiments have demonstrated that, while with both gas and oil an ordinary intensity of light can be adopted for clear weather sufficient to reach the sea horizon with efficiency for the mariner, a maximum light can be shown with impaired atmosphere fifteen to twenty times this intensity, and that in these respects both illuminants are practically on an equality. This maximum light of gas and oil is considered by the Committee to be sufficient for all the ordinary purposes of navigation, and, for this, mineral oil is the most economical illuminant; but for some special cases, where the utmost intensity and penetration are demanded, these results can only be attained by electricity, and by this agent an intensity more than ten times that of the maximum of either oil or gas is found to be practically available.

With regard to the gas and oil lights, the report of the Committee states that "It appears from the direct eye-observations, made at distances varying from 3 to 27 miles in clear weather, that through annular lenses, light for light, there is practically no difference. Both reach the horizon with equal effect. In weather not clear the records indicate practically the same relation. In actual fog, again, the records indicate a general equality of the lights. Both are lost at the same time, both are picked up together; and although here and there a very slight superiority is attributed to the gas, this superiority is of no value whatever for the purposes of the mariner." A point referred to in favour of gas is the well-known one of greater handiness and ease of manipulation than oil, which is of importance for small beacon lights, where a constant attendant is not provided; but this does not apply to a coast light, where a light-keeper is always required to be on the watch in the lantern from sunset to sunrise. With oil the great advantage, in addition to economy, lies in the simplicity of its application to a coast lighthouse in

any part of the world, however limited the space the lighthouse is necessarily required to occupy. The final conclusion of the Committee on the relative merits of electricity, gas, and oil as lighthouse illuminants is given in the following words:—"That, for ordinary necessities of lighthouse illumination, mineral oil is the most suitable and economical illuminant, and that for salient headlands, important landfalls, and places where a very powerful light is required, electricity offers the greatest advantages."

In conclusion it may safely be asserted, now that the relative merits of electricity, gas, and oil have been accurately determined, that these investigations of the Trinity House Committee will, for many years to come, furnish to the lighthouse authorities of all maritime nations of the world, and their engineers, very valuable data which cannot fail to assist very largely in the development of lighthouse illumination, and thus tend very materially to present aids to navigation, and to a consequent reduction in the loss of life and property at sea.

REPORTS

Third Report of the Committee, consisting of Prof. Balfour Stewart (Secretary), Mr. J. Knox Laughton, Mr. G. J. Symons, Mr. R. H. Scott, and Mr. Johnstone Stoney, appointed for the Purpose of co-operating with Mr. E. J. Lowe in his Project of establishing on a Permanent and Scientific Basis a Meteorological Observatory near Chepstow.—In answer to a letter written by Prof. Balfour Stewart, pointing out certain conditions indispensable to the success of the project, Mr. Lowe writes:—"The (local) Committee think that they see their way to getting two or three thousand pounds if the scheme were started. Since you were with me I have purchased nearly 150 acres of land in front of the observatory, and nothing could come between it and the channel as near as $1\frac{1}{2}$ to 2 miles. A new road is to be made to the Severn Tunnel Station, and I hear that the telegraph or telephone is likely to be carried up this road. If your Committee think well to recommend the observatory scheme, action would be at once taken, and we have reason to believe that the Bristol Docks would help us with 100*l.* a year. I should much like to see such an observatory in working order whilst I live, but my time is getting short. There is a growing interest round here about the observatory, and constant inquiries are made as to the probabilities of success." The Committee express their sympathy with Mr. Lowe and his friends under the unfortunate circumstances that have tended to retard local action. The Committee see such evidence of local interest in the undertaking that they desire to have an early opportunity of co-operating with the local Committee. They therefore ask for their re-appointment, and request that the unexpended sum of 25*l.* and an additional sum of the same amount—in all 50*l.*—be placed at their disposal for the purpo e.

A Report of the Committee consisting of Profs. Tilden and Ramsay and Dr. Nicol (Secretary), appointed for the Purpose of Investigating the Subject of Vapour-Pressures and Refractive Indices of Salt Solutions, was read by Dr. Nicol.—The report deals with the general conclusions arrived at from recent experiments on vapour-pressures, rates of expansion, refractive indices, and saturation of salt solutions. The experiments on the vapour-pressure of salt solutions completely disprove the statement of Wüllner, that the diminution of vapour-pressure is directly proportional to the percentage of salt present; in some cases it has been observed that the restraining effect of each molecule increased with the concentration, whilst with other salts it decreased on the addition of salt even in dilute solutions. Such results can, however, be readily explained by the theory of solution proposed by Nicol in the *Philosophical Magazine*, 1883.

The Report of the Committee consisting of Profs. Ramsay, Tilden, W. L. Goodwin (Secretary) and D. H. Marshall, appointed for the Purpose of Investigating Certain Physical Constants of Solutions, was read by Prof. Ramsay.—This report contained an account of an investigation conducted by Profs. Goodwin and Marshall of the Queen's Univerity, Kingston, Ontario, the object of which was the determination of the condition of equilibrium assumed by molecular weights of two salts placed in separate small vessels and inclosed with a weighed quantity of water. The process by which the water is so attracted to the alts was styled "invaporation" by Graham. The salts experimented with were the chlorides of potassium, lithium, and sodium. When sodium and potassium chlorides were used, and different quantities of water, it was found that sodium chloride invaporates the water more rapidly than potassium chloride, and that, with small relative quantities of water, the sodium chloride invaporates nearly all and leaves the potassium chloride almost dry. When this is compared with the state of equilibrium assumed by equivalents of caustic soda, caustic potash, and sulphuric acid in solution together, it seems that the force in the first case is different in character from that acting in the second. Similar experiments made with sodium and lithium chlorides, and varying the relative quantities of water, showed that with small relative quantities of water the lithium chloride attracted the whole, but with larger quantities the sodium chloride attracts part, showing that in this case there is a limit to the quantity of water which the lithium chloride can hold against the attraction of sodium chloride. When the relative quantity of water is small, it is not divided between the two salts in the ratio of their attraction for water; but this may be the case with large relative quantities of water. The process of invaporation is in all cases very slow, in some cases requiring several months for its completion. A further investigation of these phenomena with other salts, and a study of the influence of temperature is promised.

A Preliminary Report of the Committee consisting of Profs. McLeod and Ramsay, with Mr. W. A. Shenstone as Secretary, appointed for the Further Investigation of the Influence of the Silent Discharge of Electricity on Oxygen and other Gases, was read by Mr. Shenstone.—A description was given of the apparatus devised for the storage and convenient manipulation of oxygen, so as to insure its perfect purity. The use of a mixture in molecular proportions of potassium and sodium chlorates is recommended in the preparation of oxygen, inasmuch as the breakage of apparatus, when potassium chlorate alone is used, is to a great extent done away with.

The Report of the Committee consisting of Profs. W. A. Tilden and H. E. Armstrong, appointed for the Purpose of Investigating Isomeric Naphthalene Derivatives, of which Prof. H. E. Armstrong is the Secretary, was read by the latter, who pointed out that, owing to its constitution, naphthalene lends itself very easily to the production of isomeric compounds. The constitution of the disulphonic acids of naphthalene has been specially investigated, and four isomeric compounds were described, as were also several isomeric bromo-derivatives.

The Committee consisting of Prof. Sir H. E. Roscoe, Mr. Lockyer, Profs. Dewar, Liveing, Schuster, W. N. Hartley, and Wolcott Gibbs, Capt. Abney, and Dr. Marshall Watts, appointed for the Purpose of Preparing a New Series of Wave-Length Tables of the Spectra of the Elements, of which Dr. Marshall Watts is the Secretary, reported that satisfactory progress had been made during the past year with the work allotted to it, and that the forthcoming volume of the *Proceedings* of the Association will contain additions to the tables of wave-lengths of the emission spectra of the elements and compounds.

Report of a Committee, consisting of General J. T. Walker, General Sir J. H. Lefroy, Prof. Sir William Thomson, Mr. Francis Galton, Mr. Alex. Buchan, Mr. J. Y. Buchanan, Dr. John Murray, Mr. H. W. Bates, and Mr. E. G. Ravenstein (Secretary), appointed for the Purpose of taking into Consideration the Combination of the Ordnance and Admiralty Surveys, and the Production of a Bathy-hypsographical Map of the British Isles.—(1) The Committee consider that the production of a plain outline map of the British Isles and surrounding seas, on a scale of 1 : 200,000 (about three miles to the inch) would be desirable. Rivers, and such other physical features as can be shown in outline, to be marked distinctly. No hill-shading to be introduced. Roads, railways, towns, &c., to be indicated faintly, and merely for the purpose of identifying localities. Principal heights and depths above and below the datum level of the Ordnance Survey of Great Britain to be inserted. Contours to be drawn at intervals of 200 feet, with subsidiary contours where they are necessary, to give expression to the features of the ground. Incidental features, such as cliffs, &c., to be marked. The map to be tinted according to height (2) A grant of 25*l.* to be applied for in order that a specimen sheet of the map may be prepared. (3) The Clyde Trustees to be approached, with a view to their undertaking the preparation of a similar map of the Clyde estuary on a suitably larger scale. Other harbour Boards to be similarly approached.

(4) The Committee anticipate that, being provided with maps of this character as specimens of what is required to supply a national want, the Association may be in a better position than at present to move the Government to undertake the preparation of a similar map of the whole of the United Kingdom, based mainly upon the extensive data already available in the archives of the Ordnance Survey and the Admiralty.

Report of the Committee, consisting of Dr. J. H. Gladstone (Secretary), Prof. Armstrong, Mr. William Shaen, Mr. Stephen Bourne, Miss Lydia Becker, Sir John Lubbock, Bart., Dr. H. W. Crosskey, Sir Richard Temple, Sir Henry E. Roscoe, Mr. James Heywood, and Prof. N. Story-Maskelyne, appointed for the Purpose of Continuing the Inquiries Relating to the Teaching of Science in Elementary Schools.—No steps in advance have been taken by any Government Department towards the more adequate provision for science-teaching in elementary schools during the past year. There have been four different Vice-Presidents of the Committee of Council on Education during the last twelve months; and Sir Lyon Playfair only came into office after the Code for the year had been settled. The annual return of the Education Department for England and Wales issued this year, which deals with the period from September 1, 1884, to August 31, 1885, shows that the present regulations tell unfavourably on the prospects of science. The following statistics for the last three years show that, while the preferential class subject "English" is taken in an increasing number of departments year by year, geography shows an actual falling off, and elementary science seems even to be losing the little footing it had. Needlework shows a steady increase, as it is an obligatory subject in girls' schools, and it is more advantageous in a financial point of view to take it up as a class subject rather than under Article 109 (*c*), in which case it necessarily displaces geography or science:—

Class Subjects	1882-83 Departments	1883-84 Departments	1884-85 Departments
English	18,363	19,080	19,431
Geography	12,823	12,775	12,336
Elementary Science	48	51	45
History	367	382	386
Needlework	5,286	5,929	6,499
	18,524	19,137	19,266

In regard to the scientific specific subjects, the following are the number of children individually examined:—

Specific Subjects	1882-83 Children	1883-84 Children	1884-85 Children
Algebra	26,547	24,787	25,347
Euclid and Mensuration	1,942	2,010	1,269
Mechanics, A	2,042	3,174	3,527
,, B	—	206	239
Animal Physiology	22,559	22,857	20,869
Botany	3,280	2,604	2,415
Principles of Agriculture	1,357	1,859	1,481
Chemistry	1,183	1,047	1,095
Sound, Light, and Heat	630	1,253	1,231
Magnetism and Electricity	3,643	3,244	2,864
Domestic Economy	19,582	21,458	19,437
Extra (Physiography)	—	16	—
	82,965	84,515	79,774

No of Scholars in Standards V., VI., VII.	286,355	325,205	352,860

It is evident that while the number of scholars in the higher standards has considerably increased, the number examined in specific (scientific) subjects has considerably decreased; and this decrease has occurred in every subject except mechanics. Algebra and chemistry show rather larger numbers than last year, though not in proportion to the increase of scholars. The comparative decrease in the attention paid to these scientific subjects will be evident from the percentages of children examined:—

In 1882–83	29·0 per cent.
In 1883–84	26·0 ,,
In 1884–85	22·6 ,,

but it must be borne in mind that in many schools the children take two subjects, in which case they count accordingly. Increased though still very inadequate attention seems to be paid in the training colleges to the preparation of the students in the science subjects; the number of individual students who have qualified for teaching one or more sciences has risen from 2205 in 1884 to 2407 in 1885, and it is satisfactory to note that the increase has been mainly in passes in the first class. The number of papers worked in the several subjects in the two years under review have been as follows:—

Number of papers worked	1884	1885
Pure Mathematics	82	121
Theoretical Mechanics	21	25
Sound, Light, and Heat	488	690
Magnetism and Electricity	693	551
Inorganic Chemistry	245	269
,, ,, (practical)	166	160
Animal Physiology	416	257
Botany	485	483
Physiography	1030	1095
Principles of Agriculture	289	386

The increase has been mainly in sound, light, and heat, and the principles of agriculture; the falling off has been chiefly in animal physiology, and magnetism and electricity. The Scotch Code differs from the English in regard to the teaching of science in several points, but the annual return does not exhibit a much more hopeful state of affairs. The importance of technical instruction is making rapid progress in popular estimation, but this subject has not got a real footing as yet in elementary schools, owing to the inaction of the Government pending a definite expression of opinion by the House of Commons.

SECTION A—MATHEMATICAL AND PHYSICAL SCIENCE

On Stationary Waves in Flowing Water, Part I., by Sir William Thomson.—This subject includes the beautiful wave-group produced by a ship propelled uniformly through previously still water, but the present communication[1] is limited to two-dimensional motion

Imagine frictionless water flowing in uniform *régime* through an infinitely long canal with vertical sides; and bottom horizontal except where modified by transverse ridges or hollows, or slopes between portions of horizontal bottom at different levels. Included among such inequalities we may suppose bars above the bottom, fixed perpendicularly between the sides. Let these inequalities be all within a finite portion, AB, of the length, and let f denote the difference of levels of the bottom on the two sides of this portion, positive if the bottom beyond A is higher than the bottom beyond B.

Now, let the water be given at an infinite, or very great, distance beyond A, perpetually flowing towards A with any prescribed constant velocity, V, and filling up the canal to a prescribed constant depth, D. It is required to find the motion of the water towards A, through AB, and beyond B as disturbed by the inequalities between A and B. This problem is essentially determinate; and it has only one solution if we confine it to cases in which the vertical component of the water's velocity is everywhere small in comparison with $\sqrt{gD}$, the velocity acquired by a falling body falling from a height equal to half the depth.

In particular cases the water flows away unruffled at great distances from B. But, in general, the surface is ruffled, and the water flows "*steadily*" between the plane bottom and a corrugated free surface, as in the well-known appearance of water flowing in a mill-lead, or Highland burn, or in the clear rivulet on the east side of Trumpington Street, Cambridge. The train of diminishing waves which we see in the wake of each little irregularity of the bottom would, of course, extend to infinity if the stream were infinitely long, and the water absolutely inviscid (frictionless); and a single inequality, or group of inequalities, in any part, AB, of the stream, would give rise to corrugation in the whole of the flow after passing the inequalities, more and more nearly uniform, and with ridges and hollows more and more perpendicular to the sides of the canal, the farther we are from the last of the inequalities. Observation, with a little common-sense of the mathematical kind, shows

[1] I have since found, in a sufficiently practical form, the solution for the wave-group produced by the ship, which I hope to communicate to the *Philosophical Magazine* for publication in the November number.—W. T., September 13, 1886.

that at a distance of two or three wave-lengths from the last of the irregularities if the breadth of the canal is small in comparison with the wave-length, or at a distance of nine or ten breadths of the canal if the breadth is large in comparison with the wave-length, the condition of uniform corrugations with straight ridges perpendicular to the sides of the canal, would be fairly well approximated to, even though the irregularity were a single projection or hollow in the middle of the stream. But the subject of the present communication is simpler, as it is limited to two-dimensional motion; and our inequalities are bars, or ridges, or hollows, perpendicular to the sides of the canal. Thus, in our present case, we see that the condition of ultimate uniformity of the standing waves in the wake of the irregularities is closely approximated to at a distance of two or three wave-lengths from the last of the inequalities.

A mathematical treatment of the problem thus presented, which will appear in the October number of the *Philosophical Magazine*, gives, among other results, the following conclusions:—

Generally, in every case when $V < \sqrt{gD}$ the upper surface of the water rises when the bottom falls, and the water falls when the bottom rises.

On the other hand, when $V > \sqrt{gD}$, the water surface rises convex over every projection of the bottom, and falls concave over hollows of the bottom; and the rise and fall of the water are each greater in amount than the rise and fall of the bottom; so that the water is deeper over elevations of the bottom, and is shallower over depressions of the bottom.

Returning now to the subject of standing waves (or corrugations of the surface) of frictionless water flowing over a horizontal bottom of a canal with vertical sides, I shall not at present enter on the mathematical analysis by which the effect of a given set of inequalities within a limited space, AB, of the canal's length, in producing such corrugation in the water after passing such inequalities, can be calculated, provided the slopes of the inequalities and of the surface corrugations are everywhere very small fractions of a radian. I hope before long to communicate a paper to the *Philosophical Magazine* on this subject for publication. I shall only just now make the following remarks:—

(1) Any set of inequalities large or small must in general give rise to stationary corrugations large or small, but perfectly stationary, however large, short of the limit that would produce infinite convex curvature (according to Stokes's theory an obtuse angle of 120°) at any transverse line of the water surface.

(2) But in particular cases the water flowing away from the inequalities may be perfectly smooth and horizontal. This is obvious because of the following reasons:—

(i.) If water is flowing over plane bottom with infinitesimal corrugations, an inequality which could produce such corrugations may be placed on the bottom so as either to double those previously existing corrugations of the surface or to annul them.

(ii.) The wave-length (that is to say, the length from crest to crest) is a determinate function of the mean depth of the water and of the height of the corrugations above it, and of the volume of water flowing per unit of time. This function is determined graphically in Stokes's theory of finite waves. It is independent of the height, and is given by the well-known formula when the height is infinitesimal.

(iii.) From No. ii. it follows that, as it is always possible to diminish the height of the corrugations by properly adjusted obstacles in the bottom, it is always possible to annul them.

(3) The fundamental principle in this mode of considering the subject is that, whatever disturbance there may be in a perpetually sustained stream, the motion becomes ultimately steady, all agitations being carried away down stream, because the velocity of propagation, relatively to the water, of waves of less than the critical length, is less than the velocity of flow of the water relatively to the canal.

In Part II., to be published in the November number of the *Philosophical Magazine*, the integral horizontal component of fluid pressure on any number of inequalities in the bottom, or bars, will be found from consideration of the work done in generating stationary waves, and the obvious application to the work done by wave-making in towing a boat through a canal will be considered. The definitive investigation of the wave-making effect when the inequalities in the bottom are geometrically defined, to which I have just now referred, will follow; and I hope to include in Part II., or at all events in Part III. to be published in December, a complete investigation, illustrated by drawings, of the beautiful pattern of waves produced by a ship propelled uniformly through calm deep water.

On a New Form of Current-Weigher for the Absolute Determination of the Strength of an Electric Current, by Prof. James Blyth.—The object of this paper is to describe a method of absolutely determining the strength of an electric current by measuring in grammes' weight the electro-magnetic force between two parallel circular circuits, each carrying the same current. For convenience of calculation the circles have the same radius, and are placed with their planes horizontal. The construction of the instrument is as follows:—A delicate chemical balance is provided, and the scale-pans replaced by two suspended coils of wire. Each of these is made of a single turn of insulated copper wire (No. 16 about) fixed in a groove round the edge of an annular disk of glass or brass of suitable diameter. The disk is made as thin and light as possible consistently with perfect rigidity. By means of two vertical pillars of brass this annulus is attached to a rigid cross-bar of dry wood or vulcanite, in the middle of which is placed a hook for suspending the whole from one end of the balance-beam. On each side of the hook, and equally distant from it, two slender rods of brass are screwed in in the wooden bar, which support two small platinum cups for holding mercury or dilute acid. The position of these cups is so adjusted that, when the whole hangs freely, the cups are in line with the terminal knife-edge of the balance-beam, and have their edges just slightly above its level. The free ends of the insulated wire surrounding the disk, after being firmly tied together for a considerable length and suitably bent, are soldered to the brass supports of the platinum cups, which thus serve as electrodes by means of which a current may be sent through the suspended coil. A precisely similar coil is suspended from the other end of the balance-beam. We now come to the arrangement by means of which a current is led through the suspended coils, so as to interfere as little as possible with the sensibility of the balance. This constitutes the essential peculiarity of the instrument, and is effected in the following way:—An insulated copper wire, having its ends tipped with short lengths of platinum, is run along the lower edge of the beam, and is firmly lashed to it by well-rosined silk thread. The ends of this wire, bent twice at right angles, are so placed that their platinum tips dip vertically into one of each pair of the platinum cups which are attached to the vertical rods of the suspended coils. From the other cup of each pair proceed two similarly tipped copper wires, which run along the upper edge of the beam, and are also firmly tied to it. These wires, however, only proceed as far as the middle of the beam, where they are bent, first outwards, one on each side of the beam, at right angles to it, and then downwards, so that the platinum tips are vertical. The latter dip into two platinum cups attached to two vertical rods, which spring from the base-board of the balance. These rods are placed at equal distances on each side of the beam, and are of such length that the platinum cups are in line with the central knife-edge of the beam and have their edges just a little above its level. There are thus in all six cups and six dipping wires. Three of these are in line on one side of the beam, and three on the other. Also the line joining the points of each pair of dipping wires is made to coincide with the corresponding knife-edge; and, further, the edges of the cups are all in the same plane when the balance is in equilibrium. From this it will be obvious that any motion of the beam in the act of weighing causes only a very slight motion of the platinum wires, which dip into the fluid contained in the cups. The resistance, due to the viscosity of the fluid, is thus very small, even in the case of mercury, and much smaller still when dilute acid is used. In point of fact, the diminution of sensibility due to this cause is less than in the case of determining the specific gravity of solids by weighing in water in the ordinary way. With clean mercury it is quite easy to weigh accurately to a milligramme. The fixed coils, constituting two pairs, have the same diameter as the suspended coils, and, like them, are made of single turns of insulated wire wound round the edges of circular disks of glass or brass. The disks of each pair are fixed at the requisite distance apart to a cylindrical block of wood, so as to have their planes exactly parallel and their centres in the same straight line. To insure this they are turned up and finished on the same cylindrical block on which they are finally to rest. When in position they are so placed that, when the balance is in equilibrium, each suspended coil hangs perfectly free to move with its plane horizontal and exactly midway between a pair of fixed coils. For

this purpose, as will be seen, it is necessary that two large holes be drilled in the upper disk of each pair, so as to allow the brass pillars of the corresponding annular disk to pass freely through. When the connections are made, the current is led through the entire apparatus in such a way that, while the electromagnetic force acting on the one suspended coil causes it to descend, the electro-magnetic force acting upon the other causes them to ascend. The total force tending to disturb the equilibrium of the balance is thus exactly four times that due to an equal current circulating in two parallel circles of the same diameter and with their planes at the same distance apart. The current-strength is estimated from the number of grammes required to restore the balance to exact equilibrium, the weights being placed into small scale-pans attached to the movable part of the apparatus. The electro-magnetic force between each fixed and the corresponding suspended coil is calculated from the formulæ given by Clerk Maxwell (vol. ii. p. 308), viz. :—

$$\frac{dM}{db} = -2\pi \cos\gamma \left\{2F\gamma - (1 + \sec^2\gamma)\,E\gamma\right\}$$

where M = the potential energy between two parallel circles, each carrying unit current,

b = distance between their planes,

a = radius of each coil,

$$\sin\gamma = \frac{2a}{\sqrt{4^2a + b^2}},$$

$F\gamma$ and $E\gamma$ = first and second complete elliptic integrals to modulus $\sin\gamma$.

In one of the instruments constructed

$$a = 10{\cdot}8 \text{ inches}, \; b = {\cdot}566 \text{ inches},$$

which give

$$\gamma = 87^\circ,\; F\gamma = 4{\cdot}338653976,\; E\gamma = 1{\cdot}00525857\,;$$

from which, if G denote the constant of the instrument and $g = 981$, we have

$$G = 4 \,.\, \frac{dM}{db} \,.\, \frac{1}{g} = {\cdot}4818.$$

This gives for 1 ampere a force = ·04818 gramme-weight.

Besides the one exhibited I have constructed several modifications of the instrument, only one of which, however, needs be particularly mentioned. In it both the fixed and movable coils are replaced by flat spirals of wire, each of eleven turns. Here the practical construction is more difficult, and the calculation of the constant somewhat more laborious, unless one is content with merely integrating over the area of both the fixed and suspended spirals. This is, I think, however, hardly legitimate, at least with thickish wires, as we thereby suppose that electricity is circulating in the insulating spaces between the wires as well as in the wires themselves. To avoid this I have actually calculated the force exerted by each one of the coils of the fixed spiral upon each coil of the suspended spiral. This entails great labour, as the elliptic integrals have to be calculated for values of the modulus differing very slightly from each other. The labour, however, is worth the taking, as the attractive or repulsive force between two flat spirals is so much greater than that between two simple circles.

The Peculiar Sunrise-Shadow o Adam's Peak in Ceylon, by the Hon. Ralph Abercromby, F.R.Met.Soc.—A great peculiarity has been noticed by many travellers about the shadow of Adam's Peak at sunrise. The shadow, instead of lying flat on the ground, appears to rise up like a veil in front of the spectator, and then suddenly to fall down to its proper level. Various theories have been propounded to account for this, and it has usually been supposed to be due to a sort of mirage. The author, in the course of a meteorological tour round the world, spent the night on the top of the peak, 7352 feet above the sea, and obtained unmistakable evidence that the appearance is due to light wreaths of thin morning mist being driven past the western side of the mountain by the prevailing north-east monsoon up a neighbouring gorge. The shadow is caught by the mist at a higher level than the earth, and then falls to its own plane on the ground as the condensed vapour moves on. The appearance is peculiar to Adam's Peak ; for the proper combination of a high isolated pyramid, a prevailing wind, and a valley to direct suitable mist at a proper height on the western side of a mountain, is only rarely met with. Any idea that the appearance could be caused by mirage is completely disproved by the author's thermometric observations.

Description of a New Calorimeter for Lecture-Purposes, by T. J. Baker.—The instrument consists of two exactly similar metallic air-thermometers mounted side by side with their U-shaped thermometer-tubes adjacent, so that their indications can be easily compared with each other. The air-vessel of each thermometer contains a cylindrical well, in which the substance to be experimented with is immersed. Each well is provided with a discharging-tube furnished with a stop-cock. The scale common to both thermometers is of milk-glass, divided into 100 equal parts both above and below zero, and let into the stand so as to constitute a translucent window which can be illuminated from behind. By means of this instrument many thermal problems can be demonstrated before a large audience.

On the Distribution of Temperature in Loch Lomond and Loch Katrine during the past Winter and Spring, by J. T. Morrison, M.A.—The author made observations on the temperature of these lakes on or about the term day of each month from December 1885 to June 1886, in continuation of Mr. J. Y. Buchanan's researches. These included the whole length of Loch Katrine and the head and middle part of Loch Lomond, the deepest sounding, 99 fathoms, being got near Inversnaid in the latter lake. At Inversnaid, from December till March, the water was each month of uniform temperature from surface to bottom, the temperatures being—

December 22, 1885	42°·8
January 21, 1886	41·2
February 23, 1886	40·05
March 23, 1886	39·05

In the deepest sounding obtained on Loch Katrine, 79 fathoms, a similar distribution was met with up till February, the readings being—

December 23, 1885	(42°·3) [1]
January 22, 1886	40·4
February 24, 1886	39·0

And, though the maximum density-point was thus attained in February, uniformity still prevailed in March down to a depth of 70 fathoms, the readings on March 24 being : surface, 38°·1 ; 70 fathoms, 38°·1 ; 79 fathoms, 38°·7. In April the temperature distribution usually found in spring had set in in both lakes, the surface being warmest, the bottom coldest, and the temperature falling more and more slowly with increase of depth. The circumstance of most interest, however, is that the warmth of the bottom layer increased monthly over the deepest parts of both lakes, as follows :—

	March	April	May	June
	°	°	°	°
Loch Lomond (99 fathoms)	39·05	39·4	40·3	40·6
Loch Katrine (79 fathoms)	38·7	39·1	40·1	40·65

This rise is evidently due not to the conduction of heat nor to the penetration of solar radiation, but to some drainage or oozing causing mixture. This supposition seems necessary also to explain the behaviour of Loch Katrine in March. Drainage *en masse* appears to occur chiefly in winter and spring, not in summer when the river water and the lake surface water are much warmer than the deep water of the lake. The mean temperature of Loch Katrine probably has a greater range than that of Loch Lomond. The shallower parts of the lakes resemble the deep parts as to uniformity of temperature up till March. But their yearly range is greater. In both lakes the mean temperature becomes uniform along the whole length about April 4.

On the Distribution of Temperature in the Firth of Clyde in April and June 1886, by J. T. Morrison, M.A.—In the latter parts of April and June of this year Mr. John Murray, Dr. Mill, and the author made serial temperature soundings throughout the Clyde district, chiefly with Negretti and Zambra's reversing thermometer. It was found that in matter of temperature the waters of the district were divisible into four groups : I. North Channel and the plateau south of Arran ; II. the Arran and Dunoon open basins ; III. the deep-sea lochs ; IV. the shallow sea lochs. The average temperature in each group at every depth was calculated for April and June, and these averages form the basis of this paper. In April in all groups there is a deep layer of uniform temperature overlaid by a layer of temperature rising steadily to the surface. In groups II., III., and IV. the uniform deep temperatures are almost the same, about 41°·4 F. ; in group I. it is 41°·8 F. In June the superficial

[1] No sounding made here in December. Above temperature is calculated from that of another part of the lake.

layer of varying temperature had thickened to about 20 fathoms. The deep temperatures in the groups were now very different:—

	I.	II.	III.	IV.
Deep temperature in April ...	41·8	41·3	41·5	41·5[1]
,, ,, June ...	46·7	43·9	43·8	45·3[1]
Rise of temperature	4·9	2·6	2·3	3·8

To groups III. and IV. analogues are found in a deep and a shallow basin of Loch Lomond, in both of which the bottom temperature rose between April and June. From this it is inferred that land-influences, especially drainage *en masse*, produce most of the effect noticed in III. and IV. The great rise in the North Channel and southern plateau is evidently due to a warm oceanic current. The rise in temperature in group II. is due to the incoming of warm water from without. As the water between 30 and 75 fathoms in this group is very uniform in temperature, and as the south plateau is 25 fathoms below the surface, it is supposed that the dense plateau water is carried into the open basins (group II.), and through convection mixes thoroughly the water below 30 fathoms there. Loch Goil is specially remarkable for its isolation and the small rise of bottom temperature —0°·6 F. in two months. In Upper Loch Fyne a lenticular mass of water below 43°·0 F. was found in June to float between two warmer layers. Its greatest thickness, 30 fathoms, was opposite Inverary. The bottom layer of 44°·0 F. was not found to be in connection with any equally warm layer either inside or outside of the loch.

On the Critical Curvature of Liquid Surfaces of Revolution, by A. W. Rücker, M.A., F.R.S.—Let a mass of liquid film be attached to two equal circular rings, the planes of which are perpendicular to the line joining their centres. It will form a surface of revolution the equation of which is, according to Beer,—

$$y^2 = \alpha^2 \cos^2 \phi + \beta^2 \sin \phi,$$
$$x = \alpha \mathrm{E} + \beta \mathrm{F},$$

where F and E are elliptic integrals of the first and second kinds respectively, the amplitude being ϕ, and the modulus $\kappa = \sqrt{\alpha^2 - \beta^2}/\alpha = \sin \theta$. If θ be conceived as increasing from 0, when it is in the first quadrant the figure will be an unduloid lying being the cylinder and the sphere, in the second quadrant a nodoid, the limits of which are the sphere and a circle. In the third and fourth quadrants the figure will be dice-box-shaped with a contraction in the middle, being a nodoid in the third and an unduloid in the fourth quadrant. The one passes into the other through the catenoid. If now we suppose the rings to be at a fixed distance apart, and the volume of the surface to be altered, the curvature will change, and the direction of the change will depend on the diameter and distance apart of the rings, and on the magnitude of the maximum or minimum ordinate (the *principal ordinate*), which lies half-way between them. The object of the paper is to investigate the general relation between these quantities when the curvature is a maximum or minimum, if the changes in the form of the film take place subject to the conditions that the diameter and distance of the rings are constant. It has been recently shown by Prof. Reinold and the author that, if these conditions hold,

$$(\alpha^2\mathrm{E} - \beta^2\mathrm{F} + \alpha^2\Delta_1 \cot \phi_1)\delta\alpha + \alpha^2(\mathrm{F} - \mathrm{E} + \Delta_1 \tan \phi_1)\delta\beta = 0,$$

where ϕ_1 is the upper limit of the integrals and

$$\Delta_1 = \sqrt{1 - \sin^2 \theta \sin^2 \phi_1}.$$

Writing this in the form $\mathrm{A}\delta\alpha + \mathrm{B}\delta\beta = 0$, it is proved that the curvature has in general a critical value when $\mathrm{A} - \mathrm{B} = 0$; so that

$$2\mathrm{E} - \mathrm{F}(1 + \cos^2 \theta) + 2\Delta_1 \cot 2\phi_1 = 0$$

is a condition which must be satisfied by θ and ϕ_1. To find values of ϕ_1 corresponding to given values of θ the equation must be solved by trial; but it is proved that, if a pair of corresponding values is given when θ lies (say) in the first quadrant, the values of ϕ_1 can be at once found which correspond to $\pi - \theta$, $\pi + \theta$, and $2\pi - \theta$. The values of ϕ_1 corresponding to θ and $\pi - \theta$ are equal, and, if ϕ_2 be the value corresponding to $\pi + \theta$ and $\pi - \theta$, it is given by the equation

$$\tan \phi_1 \tan (\pi - \phi_2) = \sec \theta.$$

By means of these equations a curve can be drawn, showing the relations between ϕ_1 and θ, and thence are found the values of p/Y, X/p, and X/Y, where $2\mathrm{Y}$, $2\mathrm{X}$, and $2p$ are the diameter and distance of the rings and the magnitude of the principal diameter. If we now conceive the two rings gradually to approach or recede from each other, and the principal diameter to be altered so that the condition of critical curvature is always fulfilled, it is proved that the changes in its form would be as follows:—Beginning with the cylinder, the distance of the rings would (as has been shown by Maxwell, Art. "Capillarity," "Enc. Brit.") be half their circumference. As the diameter increases, the rings would move apart, and the distance between them would be a maximum when $\theta = 64°·2$, being 17 per cent. greater than in the case of the cylinder. When $\theta = 90°$, the figure is a sphere, and the distance between the rings is about 4 per cent. less than in the case of the cylinder. The sphere has a larger diameter than any other figure of critical curvature. The surface next becomes a nodoid, and the distance between the rings diminishes till when $\theta = 180°$ they touch, and thus the surface reduces to a circle. In the next quadrant the rings separate, but the figure is now dice-box-shaped, and the pressure exerted by the film is outwards. When $\theta = 270°$, the figure is the catenoid. The principal ordinate is then less than that of any other figure of critical curvature, and the radius of the rings is a mean proportion between this minimum ordinate and the maximum which was attained in the case of the sphere. The same relation holds between the principal ordinates of any two figures which correspond to values of θ which differ by 180°. In the fourth quadrant the figure becomes an unduloid, the pressure is inwards, the rings continue to separate, and the ratio of the distance between the rings to the principal ordinate is a maximum when $\theta =$. In the paper tables and curves are given to illustrate the "march" of these functions. To secure continuity, the problem is discussed without reference to the question as to whether the surfaces are in stable equilibrium, though those in the first and fourth quadrants and figures corresponding to values of θ not much $> \pi/2$ and not much $< 3\pi/2$ certainly are. In conclusion it is shown that by means of the curves we can solve a number of problems with sufficient accuracy for practical purposes. Thus, if any two of the three quantities, the diameter of the rings, the distance between them, and the diameter of the surface of critical curvature, are given, the third can be found.

SECTION B—Chemical Science

Absorption Spectra of Uranium Salts, by Dr. W. J. Russell and W. J. Lapraik.—This paper was communicated by Dr. Russell, who pointed out that well-marked absorption bands in the visible spectrum are produced by the different salts of this metal; the bands produced by the uranous salts are distinct from those given by the uranic salts; both consist, however, of three distinct bands or groups of bands. The bands produced by the uranous salts are at the red end of the spectrum, whilst those due to the uranic salts are at the blue end; and when both classes of salts are mixed in solution there are three series of bands distributed with tolerable regularity over the whole of the spectrum. Experiments with different salts show the nature of the acid radical to have no influence on the spectrum, whereas in the case of other metals, such as cobalt, it has been found that different radicals produce different spectra. The spectrum common to all uranic salts is slightly altered by the addition of free acid; a diminution in intensity in the least refrangible bands and a slight shift in others has been observed. Crystals of uranic nitrate give an absorption spectrum similar to that produced by its solutions. The spectrum of the uranous salts is less refrangible than that of the uranic salts; the examination of the spectra produced by the uranous salts in the solid state was found to be more complex than those given by these salts in solution.

The Air of Dwellings and Schools, and its Relation to Disease, by Prof. Carnelly.—The author gave an account of an elaborate series of experiments conducted by him and Dr. Haldane at Perth and Dundee, in connection with the sanitary and school authorities, the object being to determine the relations between the composition of the air and the death-rate in houses and schools, and also the effect of various systems of ventilation. For this purpose the carbon dioxide, organic matter, and micro-organisms were determined, both in the outside air and in the room to be examined. In the air of the towns of Perth and Dundee a distinct increase of impurities could be detected in close parts of the towns as compared with the open spaces. In examining the dwelling-houses, the experimenters had authority

[1] Average temperature of first few fathoms above bottom.

from the sanitary officers, and visited bed-rooms and similar places during all parts of the day or night, while actually occupied by the inhabitants. Houses are divided in the tables into one-, two-, and four-roomed dwellings, and mention was made of some cases in single-roomed dwellings in which eight persons were found sleeping in a single bed, and in many cases no bed was found in the dwelling at all. The impurities in the air of such houses were naturally much greater than in better class, and by a careful comparison of chemical composition of the air with the death-rates from various causes in the various classes of houses, it was shown that on an average the length of life in a one-roomed house was only twenty years, whilst that in better-class houses is forty years. Hence a person born and living in a one-roomed house has a chance of living only half as long as those born and living in a four-roomed house. This depends naturally to a considerable extent on other causes than impure air-supply. Some irregularity was observed in the cases of consumption, scarlet fever, and diphtheria, which is, however, quite capable of explanation. The influence of cubic space on the purity of the air in dwelling-rooms was somewhat unexpected, the best results being noticed when 1000 cubic feet was allowed for each person. With larger rooms, owing to stagnation of the air, the result is not so good. Sixty-eight schools in Dundee were examined; of these, twenty-six were mechanically ventilated, while the others were ventilated by means of windows. The advantages were distinctly in favour of mechanical ventilation, the micro-organisms being one-seventh, and the carbon dioxide one-half of that in the other schools. Mechanical ventilation not only materially improves the quality of the air, but also has less influence in unduly reducing its temperature. On comparing together boys' and girls' schools the air is almost invariably less pure in boys' schools. The amount of carbon dioxide does not afford any indication of the amount of organic matter or micro-organisms, except by taking the mean of a large number of experiments. Cleanliness of person has a comparatively small influence on the number of micro-organisms, but cleanliness of dwelling-rooms and schools has a most important effect. Hence the air of new schools is distinctly better than that of older buildings. In conclusion, the author suggested that in many cases the evil said to be due to over-pressure in schools was doubtless due to imperfect ventilation, and that if Dundee may be fairly regarded as an example of a British town, then certainly our schools are most imperfectly ventilated; and that for improvement in this respect the advantage of mechanical ventilation should be strongly insisted upon.

The Preservation of Gases over Mercury, by H. B. Dixon, M.A., F.R.S.—From a statement in Bence-Jones's "Life of Faraday" it would appear that a difference of opinion between Faraday and Davy existed on this point, and according to the experiments of the former gases cannot be indefinitely preserved over mercury, whilst the latter found that hydrogen could be preserved over mercury for a considerable time without suffering change. The author has examined various gases, including hydrogen, cyanogen, sulphur dioxide, and electrolytic gas, which had been kept over mercury for periods ranging from 2½ to 9½ years, and concludes that the gases had suffered no change in the time.

The Distribution of the Nitrifying Organism in the Soil, by R. Warington, F.R.S.—Previous experiments have shown the limit of depth at which this organism exists in soil to be about 18 inches, but later experiments have shown it to exist at depths of 3 feet, and in some cases at depths of 5 and 6 feet.

The Fading of Water Colours, by Prof. W. N. Hartley, F.R.S.—The author, referring to the correspondence in the *Times* and to an article in the *Nineteenth Century* on this subject, pointed out that two ideas had been brought forward in connection with this matter—one being that water-colour drawings fade on keeping, while others have contended that the tints increase in depth on keeping for a length of time. Hence, on the one hand it has been recommended to keep water-colour drawings in the light, while others have suggested that darkness is preferable. Colours used are of two kinds, mineral and organic. Mineral colours are generally unalterable, except in special colours, such as lead. The tendency is for red light to act as an oxidising agent on such colours, while violet light exerts a reducing action. But in the case of organic colouring-matters oxidation is promoted by light from either end of the spectrum. Acidity in any form is a great cause of the deterioration in water-colours. The chief sources of acidity are the impurities in the atmosphere in presence of moisture, imperfectly prepared colours, and the acidity of the paper. The paper is always itself slightly acid, and the use of size or gum is a source of acidity, while the burning of coal and of gas in towns produces a sensible amount of sulphurous acid in the atmosphere. The author has carefully examined the effect of acids, of exposure to sunlight, of hydrogen peroxide, and of sulphurous acid in the case of sixteen common water-colours. As a result he concludes that the character of the colours examined is very creditable to the manufacturer. Lakes are very permanent in pure air; while cases are known where indigo has remained unchanged for upwards of 1800 years. Indigo is, however, liable to be attacked by acids. Generally the effect of chemical agents upon water-colours is what might have been expected from their chemical composition. Thus yellows containing cadmium sulphide are bleached by oxidising agents. In some few cases, however, unexpected results were obtained. Ultramarine is very readily affected by dilute acids; no preparation of lead should be used as a pigment either for oil or water-colour drawing. It is shown that many water-colour drawings have been exposed to light for fifty years or more in properly arranged galleries, without appreciable deterioration. The tendency is to produce apparently darker tints, owing to the lighter tints being most likely to fade, while the brown colour developed in the paper itself tends to produce a similar effect. To preserve delicate sunlight effects the drawings should be kept in rooms imperfectly illuminated, and preferably with blinds transmitting a yellow or brown light. They should be carefully protected from the effects of an impure atmosphere, while paste or gum should not be used in affixing them. A slight wash of borax on the paper destroys its acid reaction, and makes the colours fix readily on the fibres. A small quantity of borax might be used in the water employed for mixing the colours. For illuminating galleries incandescent lamps are to be preferred to lighting by the electric arc, as the latter may be regarded as a sure means of destruction of the colours.

The Colour of the Oxides of Cerium and its Atomic Weight, by H. Robinson, M.A.—A criticism of the work of Wolf, on the atomic weight of cerium, published in the *American Journal of Science and Art*, 1868, upon which the atomic weight, 138 cerium, given in Clark's "Constants of Nature," is based. The author contends that Wolf's method of preparation would give lanthanum and not ceric oxide; experimental evidence was given in support of this contention; further the author maintains that ceric oxide is yellow and not white, as described by Wolf.

On the Relative Stability of the Hydrochloride $C_{10}H_{17}Cl$ Prepared from Turpentine and Camphene respectively, by E. F. Ehrhardt (Mason College).—According to Ribau the first of these hydrochlorides is the less easily decomposed by water, whereas the author finds it to be the one most easily decomposed under the influence of temperature. At a low red heat Tilden has shown turpentine is more completely dissociated than camphene, and this the author has shown to be true for lower temperatures. The paradoxical result that the hydrochloride of the more stable hydrocarbon is less stable than that from the unstable one, is regarded as proving this compound to be a molecular one, in which the chlorine is associated with the hydrogen of the acid and at the same time to the hydrocarbon.

On Derivatives of Tolidine and Azotolidine Dyes, by R. F. Ruttan, B.A., M.D.—An account of the preparation of tolidine, which is the homologue of benzodine, and obtained by a similar mode of preparation. Several derivatives of this base were described, as also azotolidine or tetra-azoditolyl, which is produced by the action of nitrous acid on the base. This compound forms the starting-point in the preparation of a series of important dyes, by which cotton and wool fibre may be dyed without the use of a mordant.

On the Chemistry of Estuary Water, by H. R. Mill, D.Sc. —The salinity (ratio of total dissolved matter in water) has been determined from point to point in the Firth of Clyde and Firth of Forth. In the case of the latter the distribution of salinity has been shown to be constant all the year round, whilst in the case of the Clyde there are periodical variations through the whole mass of the water. In the case of the Forth River entrance, it is evident a mixture of river and sea water takes place by a true process of diffusion, maintaining a constant gradient from river to sea. The dissolved matter of fresher water was found richer in calcium carbonate than sea water.

The Essential Oils; a Study in Optical Chemistry, by Dr. Gladstone, F.R.S.—After explaining how the refractive equivalent of an organic compound may be used to determine its con-

stitution, the author pointed out that the dispersion equivalents can be similarly used. The author also discussed the refraction and dispersion equivalents of the turpenes, citrenes, camphor, and of some other members of the group of essential oils, and showed how these values were of service in determining the constitution of these bodies.

An Apparatus for Maintaining Constant Temperatures up to 500° C., by G. H. Bailey, D.Sc., Ph.D.—The substance to be heated is placed in a glass tube, together with the bulb of an air-thermometer, which are inclosed in a wider tube resting on the iron casing of a furnace. The air-thermometer serves to measure the temperature, and is connected with a gas regulator, by which means the temperature may be kept constant at any desired temperature below that at which combustion-glass softens.

Treatment of Phosphoric Crude Iron in Open-Hearth Furnaces, by J. W. Wailes.—The process is similar to the ordinary puddling operation, and is conducted in a furnace with a basic lining; the metal is, however, removed from the furnace in a molten condition.

Notes on the Basic Bessemer Process in South Staffordshire, by W. Hutchinson.—The process described differs from the ordinary basic process inasmuch as the converting is conducted in two stages: (1) desiliconising of the metal in an acid-lined converter; (2) the dephosphorising in a converter with a basic lining.

Production of Soft Steel in a New Type of Fixed Converter, by G. Hatton.—Description of a converter, which is claimed to have many advantages over the Bessemer converter.

T. Turner, Assoc.R.S.M. (Mason College), read a series of papers relating to the chemistry of iron and steel. The first was *On the Influence of re-melting on the Properties of Cast Iron.* No general rule can be laid down as to the influence of re-melting on the properties of cast iron; chemical changes take place during the melting: the amount of silicon is reduced whilst that of the sulphur is increased, and the effect of re-melting will be dependent upon the proportion of these elements present in the cast iron; a single melting will be sufficient to produce a deterioration in the qualities unless the silicon is in excess. A second paper was *On Silicon in Cast Iron.* Addition of silicon to hard white iron causes it to become soft and grey, and too much silicon makes the iron weak; by adding silicon in right proportion cast iron can be made of any desired degree of hardness. The third communication was one *On Silicon in Iron and Steel.* The author has succeeded in making a steel in which the carbon is replaced by silicon, which can be hardened like steel, is very tough when cold, and is well adapted for tools, but is difficult to work when hot. The author gave a short description of a method for estimating carbon in iron or steel.

A New Apparatus for Readily Determining the Calorimetric Value of Fuel and Organic Compounds, by W. Thomson, F.R.S.E.—The apparatus-described is an improved form of the calorimeter due to Lewis Thompson; the substance is burnt in a stream of oxygen instead of mixing it with potassium chlorate, as recommended by Thompson.

On some Decompositions of Benzoic Acid, by Prof. Odling, F.R.S.—When benzoic acid is heated in sealed tubes at about 260° with an aqueous solution of zinc chloride, it is decomposed, and yields chiefly benzene, together with a small quantity of diphenyl.

On the Methods of Chemical Fractionation and *The Fractionation of Yttria*, by W. Crookes, F.R.S.—In the Presidential address this subject was referred to, and in this communication a detailed account of the operation of "fractionation" is given. Fractionation, briefly, consists of first fixing upon some chemical reaction in which there is a likelihood of a difference existing in the behaviour of the elements under treatment; this is then performed in an incomplete manner, so that only a portion of the total bases present is separated, the object being to get part of the material in the insoluble and the rest in the soluble state. In the second communication the author described the fractionation of the earth yttria; in this case the fractionation has been greatly facilitated by the use of what the author styles the "radiant-matter test," which is dependent upon the spectra given by these earths when phosphoresced *in vacuo*. It would appear that there are certainly five, and probably eight, constituents into which yttrium may be split.

SECTION C—Geology

Geysers of the Rotorua District, North Island of New Zealand, by E. W. Bucke.—The author of this paper has recently returned from the Lake district of New Zealand, where he spent eighteen months, and had exceptional opportunities for making observations upon the volcanic phenomena of the district. The largest geyser in New Zealand, that of the White Terrace of Rotomahana, is now destroyed. The author determined by soundings the depth of the tubes of several geysers of this district, and in the case of an extinct one, that of Te Waro, he was let down the tube. He found that this tube, 13 feet from the surface, opened into a chamber 15 feet long, 8 feet broad, and 9 feet high, from one end of which chamber another tube led downwards to an undetermined depth. Living among the natives for months, and speaking their language, the author was convinced that by constant observations on the direction of the wind and the condition of the atmosphere they have learnt to prognosticate the movements in all these hot springs with wonderful accuracy. He was also able to prove that during the whole time of his residence in the district certain of the geysers were only in eruption when the wind blew from a particular quarter.

On the Glacial Erratics of Leicestershire and Warwickshire, by the Rev. W. Tuckwell.—Gives evidence of a south-western dispersion from Charnwood. In Stockton, a village midway between Leamington and Rugby, is boulder-clay containing abundance of Mount Sorrel granite, of so-called gneiss from Charnwood Forest, largely decomposed "pockets" of red sandstone, blocks of grey sandstone highly glaciated, Bunter pebbles, flints, Carboniferous limestone, Lias rock of a different texture from that native to the district. Lying loose in the village street, recently inclosed and inscribed, is a fine boulder from Mount Sorrel, glaciated, of nearly two tons weight. The author notes extraordinary profusion of Mount Sorrel erratics as far as Leicester; at Rothley, Thurcaston, Anstey; "Stone," or "Ston," is a suffix of nearly all the villages along the line. The largest boulder found in Leicestershire is near Humberston, estimated at twenty tons, partly embedded in boulder-clay which is filled with Bunter pebbles and rolled slate from Charnwood. Charnwood stones re-appear north and south of Coventry, at Eathorpe, 6 miles south-west of Coventry, at Stockton, completing evidence of a south-west stream from the Charnwood elevation throughout the two counties.

Manganese Mining in Merionethshire, by C. Le Neve Foster, D.Sc.—Manganese ore is now being worked in the Cambrian rocks at several places near Barmouth and Harlech. It occurs in the form of a bed varying from a few inches to 3 feet in thickness; the average thickness is 1 foot to 1½ foot. The undecomposed ore contains the manganese in the form of carbonate, with a small proportion of silicate; but at the outcrop it is changed into a hydrated black oxide. Some of the outcrops of the manganese bed are erroneously marked on the Geological Survey maps as mineral veins, though Sir Andrew Ramsay was of opinion that the deposits were not true lodes. Recent workings show plainly that the deposits are truly stratified beds, or possibly various outcrops of one and the same bed, extending over a considerable area. The ore contains from 20 to 35 per cent. of metallic manganese, and is despatched to Flintshire and Lancashire for the manufacture of ferro-manganese. The new Merionethshire mines are the first instance of workings for carbonate of manganese in the British Isles.

On the Silurian Rocks of North Wales, by Prof. T. McKenny Hughes, M.A., F.G.S.—The author begins by describing some sections in the Silurian rocks of North Wales, giving lists of fossils from the various horizons in each. He then, by means of these and by what he calls syntelism, that is, the occurrence of similar sequences of beds of the same characters, lithological or other, points out the corresponding parts of the various sections described. He then does the same for the Silurian of the eastern borders of the Lake district, and, having in this manner constructed a vertical section of each, compares the two districts and shows that there is an identical series in each, with all the important zones of one represented in the other, except that in the part of North Wales which he has worked out he has not yet detected beds as high as the newer part of the series in the Lake district.

Note to accompany a Series of Photographs prepared by Mr Josiah Martin, F.G.S., to illustrate the Scene of the recent Volcanic Eruption in New Zealand, by Prof. J. W. Judd, F.R.S., Pres.G.S.—Owing to the great enterprise and energy shown by

the managers of the local newspaper press in New Zealand very full and graphic accounts of the volcanic outburst of June 10 have already reached this country, and have been copied into the English papers. On the day of the eruption, Dr. James Hector, C.M.G., F.R.S., the Director of the Geological Survey of New Zealand, started for the locality, and his preliminary report, accompanied by maps and plans, has been published. Dr. Hector concludes that the eruption was a purely hydrothermal phenomenon on a gigantic scale, and that it was unaccompanied by any ejection of freshly molten lava either in the form of fragmental matter or of lava-streams. I have been favoured by Mr. J. E. Clark, F.G.S., with specimens of the material ejected during the eruption, and the microscopic examination of these entirely supports Dr. Hector's conclusions. It is a most unfortunate circumstance that the beautiful sinter-terraces of Rotomahana appear either to be blown to fragments or covered up under the enormous masses of mud thrown out in that locality. It luckily happens that a number of most excellent photographs, which illustrate very beautifully the characters of the wonderful sinter-formations, have been obtained. Mr. Josiah Martin, F.G.S., has especially devoted himself to the study of the district, and the series of photographs now exhibited constitute an invaluable record of the characters of the district destroyed by the eruption. These photographs show the points at which the volcanic cones were formed upon Tarawera, and the beautiful characters of the White Terrace (Te Terata), and of the Pink Terrace (Otukapuarangi), and the other wonders which surround the now destroyed Lake of Rotomahana. Now that the European settlement has been formed at Rotorua, a great service would be rendered to science if a meteorological station could be established there, and by simultaneous observations of the atmospheric conditions, and of the state of activity of the numerous hot springs, the question of the exact relations between these two sets of phenomena clearly established. When we remember that a fall of 1 inch in the barometer is equivalent to the removal of a load of nearly 90,000 tons over every square mile of surface, the effect produced on a district where steam issues whenever a walking-stick is thrust into the ground must be enormous. What is especially needed, however, by vulcanologists is a carefully tabulated series of records in the place of the general statements which have hitherto been published on this most important question.

The Relations of the Middle and Lower Devonian in West Somerset, by W. A. E. Ussher, F.G.S.—It has been suggested by Mr. Champernowne that the Foreland and Hangman grits might really be the same series, the appearance of conformable superposition of Lynton upon Foreland beds at Oare being ascribed to inversion. According to this view the downthrow of the fault at Oare would be to the north. The paper discusses this suggestion, its important bearing on the mapping of the area entitling it to consideration. The author advances five points in favour of the hypothesis, and three adverse to it, and gives some reasons why such difficulties as are experienced in drawing boundaries between the Foreland grits and Hangman beds might reasonably be expected to occur. The arguments against the identity of the Foreland and Hangman groups are too strong to be entertained without positive evidence in its favour. The author then briefly disposes of the possibility of the absence of the Lynton beds east of Luccot Hill being due to unconformable overlap of Hangman upon Foreland rocks, pointing out that if such were the case conglomerates ought to be found in the Hangman series, and the junction should also be marked by discordant relations of dip and strike.

A Scrobicularia Bed, containing Human Bones, at Newton-Abbot, Devonshire, by W. Pengelly, F.R.S., F.G.S., &c.—Description of a bed of fine sandy mud, 10 feet thick, crowded with *Scrobicularia piperata*, recently discovered near the head of the tidal estuary of the River Teign, Devonshire. Its top is 1 foot above the level of the highest spring tides in the estuary, and its bottom 3 feet above the low-water level. Ten feet down in the bed were found the following human bones: a skull, part of the left superior maxilla, containing two teeth, a right femur, and a right scapula—all believed to be of the age of the deposition of that part of the bed in which they lay. From the presence of the *Scrobicularia* there is apparently no doubt that since the era of deposition the district has been upheaved not less than 14 feet, nor more than 27 feet, and that the time was in all probability that of the elevation of the raised beach of Hope's Nose, about seven miles south-east of the *Scrobicularia* bed.

On a Deep Boring for Water in the New Red Marls (Keuper Marls) near Birmingham, by W. Jerome Harrison, F.G.S.—Around Birmingham the Keuper sandstone is divided from the Keuper marls by a line of fault running from north-east to south-west, roughly along the line of the River Rea. West of this fault the Keuper sandstone occupies the surface, and yields an enormous and unfailing supply of pure water, the Birmingham Corporation alone pumping about eight million gallons daily from three deep wells in this formation. East of the line of fault the Keuper red marls form an undulating band from five to twelve miles in width, the towns and villages on which depend wholly on surface waters, or shallow wells in surface gravels, for their water-supply. As the Keuper sandstone undoubtedly underlies the Keuper marls throughout the whole or the greater part of this tract of East Warwickshire, it is not surprising that attempts have recently been made to reach its locked-up waters by means of deep borings. Some seven or eight years ago the Birmingham Corporation bored in Small-heath Park (the southern suburb of Birmingham) to a depth of 440 feet, entirely in Keuper marls. The object of this paper is to describe a boring made during the present year at King's Heath, three miles south of Birmingham, at the brewery of Messrs. Bates, in search of water, which is now 667 feet deep, and still in marls and shales. From comparisons with the Keuper marls of Staffordshire, &c., the thickness of the Keuper marls at King's Heath can hardly be more than 700 feet. It is to be hoped that the Keuper sandstone will be reached almost immediately, and that its water-bearing properties will be such as to satisfy the requirements of the district.

On an Accurate and Rapid Method of Estimating the Silica in an Igneous Rock, by J. H. Player, F.G.S., F.C.S.—This paper describes a method of estimating the silica in igneous rocks by (1) fusing the finely ground rock with a flux prepared by mixing carbonates of potash and soda and nitrate of potash; (2) disintegrating the glass so obtained by the action of strong nitric acid; (3) driving off nitric acid at a temperature just below 250°, thus rendering all silica insoluble; (4) treating with hydrochloric acid, to leave the silica with some impurity, for weighing after calcination; (5) separating the impurity by means of ammonium fluoride and weighing it.

Notes on some Sections in the Arenig Series of North Wales and the Lake District, by Prof. T. McKenny Hughes, M.A., F.G.S.—In this paper the author describes a number of sections which cross the Arenig series in different parts of England and Wales, and endeavours to explain some apparent discrepancies in what is generally a remarkably constant set of beds. He starts with the Portmadoc section, where he considers that the chief differences of opinion have arisen from mistakes in the explanation of the geological structure of the district, especially from the wrong identification of some grit bands on opposite sides of important faults. Following the series to the north he shows that, although they vary in thickness, the principal zones are still represented near Carnarvon; and, discussing the question of the unconformity of these beds on the Lower Cambrian, he points out that the Lower Cambrian rocks are seen to vary so much both in character and thickness within short distances in the neighbourhood of the existing outcrop of the Archæan that any argument founded upon their thinning-out or their different texture must be received with distrust in an area where they are known to have been deposited on the flanks of mountain-ranges of pre-Cambrian age. He then describes some localities in the Lake district where the occurrence of the same zones has been determined, and points out the difficulty of getting rid of such great thicknesses of deposits of fine mud as would be implied in the usual interpretation of those areas.

On the Rocky Mountains, with Special Reference to that part of the Range between the 49th Parallel and Head-waters of the Red Deer River, by George M. Dawson, D.S., F.G.S., &c., Assistant Director, Geological Survey of Canada.—The term "Rocky Mountains" is frequently applied in a loose way to the whole mountainous belt which borders the west side of the North American continent. This mountainous belt is, however, preferably called the Cordillera region, and includes a great number of mountain systems or ranges, which on the 40th parallel have a breadth of not less than 700 miles. Nearly coincident with the 49th parallel, however, a change in the general character of the Cordillera region occurs. It becomes comparatively strict and narrow, and runs to the 56th parallel or beyond with an average width of about 400 miles only. This portion of the western mountain region comprises the greater

part of the province of British Columbia. It consists of four main ranges, or, more correctly, systems of mountains, each including a number of component ranges. These mountain systems are, from east to west:—(1) The Rocky Mountains proper. (2) Mountains which may be classed together as the Gold Ranges. (3) The system of the Coast Ranges of British Columbia, sometimes improperly named the Cascade Range. (4) A mountain system which in its unsubmerged portions constitutes Vancouver and the Queen Charlotte Islands. The present paper refers to the Rocky Mountains proper. This system, between the 49th and 53rd parallels, has an average width of about sixty miles, which, in the vicinity of the Peace River, on the 56th parallel, decreases to about forty miles. It is bounded to the east by the Great Plains, which break into a series of foot-hills along its base; to the west by a remarkably straight and definite valley occupied by portions of the Columbia, Kootanie, and other rivers. Since the early part of the century the trade of the fur companies has traversed this range, chiefly by the Athabasca and Peace River Passes, but till the explorations effected by the expedition under Capt. Palliser in 1858-59, nothing was known in detail of the structure of the range. During the progress of the railway explorations a number of passes were examined, and in 1883 and 1884 that part of the range between the 49th parallel and latitude 51° 30′ was explored and mapped in some detail in connection with the work of the Canadian Geological Survey by the author and his assistants. Access to this, the southern portion of the Rocky Mountains within Canadian territory, being now readily obtained by the railway, its mineral and other resources are receiving attention, while the magnificent alpine scenery which it affords is beginning to attract the attention of tourists and other travellers. The results of the reconnaissance work so far accomplished are here presented in the form of a preliminary map, accompanied by descriptions of routes and passes, and remarks on the main orographic features of the range.

Surface Subsidence caused by Lateral Coal-Mining, by Prof. W. Benton, A.R.S.M.—A paper showing that a large amount of coal is annually sacrificed in British mining for the lateral support of neighbouring and disinterested surface proprietaries; pointing out the results of this sacrifice, and enumerating the considerations which should govern the extent of this support.

A New Form of Clinometer, by John Hopkinson, F.L.S., F.G.S.—A "day and night" compass-card is set to true north over the compass-needle, which necessarily points to magnetic north. The diameter of the card is less than the length of the needle, so that the points of the needle project beyond the card, and the correction made is seen and can be adjusted when required. The same result would be attained by placing the card below the needle. The clinometer dip is as usual below the magnetic needle, and can be easily seen outside the compass-card. The advantage of being able to take the amount and direction of the dip of strata with a single instrument without loss of time and liability to error in making the correction for magnetic deviation, and at the same time having the points of the compass exposed for more minute observations if required, must be obvious. The present deviation is 17° 50′ W. of N., and it is lessening. The instrument was exhibited.

Statistics of the Production and Value of Coal Raised within the British Empire, by Richard Meade, Mining Record Office.—This paper, prepared at the request of the Committee to accompany other papers on the Colonial coal resources, gave particulars of the quantity and value of coal raised for several years past, in many cases for ten years. We give here only the amount and value quoted for the latest year in each case:—

	Date	Quantity Tons	Value £
Queensland	1885 ...	209,500 ...	not given
New Zealand ...	1883 ...	408,831 ...	360,622
Victoria	1884 ...	not given ...	3,280
Natal	1883 ...	5,000 ...	1,000
India	1883 ...	1,315,976 ...	657,988
Cape of Good Hope	1884 ...	9,000 ...	7,250
Tasmania	1884 ...	7,194 ...	6,381
Canada	1884 ...	1,876,643 ...	619,336
United Kingdom	1885 ...	159,351,418 ...	41,139,408

On Canadian Examples of Supposed Fossil Algæ, by Sir William Dawson, LL.D., F.R.S.—Markings of various kinds on the surfaces of stratified rocks have been loosely referred to Algæ or Fucoids under a great variety of names; and when recently the attempt was made in Europe more critically to define and classify these objects, a great divergence of opinion developed itself, of which the recent memoirs of Nathorst, Williamson, Saporta, and Delgado may be taken as examples. The author, acting on a suggestion of Sir R. Owen, was enabled, in 1862 and 1864, by the study of the footprints of the recent *Limulus polyphemus*, to show that not merely the impressions known as *Protichnites* and *Climactichnites*, but also the supposed Fucoids of the genera *Rusophycus*, *Arthrophycus*, and *Cruziana* are really tracks of Crustacea, and not improbably of Trilobites and Limuloids ("On Footprints of Limulus," *Canadian Naturalist*, 1862; "On the Fossils of the Genus *Rusophycus*," *ibid.* 1864). He had subsequently applied similar explanations to a variety of other impressions found on Palæozoic rocks ("On Footprints and Impressions of Aquatic Animals," *American Journal of Science*). The object of the present paper was to illustrate, by a number of additional examples, the same conclusions, and especially to support the recent results of Nathorst and Williamson. *Rusichnites*, *Arthrichnites*, *Chrossochorda*, and *Cruziana*, with other forms of so-called *Bilobites*, are closely allied to each other, and are explicable by reference to the impressions left by the swimming and walking feet of Limulus, and by the burrows of that animal. They pass into *Protichnites* by such forms as the *P. Davisii* of Williamson, and *Saerichnites* of Billings, and *Diplichnites* of the author. They are connected with the worm tracks of the genus *Nereites* by specimens of *Arthrichnites*, in which the central furrow becomes obsolete, and by the genus *Gyrichnites* of Whiteaves (*Transactions* of the Royal Society of Canada, 1883). The tuberculated impressions known as *Phymatoderma* and *Caulerpites* may, as Zeiller has shown, be made by the burrowing of the mole-cricket, and fine examples occurring in the Clinton formation of Canada are probably the work of Crustacea. It is probable, however, that some of the later forms referred to these genera are really Algæ related to *Caulerpa*, or even branches of Conifers of the genus *Brachyphyllum*. *Nereites* and *Planulites* are tracks and burrows of worms, with or without marks of setæ, and some of the markings referred to *Palæochorda*, *Palæophycus*, and *Scolithus* have their places here. Many examples highly illustrative of the manner of formation of these impressions are afforded by Canadian rocks. Branching forms referred to *Licrophycus* of Billings, and some of those referred to *Buthotrephis*, Hall, as well as radiating markings referable to *Scotolithus*, *Gyrophyllites*, and *Asterophycus* are explained by the branching burrows of worms illustrated by Nathorst and the author. *Astropolithon*, of the Canadian Cambrian, seems to be something organic, but of what nature is uncertain. *Rhabdichnites* and *Eophyton* belong to impressions explicable by the trails of drifting sea-weeds, the tail-markings of Crustacea, and the ruts ploughed by bivalve mollusks. *Dendrophycus*, *Dictyolites*, some species of *Delesserites*, *Aristophycus*, and other branching and frond-like forms, were shown to be referable to rill-marks, of which many fine forms occur in the Carboniferous of Nova Scotia, and also on the recent mud-flats of the Bay of Fundy. The genus *Spirophyton*, properly so called, is certainly of vegetable origin, but many markings of water-action, fin-marks, &c., have been confounded with these so called "Canda-galli Fucoids." On the other hand, some species of *Palæophycus*, *Buthotrephis*, and *Sphenothallus* were shown to be true Algæ, by their forms and the evidence of organic matter, and *Haliserites*, *Barrandeina*, and *Nematophycus* were shown to include plants of much higher organisation than the Algæ. With reference to the latter, it was held that the form to which the name *Prototaxites* had been given was really a land plant growing on the borders of the sea, and producing seeds fitted for flotation. On the other hand, certain forms to which he had given the name *Nematoxylon* were allied to Algæ in their structure, and may have been of aquatic habit; very perfectly preserved specimens of these last had been recently found, and had thrown new light on their structure. The author proposed to apply to all these problematical plants, having a tissue of vertical and horizontal tubes, the general name *Nematophyteæ* or *Nematophyton*. The paper referred to the history of opinion on these objects and the bibliography of the subject; but this, as well as detailed descriptions, are omitted in this abstract.

Notes on some of the Problems now being Investigated by the Officers of the Geological Survey in the North of Ireland, chiefly in Co. Donegal, by Prof. E. Hull, LL.D., F.R.S.—The author stated that the investigations of the Survey were confined to the counties of Antrim and Donegal, and, restricting his observa-

tions to the latter, he said the problem was whether or not there were two great series of metamorphic rocks unconformable to each other, the older referable to the Archæan age, the newer to the Lower Silurian. Some reference was made to the great faults and foldings of these beds, which were stated to range generally in N.N.E. and S.S.W. lines. It was considered that the granites might belong to at least two periods—the intrusive being distinct both in age and structure from the metamorphic granite and gneiss. Other points noticed were the occurrence of numerous basaltic dykes, probably of Tertiary age, traversing the gneissose rocks; and marginal representatives of the Lower Carboniferous period.

On the Classification of the Carboniferous Limestone Series; Northumbrian Type, by Hugh Miller, F.R.S.E., F.G.S., of H.M. Geological Survey.—The object of this paper was to show that the classification proposed twenty years back by G. Tate of Alnwick is still sufficient, not only for North Northumberland, where Tate established it, but also for the south of the county. Prof. Lebour has proposed another classification on the assumption that Tate's divisions either do not exist in Nature, or do not persist throughout the county. Tate's classification, amplified in some not very important details, and adapted to the work of the Geological Survey, is as follows:—

		Feet
Upper Limestone Series	*Felltop or Upper Calcareous Division:—From the Millstone Grit to the zone of the Great Limestone.* Sandstones and shales; one or more beds of marine limestone, including the Felltop Limestone; some coals	350-1200
	Calcareous Division:—From the great Limestone to the bottom of the Dun or Redesdale Limestone. Many beds of good marine limestone; sandstones and shales; coals	1300-2500
Lower Limestone Series	*Carbonaceous Division (Scremerston Beds of North Northumberland:—From the Dun or Redesdale Limestone to Tate's "Tuedian Grits."* Strata prevalently carbonaceous; limestones chiefly thin, many of them containing vegetable matter; coals	800-2500
	Tuedian Division:—Upper Tuedian or Fell Sandstone Group, the "Tuedian Grits" of Tate:—From the Carbonaceous Group to the Cement-Limestones. Great belt of massive grits (Tweedmouth, Chillingham, the Simonside, and Harbottle Hills, the Peel, and Bewcastle Fells). Shales greenish and reddish as well as carbonaceous gray; coals rare, thin, or absent ...	500-1600
	Lower Tuedian or Cement-Limestone Group:—From the base of the Grits downwards. Cement-stone bands passing into limestones (Rothbury, Bewcastle); coals very rare; generally some coloration of the shales and sandstones	500-1500
	Basement Conglomerates (Upper Old Red Sandstone); local	—

Notes on the Crystalline Schists of Ireland, by Ch. Callaway, D.Sc., M.A., F.G.S.—The author gives a summary of results obtained by a preliminary survey of the principal areas of Irish metamorphic rocks in Donegal, Connemara, and the south-eastern corner of the county of Wexford. In each of these areas the following facts were observed:—(*a*) A series of hypometamorphic rocks, consisting typically of fine-grained schists, altered grits, and quartzites. A clastic structure is more or less distinct in the three areas, but is least evident in Connemara. (*b*) A group of highly crystalline schists, displaying no trace of an original sedimentary origin, dipping as if it passed below the hypometamorphic rocks. At Wexford there are true gneisses. In Connemara the rocks are less feldspathic, the chief types being quartzose gneiss, quartz-schist, mica-schist, hornblende-schist, quartzite, and crystalline limestone. This description will also apply to Donegal. (*c*) Granite, underlying (*b*), and in Connemara and Donegal clearly intrusive. The author urges that this analogy is not due to the metamorphic action of the granite; for—(1) The mineral characters apparent in the schists adjacent to the granite are uniformly distributed through the lower series from bottom to top. (2) The evidence collected is hostile to the view that this lower series ever graduates into the upper. It is concluded that the balance of proof is in favour of the Archæan age of the bulk of the Irish schists. (1) In the Wexford district the schists are thrown against Cambrian and Ordovician rocks by faults, and do not pass into them in the localities alleged by the Irish Survey. (2) In Connemara conglomerates of Llandovery age contain large rounded fragments, not only of the older schistose series, but also of its intrusive igneous rocks. (3) In the Ulster region the metamorphic area is separated from the Ordovician rocks of Pomeroy by a ridge of granite and diorite three miles in breadth. The lithological analogies between the Irish schists and the Archæan rocks of Anglesey and other British metamorphic districts are also of weight in the argument.

SOCIETIES AND ACADEMIES

Edinburgh

Royal Society, July 19.—The sense of taste, by John B. Haycraft. Sensation or feeling is a result of the operations of the external world upon our sentient bodies. A vibration of light, a sonorous wave, a molecule of sugar or of musk stimulates the appropriate nerve through the mediation of a little sensitive cellule in the eye, the ear, the tongue, or the nose. A motion called a *nerve motion* is then set up, passes to the brain, and if this organ is in a state of activity we are conscious of a feeling or sensation. In the case of sound and light the character of the vibration determines the quality of the sensation produced. Thus, a certain complex vibration of light produces a sensation we call crimson, a certain complex vibration of sound we recognise as coming from a violin-string. Motion is thus transmitted into a nerve motion or impulse, which gives rise to a sensation. Of the thousand qualities of sensation all have a counterpart in the thousand variations of motion outside the body. The physiologist knows little more about the production of the sense of taste than those facts which are the intellectual property of every one. The object of the author of the paper of which this is a short abstract is to show that taste in its method of production is precisely analogous to sight and hearing. The truth of this is indicated by the striking similarity in structure between the end-organs of all the special senses, which are all developed from primitive ectodermic cells, of much simpler form. Spectroscopic investigation has demonstrated, too, that the sapid and odorous molecules vibrate constantly and in a manner characteristic of each substance. We have, then, in the case of taste (and it is hoped subsequently to demonstrate this in the case of smell as well), vibrating matter and a sensitive end-organ, conditions analogous with those present in the other senses. If it can be shown that substances vibrating in the same manner produce the same taste, the analogy will be complete. It has been found by Newlands and others that if the elements be arranged in a series, starting with that metal which has the lowest, and passing up to that which has the highest, atomic weight, a periodic recurrence of chemical and physical properties is observed. Thus lithium, the second in the series, is similar to sodium, the ninth, and potassium, the sixteenth, and so on. This is called the periodic law. The author finds that there is also a periodicity as regards taste production. Thus the chlorides or sulphates of a series of similar elements—called a group of elements by Mendelejeff—have similar tastes. It is curious, however, that the taste changes slightly but uniformly as we pass to the higher members of a group. Thus the chlorides of lithium and sodium are salt, but as you pass to the higher members of the group the taste becomes more saline and very slightly bitter. Now Prof. Carnelley has recently discovered that compounds containing elements of the same group have similar colours, the colour changing, however, uniformly—passing to the red end of the spectrum—as we reach the higher members of a group. Colour is periodic. But this indicates that the elements of the same group are vibrating in a similar way. If the lower member be yellow from absorption of the blue, the next one will have vibrations of nearly the same pitch, being in reality at a somewhat slower rate of vibration, and absorbing rays nearer the red end. Here, then, is the analogy sought for. A group of salts of similar chemical properties have their molecules in a

similar vibrating condition, giving rise to similar colours and similar tastes. A study of the carbon compounds yields as conclusive evidence. The alcohol bodies, such as mannite, grape-sugar, glycerine, glycol, are sweet. They possess a certain common molecular structure and a compound radical, $CH_2.OH$. Associated with this radical is the taste called sweet, just as are associated with it many chemical and physical properties. Common alcohol is tasteless, but it is monatomic, all the polyatomic alcohols having a sweet taste. The organic acids, too, have a radical, CO.OH, with which seems to be associated their acid properties and the power of producing a special taste. Now it is certain that compound radicals, like elementary substances, vibrate in a definite way, however they are combined. A coloured acid like chromic and picric acid forms a class of coloured salts. Ammonia viewed in quantity shows characteristic absorption-bands; replace an atom of hydrogen by ethyl or methyl, and the same bands are to be observed, shifted, however, slightly towards the red end of the spectrum. We see, then, in the carbon compound the radical vibrates, modifies light passing through it in a definite way, and affects the sensorium by causing the production of a definite sensation of colour. So too it can produce a definite taste sensation. I do not hazard an opinion as to how the molecule stimulates the end-organs in the tongue. Too little is known about the stimulation of the retina by light. Whether or not in both cases it is mechanical, one cannot say. As to its being chemical action, it may well be asked, What is this? Chemical action itself may perhaps be most satisfactorily interpreted by the use of a mechanical hypothesis. Much has yet to be discovered as to the exact relationship between vibration and taste sensation. That this relationship exists, is all the author wishes to prove. When spectroscopic investigation of the invisible spectrum is more advanced, what Helmholtz has done for sound may also be done for taste, and we may know the exact vibrational counterpart of a taste quality as we know it already of the sound of a violin-string.

PARIS

Academy of Sciences, September 13.—M. Émile Blanchard in the chair.—Experiments on the electrical conductibility of gases and vapours, by M Jean Luvini. A series of experiments are described, which have led the author to the general conclusion that, under all pressures and at all temperatures, gases and vapours are perfect insulators, and that they cannot be electrified by friction either with themselves or with solid or liquid bodies. Crucial tests were applied to air saturated with the vapour of water at temperatures ranging from 16° to 100° C.; to hydrogen and carbonic acid not dried, but just as they left the bath generating them; to the vapour of mercury at 100° C.; the vapours of sal ammoniac; air heated by live embers or the flame of a candle; the fumes of sugar, camomile, incense, &c., none of which vapours gave the least indication of conductibility. Hence to suppose, as is generally done, that very rarefied gases, or gases at very high temperatures, are conductors, is a mistake due to confusion between resistance to disruptive and conductive discharges. Thus Masson has shown that at like potential the distance of the disruptive discharge in the air is twelve or thirteen times greater than in water, which simply means that the resistance of water to the disruptive discharge is greater than that of air, not that air is a better conductor than water. Henceforth physicists will have to reject all theories regarding the electricity of machines, the air, or clouds, in which moist air is assumed to be a conductor, or in which gases and vapours are supposed capable of being electrified by friction.—Quantitative analysis of the dry extract of wines, by M. E. Bouilhon. In order to shorten the ordinary tedious process, some chemists separate the liquids by means of porous bodies increasing the surface of evaporation. But this method leads to fallacious results, numerous experiments showing that all increase of the surface lowers the weight of the residuum to a very considerable extent, in consequence of the evaporation of a portion of the glycerine. Thus a litre of claret yielded 22·4, 22·0, and 21·2 grammes of sediment according to the various forms and sizes of the vessels employed in the process.—On *Fecampia erythrocephala*, a new species of Rhabdocœle, parasitic and nidulating, by M. A. Giard. This species, which is very common on the coasts of Fécamp and Yport, is shown to differ considerably from Graffilla and the different genera of Rhabdocœle hitherto described. It appears greatly to resemble a parasite discovered by Lang in the foot of *Tethys fimbriata*, and a more complete study of this Mediterranean type will no doubt show that, like the parasite here described, it also secretes a cocoon.—Researches on the circulatory apparatus of the Ophiures, by M. R. Kœhler. The circulatory system of these organisms, as here described, appears to be very analogous to that of the Echinidæ, as already revealed by previous investigations of the author and M. Prouho. Both groups present the same structure of the madreporic gland, the same relations of this gland, on the one hand with the periphery, on the other with a peribuccal ring; two peribuccal rings throwing off two branches in the same directions; lastly, the absence of aboral circle.—On the heart, digestive tube, and reproductive apparatus of *Amaræcium torquatum* (a Compound Ascidian), by M. Charles Maurice. In this paper the author determines the true physiological functions of some of the organs already observed by Seeliger, Von Drasche, and Della Valle in other species of Ascidians.—On the annual movement of the barometer in European Russia, by General Alexis de Tillo. While the yearly oscillations of the barometer in Siberia may be figured by a curve of somewhat simple type, those of European Russia are shown to be of a much more complicated character. From the numerous records published by the St. Petersburg Central Physical Observatory, the author has deduced the mean monthly readings for eighty meteorological stations in this region, and these data have enabled him to determine the mean type of the annual barometrical curve for the centre of European Russia. As it advances eastward in the direction of Siberia and Central Asia, this curve loses its secondary maxima and minima, while on the other hand its amplitude increases gradually.

BOOKS AND PAMPHLETS RECEIVED

"Marion's Practical Guide to Photography," new edition (Marion and Co.).—"Die Angiospermen des Bernsteins," Zweiter Band, by Dr. H. Conwentz (Danzig).—"Verhöffentlichungen der Grossherzoglichen Sternwarte zu Karlsruhe," Zweiter Heft, by Dr. W. Valentiner (Karlsruhe).—"Proceedings of the Royal Society of Queensland, 1885," vol. ii. parts 1 and 2 (Brisbane).—"Boston University Year-Book," vol. xiii.—"Results of Rain and River Observations made in New South Wales during 1885," by H. C. Russell (Sydney).—"Letters on Sport in Eastern Bengal," by F. B. Simson (Porter).—"Nyt Magazin for Naturvidenskaberne," 12 parts (Christiania).—"Mountaineering below the Snow-Line," by M. Paterson (Redway).

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THURSDAY, SEPTEMBER 30, 1886

OUR GUNS

WHEN a careful engineer sets about designing a structure, he first determines the strain which the several parts of it will have to withstand; he then selects his material and proportions it so that it will be able to carry the strains safely; in determining the margin which should be allowed he uses judgment based upon his own experience or that of others who have designed similar structures; and if the strains be difficult to determine, or if they be of the nature of severe and sudden shocks and complicated cross strains he increases his margin in proportion. Experience has shown, for example, that wrought iron in the form of a railway bridge may be worked safely at a load equal to one-fourth of that which will break it down, a cast-iron bridge to one-sixth. The builder of a steam-engine rarely loads those parts of his machinery which have to endure sudden and reverse strains to more than one-eighth or one-tenth of their ultimate strength. If his structure fail, the first thing the engineer does is to re-calculate his strains and the dimensions he has given to the various parts, and if these should prove correct he seeks for the cause of failure in unlooked-for defects in his material; and if failure in the same class of structure, of various sizes, recur repeatedly in the same place, he comes to the conclusion, either that he has under-estimated the strains, or that the margin of safety which he has allowed is not sufficient. An engineer accustomed to act in this manner must look with dismay upon the report of the Committee appointed to examine into the cause of the failure of the 12-inch gun of the *Collingwood*, and of other guns of similar construction It is possible, of course, that the Committee have been able to calculate the strains which tend to destroy the guns, and have satisfied themselves that sufficient metal has been provided for the purpose; but, if so, it is much to be regretted that they have not seen fit to make the results of their investigations public, because it would have been instructive to know how the stresses are arrived at and what margin of safety is considered sufficient for a gun. When Colonel Maitland read his paper on our new guns at the United Service Institution in the middle of 1884, the strains certainly were not known to the Ordnance Committee, because the pressure curves, purporting to represent one-fourth of the bursting pressure of the guns, and which were given on the official drawings, were incompetent to account even for the muzzle energy which the shot was supposed to possess, and consequently provided nothing whatever for other important work which has to be performed during the discharge. This fact was pointed out by the *Engineer* early in 1885, in an article commenting on one of the Howard Lectures which had just been delivered at the Society of Arts, and the weak point in our guns was actually indicated before any failures had taken place!

How much has the knowledge of the Ordnance Committee advanced in the meantime, and is four still considered a sufficient factor of safety? Any careful engineer who reflects on the strains a gun is subjected to would certainly class it among steam-engines, subject to the roughest work, and not to the class of railway bridges, which have well-defined and simple strains to stand. What takes place when a gun is discharged? First, there is the direct pressure of the gases, which is still very imperfectly known. Next, this pressure travels along the bore at a very rapid rate, producing a shearing strain between the material in advance of, and in rear of the base of the shot, and this strain is intensified at each point where a sudden change of thickness takes place. Thirdly, there is the reaction to the force producing rotation of the shot, which tends to twist the barrel. Fourthly, there are the strains produced by the momentary presence of a white-hot body pressing against the walls of the bore with a pressure ranging from 25 tons to 1 ton per square inch; and, lastly, there is the longitudinal strain representing the reaction between the pressure on the base of the bore and the inertia of the gun itself. What engineer would dream of counteracting such strains as these with a less margin than eight or ten, if he were perfectly unfettered, and if he were certain of the manner in which the strains were transmitted through the metal of the gun; but even on this point there are grave doubts, for it seems almost certain that the strains travel as pulses or vibrations, in a manner which sets at defiance all ordinary modes of calculation. No engineer would be surprised if guns, with a factor of safety of only four, burst frequently.

But it will, no doubt, be argued that guns of the strength suggested would be impracticable. We do not hold that opinion, because weight in a gun, even for naval purposes, is not objectionable, since the force of recoil diminishes with the increase of weight, and the metal appropriated to the gun would be saved in the carriage and structure of the ship; while, for land service, weight can be no objection whatever. The Committee make no allusion to a necessity of keeping down the weight of guns, hence it must be supposed that they would not sacrifice safety to this end, although it is well known that there is a kind of race among gun-makers to produce the greatest amount of shot-energy per ton of gun. It seems to us, therefore, most unfair that the whole blame of our failures is to rest on the Royal Gun Factory, that is, on the quality of material and on the manufacture, when it is certain that the faults are faults of design—a want of sufficient metal—a view which the Committee adopt by their acts, though not by their words; for they are adding largely to the weight of the guns in the very parts which experience and the investigations of outsiders have proved to be deficient in strength.

But although the Ordnance Committee are reticent as to the scientific views which they hold respecting the structure of guns, they have a way of taking the public into their confidence, through the instrumentality of a lecture delivered by one of their body, whose talents as an agreeable expounder of popular science are well known, and they show much wisdom in making these manifestations take the form of lectures instead of papers read before scientific Societies. The advantage of a lecture is that no awkward questions can be asked, and no fallacies or errors pointed out. Thus, when the *Thunderer's* gun burst, Sir Frederick Bramwell delivered a very pleasant lecture at the Royal Institution, and the other day the same gentleman selected the subject of our guns as the

theme of his address to the Midland Institute at Birmingham, and, we have no doubt, charmed his audience with his manner and ready wit. But what is there in either of the lectures just mentioned that throws the slightest light on the real difficulties of the subject? Why was not Sir F. Bramwell moved to read a paper at the meeting of the British Association which took place a few weeks ago in the very building in which he addressed the Midland Institute? Had such a paper been read, a most interesting and animated discussion would have arisen, and the nation would have had the advantage of learning the opinions of men who have devoted their lives to working out the problems connected with the theory as well as the practice of the construction of ordnance. We are driven to the conclusion that the Ordnance Committee considered that such a course would have been dangerous to their self-respect; the deplorable ignorance which characterises all attempts at working out difficult questions by Committees would have been too glaringly exposed; it was much safer not to subject themselves to cross-examination.

If we are wrong, if the Committee have complete answers to the questions raised, it is open to them to convince the nation of the fact, because the sessions of the Institution of Civil Engineers will very soon commence, and some member of the Ordnance Committee should be deputed to read, not a popular lecture, but a serious paper that will demonstrate to a roomful of practical engineers that our guns have been constructed according to the rules which guide engineers in their ordinary work, and that yet they have failed. We have small hopes that the challenge will be accepted.

THE MATHEMATICAL AND PHYSICAL SCIENCES

Histoire des Sciences Mathématiques et Physiques. Par M. Maximilien Marie. Tomes I.-IX. (Paris: Gauthier-Villars, 1883-86.)

M. MARIE'S great work advances rapidly towards completion. The first three volumes appeared in 1883; the concluding three are in the press; we have now before us nine volumes, bringing down the narrative from the time of Thales to the time of Laplace. The undertaking is a vast one; and we are not surprised to hear that it has cost forty years of preparation. The learned world is to be congratulated that it has fallen into such able hands. M. Marie combines, in a rare degree, scientific with literary qualifications. A certain grace and poignancy of style set off his wide erudition and practical acquaintance with methods of teaching. He can be vivacious even over processes of integration. The accumulated mass of his materials nowhere hinders the lightness of his tread. Keen touches of sarcasm enliven his most abstruse expositions, and agreeably remind his readers that a sense of humour may subsist concurrently with a thorough mastery of the higher analysis.

He has accordingly produced a book which, with these merits and some corresponding defects, only a Frenchman could have written—one eminently interesting and original, at once lively and profound, instructive throughout if occasionally one-sided, frankly displaying the prepossessions of its author, and not unfrequently—as we shall presently show—his heedlessness of historical and biographical accuracy. Its characteristic merit consists in the lucid interpretations contained in it of the older methods of mathematical research. The works of ancient and mediæval geometers are analysed, not barely in the view of exhibiting the results attained by them, but with the further purpose, most completely realised, of rendering their various artifices and modes of working intelligible to the least skilled in the archæology of mathematics. M. Marie's is indeed in no sense a book for beginners. It presupposes a considerable acquaintance with the most recent developments of analysis. The reader thus provided may, however, follow with ease and pleasure the steps by which earlier inquirers advanced; he may enter into their conceptions, place himself at their precise point of view, and while marvelling at the ingenuity which carried them so far, study the limitations of thought which hindered them from proceeding any farther. He may learn how the singular deficiency in the idea of abstract number which hampered the workings of such luminous and powerful minds as those of Archimedes, Apollonius, and Euclid, was supplied from the far East; how Hindu algebra and arithmetic formed the complement of Greek geometry; how both were transmitted through Arabic channels to Italy, and together constituted the starting-point of modern discoveries. Nor is it less curious to watch the gradual emergence of ideas big with the progress of the future, such as those of negative and imaginary, or infinitely small quantities; how they presented themselves with hesitation, and were at first shunned and distrusted; how they grew bolder and insisted on recognition; how their tentative and partial treatment became widened and generalised until they finally developed the whole extent of their capabilities.

It is well known that Archimedes gave the first approximation to the value of π; but the occasion and significance of the step are often lost sight of. It marked, with the almost simultaneous attempt of Aristarchus of Samos to measure the relative distances from the earth of the sun and moon, the introduction of numerical calculation into theoretical researches (Marie, t. i. p. 59). The novel effort was prompted, in each case, by the interest of a special problem. Archimedes, naïvely enough, sought to prove that the idea of infinity had its root in enumerative impotence, and could be abolished by expanding the resources of arithmetic. He exemplified his contention by computing the number of grains of sand contained in a sphere with the interval from earth to sun for its radius. But a preliminary valuation of the ratio between the circumference of a circle and its diameter was indispensable; and the tract on the "Dimension of the Circle" was accordingly, in M. Marie's plausible view, written as a kind of preface to the "Arenarius." Incidentally to the calculations in the latter treatise, he perfected the Greek system of numeration, and foreshadowed the principle of logarithms.

M. Marie has ventured a kind of restoration of the "algebra" of Archimedes (t. i. p. 262). His remarks on this disputed subject are of great interest. He holds it impossible that his inventions should have been reached by the arduous path of his demonstrations; and ascribes to him, accordingly, the possession of a compendious method of reasoning founded on the transformation of

ratios, conducting easily to results otherwise unattainable even by his transcendent genius. The secrecy observed regarding it may have been due, in part to the difficulty of setting it forth in the absence of a suitable algorithm, in part to the conventional prevalence of the synthetic mode of exposition. The art was doubtless held in common with Apollonius, and other geometers of the time; but was treated as a mere rude tool, not worth the labour of bringing to a higher perfection. Had this early analysis been independently cultivated, and set free from its servitude to geometry, the "method of exhaustions" must, by its aid, in our author's opinion, have given birth to the Calculus eighteen hundred years before Cavalieri thought of his "infinitesimals."

The work before us aims, above all, at developing, as a coherent whole, the logical sequence of ideas. It would be unfair to say that this aim has been missed; yet we cannot help thinking that it might have been more perfectly attained. A vivid light, it is true, is frequently thrown upon obscure passages of research, and the filiation and significance of discoveries are, here and there, brought out with uncommon sagacity. Nevertheless, there is something wanting which M. Marie could easily have supplied.

A somewhat fragmentary plan has governed the composition of the book. The twenty-four centuries embraced by it are divided into sixteen periods of very unequal length, treated each in a section apart, consisting of a prefatory sketch of the progress accomplished during its course, followed by a series of biographies of those who contributed towards it, arranged in strict chronological order. Chemists and mathematicians, astronomers and botanists, mechanicians and physiologists, are thus placed side by side, with no closer tie of connection than the successive occurrence of the years of their birth. We have no sooner done with Lagrange's Calculus of Functions than we are confronted with Watt's transformation of Newcomen's steam-engine; the convulsions of Galvani's memorable frog succeed; and we pass thence to Parmentier's triumphant growth of potatoes on the plain of the Sablons,—all subjects of great interest, and treated with singular charm. Yet their variety, if it form, in a certain sense, an attraction, demands a stronger bond of unity than is here afforded. The true historical element, in short, is deficient. Nor is the want satisfactorily supplied by M. Marie's sixteen prefaces. We should be sorry to lose them; but they do not suffice. The absence of a connected narrative is still sensibly felt. Even if its design had otherwise remained unchanged, the book might at least have been provided with a general introduction, delineating and characterising the course of events to be detailed, pointing out the confluence, at epochs of discovery, of various and distant streams of thought, and presenting, in one luminous view, the progress in exact knowledge made by our race from the first dawn of civilisation until now. Perhaps it may not even yet be too late to add to a most valuable work a supplementary volume which would go far towards rendering it complete.

In another direction, M. Marie has perhaps unduly extended the scope of his enterprise. To have treated adequately the history of *all* the sciences, natural as well as mathematical, would have demanded, not a dozen, but fifty volumes. Yet all are nominally included in his scheme, while, in point of fact, those branches of knowledge remote from his principal theme receive only the casual attention of some stray jottings, with biographical notices of their leading promoters.

His choice of representative names, too, is open to criticism. Among omissions, that of Adelard of Bath, the first translator of the "Elements of Euclid" from the Arabic, is very remarkable. He was one of the most effective popularisers of Arab science in the thirteenth century, and played no unimportant part in the revival of mathematical learning. Yet he is not only ignored by M. Marie, but his version of Euclid is handed over to Campanus of Novara, with whose commentary it was published at Venice in 1482, and who has in consequence frequently gained the credit of its execution.

Nor should the unfortunate Cecco d'Ascoli have been altogether forgotten. His "Acerba," if not all that it has been claimed to be, contains many striking intuitions of natural truths. M. Marie, however, takes little interest in the premonitory symptoms of discovery; and the rage for unearthing its obscure anticipations has possibly been carried a little too far. We miss, further, the name of Giambattista della Porta, the *effective* inventor of the camera obscura, whose "Magia Naturalis" was of European fame and influence. And William Cullen, the founder of rational chemistry in Great Britain, was at least as well worthy of notice as Kunckel, known in connection with the manufactures of ruby glass and of phosphorus, to whom just three times the space is allotted as to Black, the discoverer of "fixed air," and of "latent heat."

A crowd of superfluous names, on the other hand, might be cited. It seems ungracious to object to the presence of a sketch so interesting in itself as that of the career of Ambroise Paré; yet it is not easy to see what the treatment of gunshot wounds has to do with the history of mathematical or physical science. Equally outside the proper scope of its cognisance are Henkel's improvement of Dresden china, Bosc d'Antic's contributions to the art of glass-making, Perronnet's bridges, Trudaine's "superb" highways, and Vaucanson's automata. If these give a valid title to admittance, why exclude Hargraves, Arkwright, Smeaton, MacAdam, and a host besides? Why should the spinning-jenny be passed over in silence, when Loriot's ribbon-loom comes in for honourable mention? In truth, however, industrial and mechanical inventions belong elsewhere.

It remains that we should justify our hint that M. Marie's facts and dates occasionally stand in need of revision. A perusal of Prowe's "Life of Copernicus" would have obviated several inaccuracies in his brief account of the reformer of astronomy. The intention of Copernicus to embrace the ecclesiastical state was not superseded, even momentarily, by his journey to Italy; on the contrary, he received his appointment as Canon of Frauenburg in 1497, shortly after he had entered the University—not of Padua, as stated by M. Marie, but of Bologna. Nor was he ordained a priest at Cracow in 1501. He took minor orders on entering the Chapter, but never became a priest; and his sojourn in Italy was unbroken between 1501 and 1505. Moreover, his doctoral degree (not in medicine, but in canon law)

was conferred at Ferrara, May 31, 1503. He sought no diploma at Padua, though he studied medicine there during the four years of his *second* stay in Italy. The assertion that at the age of twenty-seven he was summoned to profess mathematics at Rome is inexact. Uninvited, so far as is known, he repaired thither with his brother early in the year of jubilee (1500); and delivered unofficially some brilliantly-attended lectures during the ensuing winter. Finally, he settled down to his life's work at Frauenburg, not in 1510, but in 1512.

One rubs one's eyes in amazement to find Basil Montagu's discredited and superseded edition quoted as the best and completest of Bacon's Works. Is it really possible that the news of the late Mr. Spedding's labours has not yet reached Chantilly? It would appear not; for his name is unmentioned by M. Marie, who equally overlooks Mr. Fowler's instructive edition of the "Novum Organum."

In the date assigned for Robert Hooke's death, he copies an error of Poggendorff's, who states that he died at the age of eighty-seven, whereas he did not live to complete his sixty-eighth year. The substitution of 1722 for 1703 is of more than simply formal importance, since the publication of Newton's "Optics" and his acceptance of the Presidentship of the Royal Society, both depended upon the event thus post-dated. Newton loathed controversies, although drawn into many. But while Hooke lived, they could only be avoided by self-effacement; and this was accordingly the policy adopted, as far as possible, by his great rival.

M. Marie makes no secret of his aversion to the sour little professor of Gresham College; and it is too true that his character repels sympathy, while his achievements were not of the dazzling sort to blind men to his failings. Still, his claim to due recognition remains intact, although ignored by our author, who states openly that only Poggendorff's eulogies, by "forcing his hand," frustrated his intention of punishing his egotism with neglect. Yet Hooke, by Newton's express admission, discovered independently the law of inverse squares, and it is not too much to say that, but for his incitements, Newton would not have undertaken the investigation which led to his immortal discoveries.

M. Marie's grudge against Hooke does not seriously detract from the value of his work; but it is otherwise with his ill-will towards Newton. Not only is he avowedly the partisan of Leibnitz in the never-ending debate concerning the invention of the Calculus; but his dislike (not wholly unjustifiable) to Newton's conduct in the matter extends to all the processes of his mind. He compels himself, it is true, to utter a few obligatory words in acknowledgment of the greatness of his work; but its entire significance seems to escape him. His readers are only quite casually reminded that the discovery of the system of the world was of greater moment in the history of science than the solution of the problems of the brachystochrone, or of the centre of oscillation.

We are told by M. Marie that Newton represented his University in Parliament from 1688 until 1705, during which time he was assiduous in his attendance, but spoke once only, to request the usher to shut a window (t. v. p. 170). In fact he sat three (1689-90, and again 1703-5), not seventeen, years. At p. 162 of the same volume we meet the surprising statement that Halley predicted for 1682 the return of the comet observed by Kepler in 1607. It is almost superfluous to remark that its appearance in 1682 was as unlooked-for as that of any of its predecessors, that its periodical character was then first divined, and in 1759 verified.

The observation made by Wilcke in 1787 that the auroral corona forms in the magnetic zenith is attributed by our author to De Mairan in 1747. The eminent Academician, we can assure him, would have been the last to welcome a remark so subversive of the arguments by which he sought to efface the magnetic character of the phenomenon. Neither is it the case that Halley noticed the bisection of the auroral arch by the magnetic meridian. Obvious though the coincidence appear, it was first pointed out by Ussher in 1788.

We note further M. Marie's curious incredulity as to the authentic measurement, so far, of the parallax of any single star; his statements that the accepted value of the solar parallax is 8″·5 (t. ix. p. 43), that the mass of the moon is $\frac{1}{88}$ that of the earth, and that Herschel detected interstitial movements in resolvable nebulæ (t. ix. p. 145); finally, his ascription to Sir *William* Herschel of the translation of Lacroix' "Differential Calculus" executed by his son conjointly with Babbage and Peacock.

These and other similar blemishes, however, are very far outweighed by the merits of the work in which they occur. It is one of marked individuality; and individuality lends interest, if it sometimes begets defects. No student of the higher mathematics should leave it unread; its perusal cannot fail to afford pleasure, as well as to widen comprehension of modern methods by their comparison with those they have succeeded, and by the intelligent survey of their growth in the past.

CHEMISTRY OF WHEAT, FLOUR, AND BREAD

Chemistry of Wheat, Flour, and Bread, and Technology of Bread-making. By William Jago. (Brighton: Published by the Author, 1886.)

THIS bulky volume professes to treat of its subjects in an exhaustive manner. Wheat, flour, and bread-making are as important as they are universal; and if they are common-place, their study requires deep insight into chemical science. Mr. Jago's book will form a valuable addition to economic science. The composition of wheats from all parts of the world, the minute structure of the grain, the composition of milling products, and the processes of panary fermentation, fall properly within the limits of such a work, and are dealt with in an exceedingly painstaking manner. The various methods of bread-making, the chemistry of the art, and the effects of blending different descriptions of wheat so as to secure the best possible results, are amply and ably discussed. Modern milling and baking appliances are also described carefully and illustrated graphically. There is likewise enough of the author's own thought and research to redeem the work from the stigma of being mere compilation. The book is decidedly useful, and, making due allowance for the progressive state of our knowledge upon many of the topics dealt with, it will probably be received as a standard work. It brings within its ample limits a vast

amount of information which has usually, and we think appropriately, been treated of by separate authorities. The book is, in fact, a sort of encyclopædia of bread-making, and this being the case, it is open to the faults of such works. The design or scope is too large, and the matter introduced to our notice is often too remotely relevant to the immediate wants of the reader. A definition of chemistry, a table of atomic weights, an explanation of chemical equations, atoms and molecules, are scarcely necessary in this connection. Similarly, we cannot approve of lessons upon polarisation of light, the uses of the microscope, and the *camera lucida* being introduced *in extenso* into a book specially treating of a technical subject like this. Such knowledge ought to be assumed as already possessed by the reader; and as well might the author have given instruction upon the origin and uses of decimal fractions, or led up to his subject by several preliminary volumes dealing with the whole "circle of the sciences." Certainly he lays himself open to the charge of instructing either too much or too little. He deals with abstract scientific problems lying at the root of chemistry, and with the vulgar processes of the cook and the baker; and treats with equal facility of the microscope and the flour-mill. We had rather leave the minuter criticism of this voluminous work to the many experts whom it concerns, and who will no doubt be ready to detect any errors into which the indefatigable and talented author may have fallen. If Mr. Jago is ever tempted to bring out a second edition, we may recommend the use of the pruning-knife, which, if judiciously and freely used, will leave a better proportioned but less bulky treatise in our hands.

JOHN WRIGHTSON

OUR BOOK SHELF

American Journal of Mathematics, vol. viii. No. 3. (Baltimore, July 1886.)

IN her note on "the binomial equation $x^p - 1 = 0$," Miss Scott gives a somewhat simpler form of the equation for quartisection than that given by Prof. Cayley (*L.Math. S.Proc.*, vol. xi. pp. 11-14), and works out the equation for quinquisection on apparently different lines from Mr. F. S. Carey's solution (cf. Prof. Cayley, *L.Math.S.Proc.*, vol. xii. and vol. xvi.). Mr. F. N. Cole furnishes "A Contribution to the Theory of the General Equation of the Sixth Degree," which is interesting from the historical details which he gives. He acknowledges his great indebtedness to Klein, but there is a good deal of original work in the note. Mr. J. C. Fields gives a neat "proof of the elliptic-function addition-theorem." The *pièce de résistance* of the number is, however, the long-looked-for notes of "Lectures on the Theory of Reciprocants," by Prof. Sylvester. These are designed as "a practical introduction to an enlarged theory of algebraical forms, and, as such, are not constructed with the rigorous adhesion to logical order which might be properly expected in a systematic treatise. The object of the lecturer was to rouse an interest in the subject, and in pursuit of this end he has not hesitated to state many results, by way of anticipation, which might, with stricter regard to method, have followed at a later point in the course." The lectures, which are ten in number, have been reported by Mr. J. Hammond. The subject, which, it will be remembered, was fully brought before our readers by Prof. Sylvester's inaugural lecture, printed *in extenso* in NATURE (vol. xxxiii. pp. 222-231), has already attracted many of our younger mathematicians, so that there is hope of the Professor's closing aspiration of creating "such a school of mathematics as might go some way at least to revive the old scientific renown of Oxford" being soon an accomplished fact.

The Non-Euclidian Geometry Vindicated: a Reply to Mr. Skey. (*Transactions* of Wellington Philosophical Society.)

A PAPER entitled "On the Simplest Continuous Manifoldness of Two Dimensions and of Finite Extent," by Mr. F. W. Frankland, was read before the London Mathematical Society, December 14, 1876 (*Proc.*, vol. viii. pp. 57-64), and was subsequently published in our columns (vol. xv. p. 515) This same paper, or one of similar character with identical title, appears to have given rise to a paper by Mr. Skey, which took the form of notes on Mr. Frankland's paper, and was read before the Wellington Philosophical Society on June 26, 1880 (published in the thirteenth volume of the *Transactions* of the New Zealand Institute). The pamphlet before us is an abridgment of a series of letters in which Mr. Frankland convincingly replies *seriatim* to the main points raised by Mr. Skey, and maintains his former ground by additional arguments.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Sense of Smell

I KNOW a person who has never been conscious of any odour from a bed of mignonette, and I know another person who has never been conscious of any odour from a bean-field. Both of these persons have the sense of smell acute and discriminating as regards other odours.

I know persons who cannot discover a difference between certain odours which are very different to ordinary persons. Then there are persons who are sickened by certain odours which usually give pleasure. A considerable number of persons seem to be altogether destitute of the sense of smell; and on the other hand there are a few who have the sense very strongly developed.

I am at present investigating peculiarities of this sense, and I shall feel obliged to any one who will furnish me with illustrations or examples—whatever their character may be—with such fullness and precision as will enable me to use them in a scientific inquiry.

ARTHUR MITCHELL

34, Drummond Place, Edinburgh, September 24

Palæolithic Implements in Cambridgeshire

FEW Palæolithic implements have, I believe, been discovered in Cambridgeshire, although they are abundant in the gravels of the neighbouring counties of Suffolk and Essex. It may therefore interest your readers to learn that three implements have lately been found near Kennet, on the surface of a field not far from the high-road from Newmarket to Thetford. Two of the implements are kite-shaped. One, of lustrous black flint, is acutely pointed, with sharp cutting edges, and has a part of the original crust of the flint left on one of the faces, which is less convex than the other. It has lost a portion of the butt-end, but is otherwise perfect. The third is a sharp-rimmed ovate implement, the surface of which is stained a deep ochreous colour. Portions of the original crust remain on the faces and base.

Two other implements of the pointed type have been found on the surface near Kennet, but are not in my possession.

In the winter of 1884-85 several implements and flakes were

found in a ballast-pit opened by the Great Eastern Railway Company by the side of the line, about a mile and a quarter from Kennet Station, in the county of Suffolk. A reference to the Geological Map of the Ordnance Survey, Sheet 51, N.E., will show that the gravel in which this pit was opened is an extension of that which underlies the site of the above *surface* discovery. The implements from the ballast-pit which have come into my possession are of the kite-shaped and ovate types. Some are water-worn and abraded, in others the angles and edges are as sharp as if made yesterday.

ARTHUR G. WRIGHT

Sign-Numbers in Use among the Masai

AMONG the numerous tribes of Central Africa the Masai are distinguished by their use of finger-signs to denote numbers. These notorious warriors rarely ever use language to indicate numbers without accompanying signs on the fingers, though very frequently the latter are employed without the former, especially in answering questions.

As by some inadvertency I omitted giving a list of these signs in "Through Masai-Land," and Mr. Johnston, in his book "Kilimanjaro," has followed suit, it may still be of some interest and value to anthropologists to learn what these are.

English	Masai	Sign
1	Nabo	First finger held out alone
2	Aré	First and second fingers held out and alternately moved backwards and forwards
3	Uni	Thumb and two first fingers placed tip to tip
4	Unghwani	First and second fingers laid on top of each other
5	Umiēt	Thumb placed between first and second fingers
6	Ilé	Thumb scratched over nail of second finger
7	Nabishana	No finger indication
8	Usiēt	Hand held open and vertical and moved up and down
9	Naūdo	Thumb and first finger form a circle by joining the tips
10	Tomon	First finger drawn over the nail of the thumb
11	Tomoni-obwo	Same sign as in 10 accompanied by that for 1. The same rule for the succeeding numbers
20	Tikitum	The hand closed and opened rapidly
21	Tikitum-o-nabo	The same as 20, but followed by the sign for 1
30	Othman	First finger held out and shaken by a circular movement of the wrist
40	Artum	The hand held open and vertical and shaken or agitated by a circular movement of the wrist; not up and down as in 8
50	Unum	Thumb placed between first and second fingers, and hand agitated as in 40
60	Tomoni-ilé	Nail of thumb scratched on nail of third finger
70	Tomoni-nabishana	No finger indication
80	Tomoni-usiēt	Same as in 8, but sign never employed alone
90	Tomoni-naūdo	Same as in 9, but words always employed along with sign
100	Ipé	The partially closed hand opened once or twice
200	Ipé-aré	

JOSEPH THOMSON

A GLACIAL PERIOD IN AUSTRALIA

A GREAT many theories have been put forward to explain the extensive glaciation which repeatedly covered Europe and North America with enormous ice-streams. The ingenuity displayed by those who dealt with the subject was well worthy of the importance which attaches to the solution of the problem. However plausible some of the theories propounded may be, still it seems premature to approach such a question until all the available evidence bearing on the subject has been brought together. The southern hemisphere has, up till very recently, revealed only a few, and not very important facts, regarding glaciation, and it is evident that glacier traces in that hemisphere must be of great importance to explain the cause of glaciation; whether we may suppose it to be cosmic or terrestrial. I think, therefore, that my discoveries of glacier traces in Australia may be of sufficient general scientific interest to warrant my giving a short account of them in this journal.

Dr. von Haast, in his excellent work on the "Geology of Canterbury and Westland (New Zealand)" gave a detailed account of the traces of an extensive glaciation in the Middle Island of New Zealand, together with a map, showing that at one time the glaciers on the western slopes of the Southern Alps in many places reached down to the sea, and that those which descended from their eastern flanks covered a large portion of the lowlands extending between the mountains and the coast-line.

During my exploration of the central part of the Southern Alps I observed numerous old moraines and *roches moutonnées* in the area which, according to von Haast's map, had once been covered by glaciers. Particularly was I struck with the freshness of the striæ, the scratches and grooves in the steep and rocky precipices on the sides of Milford Sound, that jewel of the Southern Alps.[1] Capt. Hutton, who examined some of the other sounds has not discovered any glacial traces there.[2]

Even now the glaciers in New Zealand reach down to 700 feet on the west, and to 2000 feet on the east side, which shows that New Zealand must be subject to a very different climate to that in similar latitudes—44°—in the northern hemisphere. Like Patagonia, New Zealand is at the present day to a certain extent *in a Glacial period*. The much greater extent of the prehistoric glaciers shows, however, that it is now by no means *at the height* of its glaciation.

Although a Glacial period was shown to have existed in New Zealand, there have not up to now been any definite statements regarding this subject in the mainland of Australia. The Rev. Tenison-Woods[3] examined certain rocks in the Blue Mountains, an insignificant table-land to the west of Sydney, and came to the conclusion that these, which had been supposed to indicate ice-action, did not do so, and that in fact there was no evidence of a Glacial period in the Blue Mountains. Mr. Howitt[4] came to a similar negative result regarding certain gravels and conglomerates, which according to others indicated glacial action. Griffiths,[5] on the other hand, claims these and other conglomerates of Omeo and Gippsland as evidences of a Glacial period in Australia.

Prof. Tate[6] described some striated rocks found near Adelaide, and Stirling[7] has shown that there exist extensive traces of glacier action in certain valleys near Omeo.

I myself have,[8] in several papers, published some of

[1] R. von Lendenfeld, "Der Tasmangletscher und seine Umgebung." Erganzungsheft No. 75 von *Petermann's geographischen Mittheilungen*. "The Time of the Glacial Period in New Zealand," *Proceedings* of the Linnean Society of N.S.W. for 1885.

[2] F. M. Hutton, *Proceedings* of the Linnean Society of N.S.W. for 1885.

[3] *Proceedings* of the Linnean Society of N.S.W., vol vii. p 382.

[4] *Quarterly Journal* of the Geological Society of London, vol. xxxv. p. 35.

[5] "Evidences of a Glacial Epoch in Victoria," *Proceedings* of the Royal Society of Victoria for 1884.

[6] Tate, Anniversary Address, *Transactions* of the Royal Society of South Australia, 1879-80.

[7] T. Stirling, "Notes on some Evidences of Glaciation in the Australian Alps," *Proceedings* of the Royal Society of Victoria for 1885.

[8] R. von Lendenfeld, "Official Report on the Exploration of Mount Kosciusco to the Government of New South Wales" (Sydney, 1885.) "The Glacial Period in Australia," *Proceedings* of the Linnean Society of New South Wales for 1885. "Report on an Exploration of Mount Bogong," *Proceedings* of the Royal Geographical Society of Victoria for 1886.

the results of my two explorations of the Australian Alps, and described numerous indubitable traces of prehistoric glaciers on Mount Kosciusco and on Mount Bogong.[1]

Mr. Brown, Government Geologist of South Australia, has furnished me with photographs of beautifully preserved striæ on rocks in the Mount Lofty group near Adelaide. Prof. Hutton [2] has taken objection to the conclusions arrived at by myself, and although he acknowledges a *Glacier* period in Australia, objects to its having been a *Glacial* period. If I now revise the facts stated by others, and compare them with my own observations, I hope I may be able to give some idea as to the time and extent of the Glacial period in Australia. Before entering on the subject, however, I must give an outline of the physiography of the Australian Alps.

The greater part of Australia is destitute of high mountains; only in the south-eastern corner we meet with greater elevations. Here a true Alpine chain is situated. These Australian Alps consist of numerous parallel chains extending from south to north, which are curved in such a way as to advance with their convex sides eastward. The Australian Alps are very old; only Palæozoic formations participate in the folding which runs parallel to the extent of the ridges. The predominant rocks on the surface are gneiss-granite and Silurian, which appears generally in the facies of brown slate. Devonian limestones and slates are found; they are, however, not common and discordant to the underlying Silurian. The stratified rocks are highly folded, and usually with a very large dip or vertical. The Silurian appears on the surface in elongated islands or bands (compare the Government geological map of New South Wales), which extend parallel to the strike and to the main ranges. The coast-line follows precisely the same direction as the mountain-ranges, and the contour lines on the steep submarine precipice which extends down from it also run in the same direction. It appears that these mountains have been formed by a process of folding consequent on a horizontal pressure acting from west to east, and moving the folds in that direction away from a centre of depression *situated in the interior of Australia*. The steep submarine precipice by which the land sinks abruptly to a very great depth appears *as a more recent fault*.

Volcanic action participated in the formation of the Australian Alps, particularly in the vicinity of Mount Bogong. The volcanoes which formed the Bogong basalt plains, &c., seem to have been active during the early part of the Devonian. It appears probable that the upheaval of the Australian Alps—the folding—took place between the Silurian and Devonian or in the ealy Devonian. Hardly any formations later than the Palæozoic take part in the formation of the Australian Alps, and those which, like the Miocene in the valleys, have been observed, show a perfectly undisturbed horizontal stratification.

It will be seen from this that the Australian Alps are *very old*, much older not only than the European Alps and Himalayas, but older also than the New Zealand Alps. The effects of erosion are consequently much more matured there than in the other mountain systems mentioned, and consequently the appearance of these mountains is of particular interest. Only here and there rocks crop out on the summits of the hills or form steep precipices on their sides. Generally speaking, the mountain forms are very tame, and round. Mountaineering in Australia can generally be performed on horseback. The basements of high massive elevations only are left. High and sharp ridges weathered into series of grotesque rock pinnacles, the characteristic of the much younger Alps of Europe, have long since disappeared, and extensive undulating table-lands now mark the localities where once high peaks have stood. These table-lands are well defined and surrounded by steeper inclines, by which they descend to the surrounding lowlands. The Kosciusco group, from which the highest mountains in Australia arise, is a remarkable example of such a table-land, extending over an area of 160 square miles, with an average height of 5600–6000 feet.

The highest mountain in Australia, Mount Townsend, which I discovered to be the culminating point of Australia, is 7351 feet high.[1] There are several other peaks in the Kosciusco group over 7000 feet, particularly Mueller's Peak, 7266 feet, which was, up to the time of my expedition, considered the highest. Two peaks, which I have named Abbott's Peak and Mount Clarke, are over 7100 feet high.

In other parts of the Australian Alps the height of 7000 feet is nowhere reached. Next in importance to the Kosciusco group is the Bogong range, the highest point of which was ascended by me this year; it is 6508 feet above sea-level. Other peaks on the basaltic plateau south of Mount Bogong attain a height of 6000–6400 feet.

The whole of the Australian Alps consists of several high table-lands divided by very broken and hilly country from each other. The lowest levels on the table-lands are usually higher than the hill-tops in the adjacent country. The valleys are cut deep into the land. The main Alpine valleys have in their upper and middle portions an elevation of about 2000 feet. Steep gorges and waterfalls occur only on the margins of the table-lands. Terraces in the valleys themselves are not met with.

The Australian Alps reach to the sea. The whole of the south-eastern coast of Australia is hilly. Towards the west and north-west they dive under the Tertiary plains through which the Murray River wends its way. In the north they terminate on the left bank of the Yass and right bank of the Shoalhaven River. The mountainous country extends beyond this line to the north-eastern corner of Australia with a similar direction of the chains; but in this locality the geological structure changes. Extensive Triassic and Carboniferous formations take the place of the gneiss-granite and Silurian of the Australian Alps South-west the Alps may be considered to terminate near Melbourne.

The Australian Alps, from the Murray plain to the sea, have a width of about 120 miles on an average, and they are, from Melbourne to the Yass River, nearly 400 miles long. They are situated between 35° and 39° S. lat. and 145° and 150° E. long. Their latitude accordingly corresponds to that of the Sierra Nevada.

The Australian Alps exercise a vast influence on the climate and rainfall, in such a manner that, whilst the greater part of Australia south of the zone of tropical rains, suffers exceedingly from want of rain, the south-east corner—the Alpine part of Australia—has sufficient rainfall for the development of the country. To this climatic influence of the Australian Alps the great superiority of the colonies of New South Wales and Victoria over all the other Australian colonies must be ascribed.

At Kiandra, situated to the north of the Kosciusco group, the only meteorological station at a high elevation, there is a very heavy rainfall; from there it rapidly decreases as we advance westwards towards the interior, and also to the east, although not so much. It increases again on the east coast.

The Eucalyptus forests of the lowlands extend up to a height of about 5800 feet. Higher elevations are destitute of trees. On the upper margin of tree-growth the forest consists of very low and stunted trees belonging to the species *Eucalyptus pauciflora* and *E. Gunnii*. This Alpine forest resembles "Krummholz" of our Alps.

[1] A detailed account of the results of my explorations will be published in the Ergänzungshefte zu *Petermann's Geografischen Mittheilungen*.

[2] Hutton, "The Supposed Glacial Period in Australia," *Proceedings* of the Linnean Society of New South Wales for 1885.

[1] I made the ascent of Mueller's Peak, the height of which was trigonometrically measured by the Victorian Geodetical Survey (Mr. Black), and Mount Townsend on the same day, and the height given above was calculated from the aneroid readings on *both peaks*.

It snows in the Australian Alps very much in the winter, and the prevailing westerly winter winds pile up masses of wind-blown snow just below the high ridges on their eastern or lee side. These masses of snow never disappear altogether in summer, and we find eternal snow in the Australian Alps at an elevation of 6500 feet.

The excessively clear and bright Australian atmosphere affords no obstacle to the nocturnal irradiation of the day's heat, and so it freezes very frequently there at night, even in midsummer, down to 5000 feet. I experienced severe frosts on Mount Kosciusco in January 1885 —January corresponds to our July—whilst it was intensely hot in the adjacent lowlands.

From these statements it is evident that we have in the Australian Alps a formidable mountain-range, which, although not glaciated now, would bear glaciers if the climate were slightly colder and more humid.

It seemed particularly surprising, therefore, that the older authors on Australian glaciation had given a verdict without examining the Alps. If no glacier traces were found in the lowlands, they yet might be found in the Alps; and if glacier traces were found in the lowlands, how much more extensive must they be in the mountains. Up the mountains I accordingly went to look for them. I undertook two expeditions. In 1885 I visited the Kosciusco group and ascended Mueller's Peak and Mount Townsend, and this year I explored the Bogong range and ascended the highest mountain in Victoria, Mount Bogong.

The Governments and learned Societies of New South Wales and Victoria greatly assisted me in my work by pecuniary aid and in other ways, and I am glad here to find an opportunity of expressing my gratitude for the great—I might say splendid—liberality with which the Australians have aided me. On my second journey I was accompanied by Mr. James Stirling, District Surveyor of Omeo, whose well-known essays on Australian glaciation have closely connected his name with the subject I had in view.

I was favoured with fine weather on both occasions, and on both occasions travelled through country never previously explored by any one with practical mountaineering experience. North of Mount Bogong I travelled for three days through country hitherto unknown. I found glacier traces on both occasions in great abundance, and in a sufficient state of preservation to be recognised as such without the shadow of a doubt. On the sides of the valleys of the tributaries to the Snowy River, which drains the eastern slopes of the Kosciusco plateau, I found abundant *roches moutonnées* at levels over 5800 feet, and high above the bottoms of the valleys. Also in some parts of the table-land itself such were found. With a little Alpine experience it is not difficult to discriminate between such ice-worn rocks and the ordinary bosses of weathered granite. These rocks are particularly well-defined in the Wilkinson Valley, the upper part of which is situated between Mount Townsend and Mueller's Peak. The bottom of the upper part of this valley is a broad and flat plain 6260 feet above sea-level. The hill-sides which surround it are everywhere worn down by glacial action up to about 800 feet above the valley bottom. The upper limit of ice-action is clearly marked, as in many valleys of the European Alps, and the thickness of the prehistoric ice stream thereby clearly indicated.

On the southern slopes of Mount Bogong, and also on the spurs of the northern flank of the mountain, basaltic erratics were found, which rocks could hardly have been transported to that locality without ice-action. In the valley of Mountain Creek, to the north of Mount Bogong, we discovered a large and well-preserved terminal moraine at an elevation of about 2800 feet, and some traces of others further down the valley.

The large moraine was carefully studied by Mr. Stirling and myself. Rocks of various kinds are scattered irregularly in it. It extends from one side of the broad valley to the other, and is cut through near the centre by the Mountain Creek. On the steep slopes towards the stream its composition of rocks brought down by an ice-stream can be easily recognised.

These two expeditions to the Australian Alps convinced me that at one time these mountains were glaciated, and the discovery of the moraine in Mountain Creek Valley, together with Stirling's (*l.c.*) elaborate researches in the Livingstone Valley, prove that the ice-streams of the Glacial period must have descended to pretty low levels. Down to 2000 feet glacial traces have been found in various parts of the Alps, and also in the Lofty Mountains near Adelaide. It is assumed by C. Wilkinson and other leading Australian geologists that a pluviatile period existed in the Miocene period, and it is obvious that such a period would probably be isochronous with the glaciation at high levels.

It is difficult to say whether the Australian and New Zealand glaciation was simultaneous, but that also appears probable. The better preservation of striæ, &c., in New Zealand is doubtless due to the greater hardness and resisting power to meteorological influences, of the ice-worn rocks in New Zealand than in the Australian Alps, where rapidly weathering granite is prevalent.

Whether this glaciation of Australasia was simultaneous with the last glaciation of Europe, or whether it was in time situated between the last glaciation and the last but one of the northern hemisphere, is not easy to decide. It appears nearly certain that it was *not later* than the last European Glacial period, and, as far as my opinion of the appearance of the traces it left behind goes, it was *earlier*. It may be hoped that future researches will show in a decisive manner whether it was simultaneous or earlier. If we do not consider merely local circumstances of sufficient effect to produce such a great change of climate as to cause so extensive a glaciation, we may, by arriving at the decision of the time of the Glacial period in Australia, also ascertain whether Glacial periods in the southern and northern hemispheres are *simultaneous or alternating*, which would give a clue to the difficult problem before us.

The necessary researches will doubtless be carried on with vigour by Australian men of science; and we may hope that their sagacity and perseverance may lead to the solution of the question, What is the cause of Glacial periods?

R. VON LENDENFELD

ROOTS [1]

IT is a fact which has become more and more evident to the practical cultivator that the results of his efforts manifest themselves on the whole in a sort of compromise between the plant and its environment: I mean that although he sees more or less distinctly what his plant should be—according to a certain standard, however—it is but rarely, if ever, that the plant cultivated perfectly fulfils in every respect what is demanded of it. Of late years this has of course forced itself more prominently before the observer, because the facts and phenomena constituting what is termed variation have been so much more definitely described, and the questions arising out of them so much more clearly formulated.

Two points can be asserted without fear of contradiction: first, the plant itself is a variable organism; and, secondly, its environment varies. Now within limits which are somewhat wide, when closely examined, the experience of man leads him to neglect the variations occurring around him, and so no one quarrels with the statement that two individual geraniums belong to the same variety, or two oak trees to the same species,

[1] See NATURE, vol. xxxi. p. 183. A lecture by H. Marshall Ward, M.A., F.L.S, Fellow of Christ's College, Cambridge; Professor of Botany in the School of Forestry, Royal Indian College, Cooper's Hill.

although an accurate description of each of the two geraniums or of the two oaks might require very different wording.

It has also become more and more evident that although we cannot ascribe all variations to their causes —very often, indeed, we cannot even suggest causes for them—there are nevertheless numerous deviations from the normal, so to speak, exhibited by plants which can be distinctly referred to certain deviations from the normal on the part of their environment.

To illustrate this we may take the case of two plants of that very common weed, the Shepherd's Purse, growing at different ends of the same small plot of ground: the soil is sandy, and so much alike all over as to be regarded as the same everywhere, nevertheless the plant at one end is large, more than a foot high, and luxuriant, with many leaves and flowers, and eventually produces numerous seeds, whereas that at the other end is small, less than 4 inches high, and bears but a few stunted leaves and three or four poor flowers and fruits. The cause of the difference is found to be the different supply of water in the two cases, and if any one doubts that this may be so, let him try the experiment of growing two or more specimens of this weed in pots: the pots to be new, filled with soil which has been thoroughly mixed, and all the pots exposed to the same conditions—*i.e.* practically the same—except that those of one series are watered sufficiently often, and those of the other only just sufficient to keep the plant actually living. The experiment is easy and conclusive with such a weed as the above. Now, it is just such experiments as that above described—some of them equally simple, others less so—that the physiologist devotes much of his attention to, and in just such a manner has been gathered together a nucleus of information around which more knowledge can be grouped.

I may make these points clearer by again quoting an illustration, and, not to confuse or mislead you by going too far afield, I will keep to the same line of investigation, partly because it is quite as simple and conclusive as any other of many that might be selected, and partly because it may be possible to set before you some facts which are interesting or even new to you.

It has been found that in some cases where two plants are growing in the same soil and under the same conditions as above, but where one plant receives less water than the other, that the dwarfed drier plant is more hairy than the larger and luxuriantly growing plant, which has been well watered. On looking more closely into this matter it turns out that the extra hairiness is (in some cases at any rate) simply due to the fact that the hairs are closer together, because the little cells on the outer parts of the plant which grow out into hairs do not increase so much in length and superficial extent as those on the well-watered plant, and thus the hairs stand thicker together on the same superficial area of the organ —of a leaf, for instance. In other cases, however, the hairs are really increased in numbers and length—the plant is absolutely more hairy. It will be noticed that details concerning growth and turgidity, and of the influence of various minerals, and so on, are not under consideration here. I am not asserting that all cases of hairiness in plants are to be ascribed to this cause; but it does occur, as stated, and the point is a curious one in view of the fact that very many plants which grow in sandy dry soils are conspicuously hairy, whereas allied species growing in or near water, or even only in moister situations, are devoid of conspicuous hairs, or even quite smooth.

The above peculiarity is not confined to leaves and stems, moreover, for experiments with roots have shown that the root-hairs, which are so important in collecting moisture, &c., from the soil, can be made to appear in enormons numbers when the root is kept in a soil which is very open and only slightly moist, whereas none or very few are developed on the same roots growing in water: this again is in accordance generally with the fact that the roots of land-plants growing in light soils develop innumerable root-hairs, whereas those of water-plants do not thus increase their surface and points of attachment. I cannot here go into all the interesting facts known about these hairs, but it will be sufficient if you bear in mind the main points just mentioned.

Let us now vary the experiments a little. It is obvious that we might suppose any number of differences in the amount of water given to the plants used in the experiments described above; but it would be found, as matter of fact, that however little be the quantity of water given to the soil in which the dwarfed plant is, compared with that put into the soil in which the luxuriant plant grows, the actual weight of water will nevertheless have to be considerable, taking the whole life of the plant into consideration—there will be more used than you probably know, moreover, because the soil itself will no doubt condense and absorb some from the atmosphere during the night. There is a minimum of water absolutely necessary, and if the plant does not obtain this it will die. Its death will be ushered in by drooping and withering of the leaves, stem, and roots, and this condition, in which the functions of the plant are interfered with beyond a certain point, passes into a condition of disease.

Now take another case. We might so arrange the experiment that we poured and continued to pour too much water into the soil. Here again it would be found that a condition of disease eventually sets in—*i.e.* a condition in which the functions of the plant are again interfered with beyond a certain point. The symptoms and progress of the disease will be very different in the latter case, however, from those in the former. It may also be mentioned that in neither experiment is death inevitable if the disturbing cause is removed soon enough—*i.e.* if sufficient water be added in the first case before the cells have ceased to be able to take it up, or if the previous conditions of the soil are restored soon enough in the case of the over-watered plant.

Here we come to a matter which is less simple than may appear at first sight. You will note that the problem in the latter case is to restore the previous conditions of the roots and soil soon enough; I put it thus, because the conditions of the roots and soil may soon be very profoundly altered by the over-watering.

To understand this, it is necessary to become a little more fully acquainted with the condition of affairs in what may be called the normal case, where the soil is light and open, and plenty of water but not too much is at the disposal of the roots. Such a soil will consist of innumerable fine particles, of different shapes, sizes, and composition. No doubt there will be grains of quartz, particles of broken up vegetable matter, and little rugged bits of stones containing various minerals; each of these tiny fragments will be covered with a thin layer of water, and you would probably be greatly surprised if I were to go into the proofs showing how extremely tenacious of its water-blanket each particle is. It may be enough for our present purpose if you accept the fact that it requires enormous force to deprive the particles of the last traces of their water-layers; they will give off some—or in some cases even a good deal—rather easily, and in fact when the layers become of a certain thickness no more water can attach itself to the particles, but it falls away and the soil remains saturated, as we say.

Now these particles of soil, each enveloped in its water-blanket, are not in close contact; there are spaces between them, and these interspaces influence the quantity of water which can be held back by the soil.

Let us suppose such a soil perfectly dry; the particles above referred to being irregular in shape and size, and only roughly in contact at various points, the interspaces will be filled with air. If water be then added in some

quantity, each of the particles becomes clothed with a layer of water, and some of the air is driven out, though bubbles of air will still exist in the larger interspaces.

A third case is conceivable—so much water might be supposed to find its way in, that no air remained in the interspaces between the particles of soil. Now it is true that such a state of affairs is not readily brought about in a normal soil; but I may indicate how the result is occasionally attained to a great extent. Suppose that a layer of clay or other impenetrable subsoil lies beneath the soil in question; then if water oozes into the soil in larger quantities than can be got rid of in the time, it is possible for nearly all the air to be displaced. Of course the object of good drainage is to prevent this; and it is often overlooked that drainage from below has the effect of drawing in air as well as of running off superfluous water—air is driven into the spaces as the water leaves them.

In speaking of the "bubbles of air" entangled in the interspaces between the particles of soil, each with its water-blanket, I have overlooked some details as to what the bubbles really are. As a matter of fact they will not remain of the same composition as ordinary air, and may soon differ considerably; besides the vapour of water, they may contain gases in quite different proportions from those in the air outside. In the type case, however, there will be some oxygen present in the bubbles.

It is not intended here to go very fully into a description of the structure of the roots of land-plants; enough if you are reminded how the smaller ramifications of a root are found to be more numerous and thinner as we approach the periphery of the mass of earth which they traverse. From the youngest rootlets are produced the root-hairs, in enormous quantities, new ones arising forwards—*i.e.* near the tip of the rootlet—as the rootlet grows on, and those behind dying off after fulfilling their functions. These functions are chiefly to apply themselves in the closest manner to the surfaces of the particles of soil, and in this way to place the water which they contain in direct continuity with the water which clings with such enormous force to the surfaces of the particles. Hence this water can pass from the soil to the plant, and anything dissolved in the water can also pass into the root-hair and thus up into the plant.

I am not going to dwell on how the root hairs themselves aid in dissolving mineral substances—corroding the surfaces of the particles of soil they cling to—nor shall I trouble you with the details of what substances will be dissolved in the water; for, of course, you will see that anything soluble will pass into solution and may be carried into the plant.

The chief point to be insisted on just now is that this water in the soil will contain among other substances oxygen dissolved in it from the air-bubbles referred to above, and that this dissolved oxygen will pass into the root-hairs in solution together with the minerals and any other substances. This oxygen, moreover, is absolutely indispensable for the life of the root-hairs; it can be easily shown that if the supply of oxygen is stopped, or even diminished to any considerable extent, the roots begin to die, because the root-hairs cease to act.

Let us look a little more closely into this point. Each root-hair is a tiny cell containing living protoplasm and certain other substances, all inclosed in a thin, elastic, porous membrane. Now it has been abundantly proved that if such a cell is deprived of oxygen, its protoplasm becomes dormant for a time, and slowly breaks up, as it were; subsequently it becomes decomposed into other and simpler materials. A sort of internal combustion and fermentation take place, and these processes result in the formation and liberation of bodies like carbon-dioxide, alcohol, acetic acid, and other acid matters—substances in the main not only incapable of supporting the life of the root-hairs but actually destructive of it.

Evidently, then, if we deprive all the root-hairs of oxygen, they will eventually die. Their death will entail that of the rootlets and roots to which they belong, and this for two obvious reasons—first, it is the root-hairs and the root-hairs alone which can absorb the necessary water and substances in solution from the soil to supply such a plant as we are concerned with; and, secondly, the noxious products resulting from their death accumulate in the soil and diffuse into the root, and so hasten similar decompositions in what were hitherto healthy cells.

It must not be supposed that these disastrous consequences of the deprivation of oxygen always follow immediately. Not only are the roots of some trees, for instance, able to withstand ill-treatment longer than others, but, obviously, the kind and degree of ill-treatment may affect the problem of how long the plant shall survive. The number of rootlets and root-hairs, and the spread of the roots and other factors, will obviously affect the matter.

Suppose the following case as an example. A young tree is growing and flourishing in an open, good soil, and, for some reason or other, more soil is heaped about the roots until the depth is increased considerably: the deeper situation has placed obstacles in the way of the roots obtaining oxygen so readily as before. Not only are the roots further from the atmosphere, but the water carried down has to percolate through more soil, and may part with much of its oxygen (or even all) on the way: of course the nature of the soil, the presence of organic matters, and other circumstances decide this. It is not at all difficult to conceive of such a case where the supply of oxygen to the roots is thus diminished so far that the activity of the root-hairs as a whole is simply lowered, but not destroyed,—a stage or two further and they might become dormant, and their protoplasm undergo intramolecular respiration for a time, and break up. It is clear that the diminished activity of the roots will affect the supply of water (and the substances dissolved in it) to the leaves: this will obviously react on the thickness of the annual rings, and this again on future supplies—since the water passes up the alburnum or young outer layers of woody tissue. Moreover, a diminution of supplies from the leaves means less substance and power for replacing the root-hairs, and so on. In this way it may require some time to kill the tree, and all kinds of complications may arise meanwhile. This case is probably by no means uncommon.

A more extreme case is where the soil becomes damp and clogged with excessive moisture: not only does no oxygen reach the roots, but noxious gases accumulate in solution in the soil, and will hurry matters by poisoning cells which might otherwise live a longer life of usefulness. It is extremely probable that such gases find their way into higher parts of the plant in the air-bubbles known to exist and to undergo alterations of pressure in the vessels of the wood: this being so, they would slowly retard the action of other living cells, and so affect the upper parts of the plant even more rapidly than would otherwise be the case. Damp soil may thus do injury according to its depth and nature; but it need not necessarily be deep to be injurious if much oxygen-consuming substance is present. I have seen excellent soil converted into damp, stinking, deadly stuff from the action and accumulation of the larvæ of cockchafers: these "grubs" may, it is true, accelerate the devastation caused by the consumption of oxygen and the accumulation of poisonous waste matters in the soil by directly cutting off portions of the roots themselves, but the accumulation of oxygen-consuming substance, and the cutting off of supplies to the root-hairs evidently plays a chief part in the destruction.

There is another matter with regard to damp soils that cannot be left out of account. I have already told you that roots which are developed in water or in very damp sandy soil—and which are perfectly healthy—have

few or no root hairs formed on their surfaces; whereas it may be readily shown that the roots of the same plant growing in a well-aërated open soil, which is scarcely moist to all appearance, will be densely covered with a close-set pile of hairs. Indeed it is by means of the millions of root-hairs on its rootlets that a sunflower or a bean, for instance, obtains the enormous quantities of water necessary for its needs from soil which, to our rough perception, seems to be dry.

I cannot here go into all the proofs that such a soil is by no means so dry as it looks; but will simply remind you of what was said above as to the enormous force with which the minute particles of rock, &c., which form "soil" retain their hold on the thin films of water which constitute what have been termed their water-blankets. This is certain, that a healthy well-rooted plant can take up water from a soil which is to all appearance air-dry; whereas a plant which has not yet had time to develop its root-hairs in sufficient numbers to take these firmly adherent water-films, from numerous particles of soil, would droop and wither.

Of course it must be borne in mind that we are speaking of land-plants such as we commonly meet with on ordinary dry land: in the case of plants which flourish in bogs or in water there are corresponding differences in the structures of their roots agreeing with the differences of environment. Even such plants need air at their roots, and an excellent illustration of this is is afforded by some willows. Our common osier and other willows grow, as you are aware, in low-lying damp and even boggy places, often flooded: now, it has been found that, if young willows are planted too deep in the soil, they very soon send out new roots—adventitious roots they are often called—close to the surface of the soil, and these roots soon do all the work. There is no doubt that this power enables these willows to live in places that would be fatal to them otherwise; and the same is true of some other plants.

Enough has now been said to show you how necessary it is that some care should be exercised in watering plants, or in exposing them to conditions different from those to which they are accustomed; and, it need scarcely be added, apparently mysterious diseases may sometimes be explained when it is shown that such precautions have been neglected. Any one can quote instances of plants which will grow in some soils and not in others, but no very satisfactory reason is afforded by simply saying that the one soil is suitable and the other not: however, all I have attempted to show you is that some soils are not suitable for some plants because the plants in question need more air at the roots than these particular soils can afford them under the circumstances.

Many plants flourish in an open soil with plenty of sand in it, but will not grow in a stiff wet soil. This is not necessarily because the heavier soil does not contain the right food-materials, but because its particles are so small, so closely packed, and so retentive of moisture, that the root-hairs do not obtain sufficient oxygen: moreover, the very damp state of the soil does not favour the development of the numerous root-hairs necessary, as we have seen. Nor is this all,—though I cannot here enter at length into this point,—root-hairs and roots cannot grow or act unless the temperature is favourable, and we have plenty of evidence to show that a close wet soil may be too cold for the roots at a time when an open drier soil (exposed to similar conditions as regards sunshine, &c.) would be of a temperature favourable to their growth. Many a pot-plant receives an extra over-dose of water because it is drooping from the roots being too cold to act properly. The opening up of stiffer soils by means of the spade or plough, or by the addition of other kinds of soil, such as sand, burnt lime, &c., or by means of drainage of various kinds, is thus to be regarded as a means of letting in air and therefore oxygen to the roots. "Sweetening the soil" is an expression one hears used by planters and others: this is often no doubt their way of expressing the fact that the air thus let in does so much to turn the noxious substances which have accumulated into other substances which the root-hairs of the plant can take up with profit. The exposure of certain soils to sharp winter frosts in part benefits the plants subsequently grown in it, because air can make its way into the cracks produced as the particles crumble: there are other advantages also due to the "weathering" of soils, of course, as also to the addition of lime, &c., but I am purposely abstaining from referring to points concerning the nutrition of plants as generally understood.

Let me shortly call your attention to a few other practical applications of the knowledge briefly summed up above. It is well known that a good deal of experience has been brought to bear on the question of what trees are the best to plant in or near large towns: there are very many facts to be considered. It is not sufficient to find a tree which will accommodate itself to the possibilities of the annual rainfall, or a diminished supply of sunlight throughout the year, and so on; nor is the problem solved when a tree is found that will put up with traces of acid gases in the atmosphere, and, as may follow, the accumulation of acids in the soil, and consequent alterations in its chemical composition. In many cases trees have been found to die as they grew older because the pavement or asphalt over their spreading root-system prevented proper aëration and a proper supply of aërated water to their root-hairs: imagine the effect of a few days' hot summer sunshine on roots just beneath the pavement of an exposed street! It is true the cover may prevent rapid evaporation, but it also shelters the soil from the well-aërated rain-drops; moreover, such sheltered roots will at certain seasons grow up to the surface of the soil and in contact with the lower surface of the pavement. Then there is the question of drainage. If the water which does find its way in slowly accumulates and becomes stagnant, the results are as disastrous or even more so; yet it is obviously a difficult matter so to arrange things that the accumulated surplus water of certain seasons shall pass away below, acting like a suction-pump and drawing in air after it, and still fulfil the other requirements hinted at above. I leave out the question of exhaustion of the soil—the dead leaves, &c., being carefully removed. Can we wonder that there are so few trees to choose from that will stand such treatment? The fact that there are some only accords with what has been already stated—that plants vary in their requirements and powers; and no one doubts that the variations have been influenced by variations in the environment.

We have now seen to a certain extent how variations of a particular kind may affect a plant. The plant responds to a certain extent—it is, as some people say, "plastic"—but if the limits are reached and slightly overstepped, the variations on the part of the plant become dangerous to its existence, and the plant becomes diseased and may die.

Not to dwell upon hypothetical matters, I will content myself with saying, in conclusion, suppose a variety of a given plant grows in damp places and has roots which form few or no root-hairs, and suppose an individual of that plant to become transferred to a more open soil: I have shown you reasons for regarding it as probable that the latter individual might produce more root-hairs and thus adapt itself to the altered conditions. If such a case happened, it is by no means improbable, but the contrary, that other circumstances co-operating or adverse would decide certain problems of importance to the existence of that particular individual.

But the main object of this lecture has been to show you how very complex the conditions may be which bring about a "diseased" condition of the roots. It is no

uncommon event to see a tree flourish for years and then slowly die off from "something at the roots": examination shows that the soil still contains the necessary foods, the water-supply is constant and good, the tree is exposed to no obvious adverse influences, and yet with steps so slow that they are scarcely noticeable, the tree begins to die off before its time. In some cases this is probably because the root-hairs are not receiving their proper supply of atmospheric oxygen, and this may be due to very slight changes in the *structure* (not the chemical composition) of the soil: a very slight diminution in the activity of the root-hairs may cause a diminution in the supply of water to the leaves at seasons when they require much, and this means lessening their supply of food-materials. If the leaves are placed on short commons, they cannot form wood, and so the next season's supply of nutritive solutions may be cut short; moreover, fewer root-hairs will be formed. No doubt differences will appear in different years or seasons; but if the tendency on the whole is in the above direction, the life of the tree is already limited—it may drag on for years as an object, which can scarcely be termed a tree however, but its doom is sealed.

The difficulty of placing one's hand on an exactly illustrative case is due to the fact that other causes are usually at work after a short time. I have purposely avoided any reference to the changes brought about in the chemical nature of a soil by the addition or cutting off of air, &c.; and for the same reason—to keep your attention directed to the root-hairs as living cells exposed to the influence of a definite environment—I have left out of account some questions of food-supply. These matters do not invalidate anything said above, but they do profoundly affect the problems of the diseases of plants, and especially those diseases which start from the roots.

ON THE PROPOSAL TO ESTABLISH A PERMANENT COLONIAL MUSEUM IN LONDON

THE proposal to continue the present Colonial and Indian Exhibition at South Kensington having met with a good deal of support, it is worth while to examine it on its merits; quite apart from the popular accessories of music, illuminations, &c., the continued existence of which depends upon altogether different considerations.

The first point for examination is whether such a permanent exhibition or museum would materially and usefully supplement or form a real addition to the existing public institutions of London, for upon the determination of this question the decision ought largely to depend.

On a general review of the vast collection of objects exhibited in the present Exhibition, they are seen to be mainly included under the four following categories:—

(1) Natural history objects, or specimens of the animal, vegetable, and mineral kingdoms of Nature.

(2) The raw products derived from them, and their economic applications.

(3) Art of every description, with which may be included objects bearing upon archæology and ethnology.

(4) Manufactures of all kinds.

(1) With reference to natural history, it can scarcely be a public desideratum to attempt to form a new museum of this kind when there exists, within a few hundred yards of the Exhibition, the finest collection in the world in the great national Museum of Natural History. There the animals, plants, fossils, and minerals not only of the British colonies, but of the whole known world, are exhibited with a fullness and in a manner that there could not be a possibility of in any way approaching.

(2) Then, as regards the economic uses of the vegetable kingdom at least—such as food-products, drugs, timbers, &c.—the nation possesses in the Museum of Kew Gardens a probably unrivalled public collection admirably exhibited. Many years of energy and a very large expenditure of time and money would fail to make up again such a collection as this has now become.

(3) Objects of art both ancient and modern form a very striking and important portion of the Exhibition. It is probable, however, that the best part of those which are not on loan have been sold or otherwise disposed of, and thus are not available for future exhibition. But with the South Kensington Museum at our doors, the initiation of a new art collection cannot be needed; whilst as for objects illustrative of ethnology and archæological specimens, they are, it is needless to say, magnificently displayed in the galleries of the old British Museum in Bloomsbury.

(4) There remains only the commercial products and manufactures of the colonies and India, and, so far as I am aware, there exists at present no general public collection of such articles. Here then, it appears to me, we have a reasonable basis for the formation of a permanent museum. A public collection of trade samples is a real want in London.

It appears, then, from the above observations, that no necessity exists for a new *general* museum of colonial and Indian productions, inasmuch as the public is already amply provided with other museums which illustrate fully nearly all the objects and articles proposed to be exhibited in the new one.

There is also good reason to think that the multiplication of museums is undesirable as well as unnecessary. We are not without experience of this, and the history of the late India Museum is quite to the point. The vast collections brought together by the Honourable East India Company were quite similar in kind to those it is now proposed to form, and illustrated very thoroughly the productions of India. But the Museum never attracted public interest or proved of much practical utility; many departments were neglected, the specimens badly conserved, and not available for consultation or study, and at last its condition having become somewhat of an official scandal, it was, six or seven years ago, broken up and dispersed. It bears strongly on the remarks above made that the collections had to be distributed among the very museums which I have there enumerated. No doubt additions of much value thus accrued to them; but there was also an immense mass of duplicate and damaged material, some of which at least was destroyed. After this experience it seems scarcely credible that a proposal to form again another general *Indian* Museum in London will be seriously entertained, whatever may be the case as regards the colonies. But in the latter, as in the former, it is almost certain that from similar causes a few years would witness the same history and a similar termination.

It is then, I believe, in a permanent museum of trade samples and of the commercial products of our colonies that a really useful outcome of the present Exhibition is to be sought. The precise scope and character of such a museum would of course require careful consideration; but there is a great and increasing want of some central emporium of a public character where authentic samples, accurately determined and labelled, can be readily inspected and examined by those interested in commercial pursuits. The collection might well be arranged geographically, and should be accompanied by maps, trade statistics, and other aids to inquiry. Under able management such a museum would be capable of rendering great service to the commerce of the Empire, and be the means of bringing into trade the numerous neglected products of the world. I may add, parenthetically, that it would also relieve the staffs of our chief scientific establishments of a good deal of work, involving often much sacrifice of time, which now falls upon them, though outside the scope of their duties.

The situation of such a museum should, however, be

readily accessible to business men, and would be preferably in or close to the City rather than in the West End of London.

HENRY TRIMEN

NOTES

THE International Geodetic Conference will assemble in Berlin on October 20. Its principal business will be to deliberate on the best method of executing the resolutions arrived at at Rome and Washington in 1883 and 1884 respecting the actual measurement of a degree on the earth's surface, and likewise in reference to a scientific survey of the European continent. The adoption by all nations of Greenwich as the first meridian, in accordance with the decision taken at Washington, is to be strictly enforced in practice. The introduction of international normal time, on the other hand, has had to be postponed, owing to insuperable practical difficulties connected with ordinary business life. In order to promote the project of any international survey of the entire globe, it is proposed to establish a Central Geodetic Office in Berlin.

THE Association for the Improvement of Geometrical Teaching has revised its "Syllabus of Elementary Geometrical Conics," and is about to publish the same, with three figures lettered in accordance with the enunciations of the Syllabus. The work will be interleaved to allow of teachers and students supplying their own proofs, and will, it is hoped, appear early in November. Messrs. Swan Sonnenschein are the publishers.

THE Bombay Government has just issued a long resolution on the subject of technical education, which is one of special importance to India. The resolution lays down the outlines of the scheme which it favours under three heads—agriculture, art, and mechanical industries. It proposes that the College of Science at Poonah should be a central institution for the teaching of higher agriculture, and that local classes and schools should be established throughout the province under the supervision of district officers and of the Educational Department. The Jamsetjee Jeejeebhoy School of Art in Bombay is to be the centre of Government efforts for the purpose of art teaching, and a report is called for as to the propriety of obtaining additional teaching. The question whether a technological institute for mechanical industries should be established is discussed at some length, and the Government expresses the opinion that the time for doing so has not yet come. Meanwhile, it is suggested that the Committee of the Ripon Memorial Fund should form itself into an association for promoting technical education in Bombay city, the Government promising to give it the utmost possible aid. The main dependence of other parts of the province must be upon the high schools for elementary science, and upon such institutions as may be started by means of local efforts. The resolution concludes by saying that the scheme is not academic, but that it is meant to enhance the well-being of the people at large by giving increased employment to labour and capital, and by cementing harmonious relations between them.

THE International Congress of Orientalists was opened at Vienna on the 27th inst., under the presidency of the Archduke Rénier. This is the seventh Congress of this body, the previous ones having been held at Paris in 1873, at London in 1874, St. Petersburg in 1876, Florence in 1878, Berlin in 1881, and Leyden in 1883. The Austrian Minister of Public Instruction welcomed the members, of whom there were about 300, in the name of the Government.

DR. SCHWEINFURTH has, in the interests of science, addressed to all Europeans, especially physicians, residing in Egypt, an inquiry as to whether, so far as they are aware, families of Northern origin settling in Egypt do, or do not, die out within three generations, or whether the race is capable of being perpetuated beyond that limit.

WE are requested to announce that the seventh annual Cryptogamic and Botanical Meeting of the Essex Field Club will be held on Friday and Saturday, October 15 and 16, in Epping Forest, the head-quarters for the day being at Buckhurst Hill. A large number of well-known botanists have promised to take part in the meeting, and the naming and arrangement of the specimens collected will be in the hands of Dr. Cooke, Rev. Canon Du Port, Dr. Wharton, Mr. Worthington Smith, and other fungologists. Botanists and others desirous of attending should communicate with the Hon. Secretary, Buckhurst Hill, Essex.

THE U.S. Hydrographic Office has received the following note:—"August 31, at 9.45 p.m., the steamer *City of Palatka*, Capt. Vögel, when a mile and a half north of Martin's industry lightship (off the coast, south of Charleston), in eight fathoms and a half of water, experienced a terrible rumbling sensation, lasting a minute and a half. There was quite a heavy sea from the south-east after leaving Charleston Bar at 5.30 p.m. When this rumbling sensation took place the wave-motion ceased. It was a perfect calm during the rumbling; after that the usual motion of the south-east swell took place. The wind at the time was south-west, light, weather cloudy, barometer 30·1, thermometer 80°. The sensation resembled a ship scraping a pebbly bottom, and the vibration of the ship was very great."

H.R.H. THE PRINCE OF WALES has decided that the Colonial and Indian Exhibition shall close on the evening of Wednesday, November 10.

WE hear that the first of the Grocers' "Medical Research Scholarships" has been awarded to Dr. Sims Woodhead, of Edinburgh. The value of the award is 250*l*.

"PHILIPS' Planisphere, showing the Principal Stars visible for every Hour in the Year" (London: George Philip and Son), is perhaps the best means yet devised of getting a preliminary acquaintance with the aspect of the sky. It consists of a movable disk representing the celestial sphere, and a fixed horizon corresponding to the latitude of London. On the edge of the disk are inscribed the signs of the zodiac, the months and days of the year; on the horizon, the hours of the day and night. By merely rotating the disk until any given day and hour are brought to coincide, the stars above the visible horizon of London at that time come into view. A continuance of the movement from east to west exhibits ten apparent revolutions of the stars. Each successive group on the chart rises and sets in its proper order, while its distance from the sun at any selected date can be estimated by following a line drawn from the celestial pole to the corresponding section of the disk. Its point of intersection with the ecliptic indicates the position of the sun. The same lines show the differences between solar and sidereal time throughout the year. A very little attention will enable the student to distinguish the circumpolar stars, to track the course of the Milky Way among the constellations, and to acquire some rough notion of the magnitudes of the principal stars. Quite a little stock of uranographical information, in short, is concentrated in this ingenious toy.

A CAREFUL revision of the hydrographic map of the Lake of Geneva has been lately made by M. Hornlimann, soundings being taken by the steel-wire method. It is shown that between Lutry, Ouchy, Evian, and La Tour-ronde the bottom of the lake is absolutely horizontal. For distances of 2 kilometres and more the differences of depth did not exceed 0·10 to 0·15 m. (being thus quite within the limits of observational error. The point of greatest depth was met with in the line which joins the mouth of the Flon, below Lausanne, and the church of Evian, 7 km. from the Swiss side and 5 from that of Savoy. This was 310 m. (say 1034 feet). The bottom of the lake is here about 219 feet above the sea.

THE earthquakes still continue in North America. Fresh shocks were felt at Charleston and other places in the south at five o'clock on the afternoons of the 27th and 28th inst. Shocks of earthquake were also at the same hour distinctly felt in Columbia, Augusta, and Savannah.

A SHARP shock of earthquake occurred at Constantinople at half-past four on the morning of the 26th inst., but no damage was done. At about a quarter to five on the same morning two sharp shocks in rapid succession were felt in Smyrna and the neighbourhood.

AN earthquake was felt at Aumale on the 22nd inst. at 11 a.m.; four shocks were reported.

THE White Island volcano, in the Bay of Plenty, off the North Island coast, New Zealand, is in active eruption, and sending forth a vast column of flame and smoke, rising to a height of 100 feet.

THE Ceylon branch of the Royal Asiatic Society has decided to print *in extenso* a translation of Prof. Virchow's monograph on the Veddas. An abridgment will appear in the forthcoming number of the Society's *Proceedings*.

FROM the Cambridge University Press the following new publications are announced:—"A History of the Theory of Elasticity and of the Strength of Materials, from Galilei to the Present Time," vol. i. "Galilei to Saint-Venant, 1639–1850," by the late I. Todhunter, D.Sc., F.R.S., edited and completed by Karl Pearson, M.A. "Lectures on the Physiology of Plants," by S. H. Vines, M.A., D.Sc., Fellow of Christ's College. "Travels in Northern Arabia in 1876 and 1877," by Charles M. Doughty, of Gonville and Caius College (with illustrations). "The Scientific Papers of the late Prof. J. Clerk Maxwell," edited by W. D. Niven, M.A.

MESSRS. CROSBY LOCKWOOD AND CO. announce the following books for the forthcoming season:—"Modern Engines and Boilers: Marine, Locomotive, and Stationary," by Walter S. Hutton, Civil and Mechanical Engineer (with upwards of 300 illustrations). "The Works' Manager's Hand-book of Modern Rules, Tables, and Data, for Civil and Mechanical Engineers, &c.," by Walter S. Hutton (third edition). "The Portable Engine, in Theory and Practice," by W. D. Wansbrough (with numerous illustrations). "Expansion of Structures by Heat," by John Keily, C.E., late Indian Public Works and Victorian Railway Departments. "Safe Railway Working," by Clement E. Stretton, C.E. "Drainage of Lands, Towns, and Buildings," a practical treatise, being an abridgment of the works of the late G. D. Dempsey, C.E., with extensive additions by D. Kinnear Clark, M.Inst.C.E. "Trusses of Wood and Iron: Practical Applications of Science in determining the Stresses, Breaking Weights, Safe Loads, Scantlings, and details of Construction," by William Griffiths. "Shoring and its Application," a manual for students, by George H. Blagrove (with numerous illustrations).

H. K. LEWIS has in preparation "An Introduction to Practical Bacteriology," by Edgar M. Crookshank, M.B. Lond., F.R.M.S., Demonstrator of Physiology, King's College, London (2nd edition); also, by the same author, "Photographs of Bacteria: an Investigation into the Value of Photography for delineating Preparations of Bacteria" (illustrated with 50 permanent autotypes and numerous wood engravings).

THE following publications are announced by Messrs. W. and R. Chambers:—"Natural History: its Rise and Progress in Britain, as developed in the Life and Labours of Leading Naturalists," by Prof. H. Alleyne Nicholson (Aberdeen). This will form vol. i. of a series called "Chambers's British Science Biographies," of which series the second volume, by Prof. Lapworth (Birmingham), will cover the field of British Geology. Other new books by the same publishers are: "Recent Travel and Adventure," with illustrations; and "Lessons in Elementary Dynamics," by H. G. Madan, M.A., Science Master in Eton College.

THE grease of sheep's wool, a substance hardly utilised hitherto, may now find use, according to a process lately brought before the French National Society of Agriculture by M. Robart. He finds that, brought to its point of fusion, it very readily absorbs certain sulphur-compounds; thus it will fix as much as 100 times its volume of sulphuretted hydrogen; and so treated it becomes saponifiable in the cold state. M. Rohart presented some excellent soap made from the grease. The operation required takes less than an hour, whereas soaps with a base of soda generally take 6 to 8 hours in their production. Moreover, the saponification can be obtained completely without caustic alkalies, and simply with alkaline carbonates; a new scientific fact, applicable to all fatty matters when sulphurised. Thus a great economy is possible. This sulphurised soap is recommended by M. Rohart, *inter alia*, for use in vine-cultivation.

IN a recent thesis on the modification of plants by climate, Mr. Crozier, of Michigan University, considers it established "that as plants move from the locality of their largest development towards their northern limit of growth, they become dwarfed in habit, are rendered more fruitful, and all parts become more highly coloured, Their comparative leaf surface is often increased, their form modified, and their composition changed. Their period of growth is also shortened, and they are enabled to develop at a lower temperature."

THE successful cultivation, since 1884, of the Ramie or China grass plant (*Boehmeria nivea*) on the Champ-de-l'Air at Lausanne (altitude 520 m.), by Prof. Schnetzler, is an interesting fact in botany. This shrub, a native of China and Sumatra, has been grown in the south of the United States and of France for thirty years. Recently it has been introduced into Algeria. There is of course a striking difference in the conditions of temperature between Lausanne and the places in Asia where Ramie is grown. While the latitude of the latter is from 15° to 35°, that of Lausanne is 46° 31′. The mean temperature at Lausanne is 9°·5 C. Last winter the plants underwent long periods of great cold; in one case, *e.g.*, the thermometer being below zero for 124 hours, with a minimum on the ground of −12°·5 C.

THE question of telephony *v.* telegraphy has been recently discussed by a well-known German electrician, Dr. Wietlisbach. The chief hindrance to the use of the telephone for long distances is, he points out, of a financial, not of a technical, nature. A telephone-line 2000 km. long costs considerably over a million marks. It is still possible to speak very well this distance; but even supposing the line were in constant use day and night, the receipts must be 5 marks (say shillings) a minute to make it pay. In telephone work, however, the line is in use only a few hours daily; hence a short conversation would cost at least 50 marks (2*l.* 10*s.*). That is, of course, too dear for ordinary traffic. The telegraph works, with almost the same speed, more than ten times more cheaply. Thus the question as to rivalry between telephone and telegraph finds its settlement. The telephone, up to about 500 km. distance (say 310 miles), will more and more displace the telegraph, and find an extension which the telegraph would never reach. But for greater distances the telegraph must keep the upper hand. Thus telephone and telegraph are really not rivals, but fitted to supplement each other.

WE have received the report for the past year of the School of Mines, Ballaarat, an institution which its Council believe is in a fair way of becoming the leading School of Science in the colony of Victoria. The increasing number of the students who avail themselves of the constantly extending opportunities for instruction offered by the School renders additional teaching power a necessity, and this requires, first of all, an increased income. It is to be hoped that the Council have been successful in its request for double the present annual subsidy from the Government. A School of Mines is perhaps the most immediately useful and paying one a young community can have. A new and enlarged museum has been added to the School, and Mr. Oddie, the Vice-President, has undertaken at his own expense to erect and equip an astronomical observatory. Two rooms, each 16 feet by 18, were erected when the report was drafted, and in one of these a 12½-inch Newtonian reflector has been placed in position. The second room is to be utilised for spectrum analysis, solar physics, testing specula, &c. A system of meteorological observations with the latest instruments, in connection with the Melbourne Observatory, has also been introduced. A recent task of the School authorities, in which many of our readers may be presumed to be interested, is the collection of rocks and minerals representing the geology of Western Victoria in the Colonial and Indian Exhibition. At the close of the Exhibition it will be presented to the Museum of the Geological Survey of Great Britain. The reports of the individual professors show progress in almost every direction—in the number of students, of subjects taught, and of average attendances of each student. We observe that the benefits of the School are largely extended by means of a concession from the Government railways permitting students to travel over long distances at exceedingly low fares. This is one of those concessions which cost so little, yet are worth so much, and which are more common in the United States or the colonies than they are in England.

IN a very interesting paper contributed to the *Bulletin* of the Essex Institute of Salem, Mr. A. McFarland Davis writes on some of the games of the Indian tribes of North America. Several of these are described at considerable length, mostly from the early Jesuit records. Lacrosse is the first and most important of these; it was, as it is now, purely a game of skill, but it was a contest of grave importance, not a mere pastime, and was domesticated over a wide extent of territory. Another very widely-spread game was "platter," which was played with dice, and was wholly a game of chance; the third was a game of chance and skill combined, and in some of its forms was exceedingly complicated. It was called "straws," because a bundle of straws was divided, the game turning on the odd or even numbers in the heaps. It resembles the celebrated Chinese game of *fantan*, which forms one of the principal sources of revenue of one European colony in the East. Sundry other games not so widely spread as these are also described by Mr. Davis. The extraordinary importance attached to these games, the strange and solemn ceremonies with which they were frequently initiated, give them an interest in the eyes of anthropologists beyond that of mere curiosity.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*) from India, presented by Mrs. Faulkner; two Golden Eagles (*Aquila chrysaetos*) from the Isle of Mull, Argyllshire, Scotland, presented by His Grace the Duke of Argyll, K.G., F.Z.S.; a Stock Dove (*Columba œnas*), British, presented by Mr. Charles Whymper, F.Z.S.; an Anaconda (*Eunectes murinus*) from South America, deposited; a Lesser White-nosed Monkey (*Cercopithecus petaurista*) from West Africa, purchased; a Maned Goose (*Bernicla jubata*) from Australia, received in exchange; a Spotted Hyæna (*Hyæna crocuta*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

STELLAR PHOTOMETRY.—Mr. Chandler, of Cambridge, U.S., presented an interesting and important paper to the Section of Mathematics and Astronomy of the American Association at the recent meeting, the title being "A Comparative Estimate of Methods and Results in Stellar Photometry." According to the account of the paper given in *Science* (vol. viii., No. 187), Mr. Chandler took for his text the general statement that instrumental photometry had thus far proved a failure; that is, it had not developed a more uniform scale than Argelander's, nor had the accuracy of individual determinations been increased, but they were, on the contrary, far more uncertain than the old differential naked-eye estimates. In support of his views Mr. Chandler showed that, for stars of Argelander's scale between magnitudes 2 and 6, the photometric catalogues of Seidel, Peirce, Wolf, Pickering, and Pritchard differed among themselves as much in their measures of what Argelander called a difference of one magnitude, as they did in their measures of his successive magnitudes. Their average values of the logarithm of the light-ratio for one of Argelander's magnitudes between 2 and 6, ranged between ·30 and ·38, about ·35 for the mean of all the above-mentioned catalogues. Between magnitudes 6 and 9 of Argelander's scale, the catalogues of Rosén and Ceraski averaged about ·35 for the light-ratio, while Pickering's late results with the meridian photometer gave (between magnitudes 6 and 8·5) ·48 instead of ·35 for this ratio. Coming to accidental errors, Mr. Chandler showed that, from a discussion of the naked-eye estimates of Gould, Sawyer, and himself, the probable error of a single estimate was a little over ± ·06 of a magnitude when the stars were at considerable distances from each other, and about ± ·05 of a magnitude when near; while the probable error of a single measure in the "Harvard Photometry" was ± ·17 of a magnitude, and in the "Uranometria Oxoniensis" about ± ·10 of a magnitude. The large residuals in the "Harvard Photometry" appear to arise, according to Mr. Chandler, from the wrong identification of stars in many cases, one instance being cited where no bright star exists in or near the place given in the observing-list, on account of a misprint in the *Durchmusterung*, and yet some neighbouring star was observed on several nights for it. The author, in conclusion, pointed out that we must obtain better results from photometers if we ever expect to use their results for the detection or measurement of variable stars, since several variables have been detected, and their periods and light curves well determined by eye-estimates, whose whole range of variation is no greater than the whole range of error in the photometric observations upon a single star with the meridian photometer.

A NEW OBSERVATORY IN LA PLATA.—In the *Bulletin Astronomique*, tome iii. Août 1886, M. Mouchez gives an account of a new Observatory which is being built in the town of La Plata. The Observatory appears to have a remarkably good instrumental equipment, including a telescope of 0·80m. aperture, an "équatorial coudé" of 0·43m. aperture, a meridian instrument of 0·22m. aperture, an apparatus for celestial photography of the same dimensions as that of MM. Henry at the Paris Observatory, a Thollon spectroscope with objective of 0·25m. aperture, besides a collection of geodetical instruments. The new Observatory is under the direction of M. Beuf, lately an officer in the French Navy, and his first efforts are to be directed towards the carrying out of a geodetic survey of the vast territory of the province, including the measurement of an extensive meridian arc in the plains of Chaco and Patagonia. The measurement of this arc will supply a want which has been long felt by geodesists, and will give new and valuable data for an increase in our knowledge of the terrestrial spheroid. He trusts that M. Beuf will be successful in this arduous and important undertaking, and also that he will have sufficient energy, and be supplied with a sufficient staff of observers, to work to advantage the numerous and powerful instruments which the Observatory possesses.

HELIOMETRIC OBSERVATIONS OF THE PLEIADES.—In the note on this subject, printed in last week's "Astronomical Column," the words "since 1860" should read "since 1840," the latter being the date of Bessel's determinations resulting from his observations with the Königsberg heliometer made during the years 1829-41.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *OCTOBER* 3–9

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on October 3

Sun rises, 6h. 6m.; souths, 11h. 49m. 1·7s.; sets, 17h. 32m.; decl. on meridian, 4° 2′ S.: Sidereal Time at Sunset, 18h. 21m.

Moon (at First Quarter October 4) rises, 12h. 26m.; souths, 16h. 54m.; sets, 21h. 20m.; decl. on meridian, 18° 28′ S.

Planet	Rises	Souths	Sets	Decl. on meridian
	h. m.	h. m.	h. m.	° ′
Mercury ...	6 26	12 5	17 44	4 52 S.
Venus ...	4 34	10 55	17 16	3 23 N.
Mars ...	10 44	14 55	19 6	20 45 S.
Jupiter ...	6 30	12 8	17 46	4 54 S.
Saturn ...	22 43*	6 46	14 48	21 24 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich)

Oct.	Star	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image
			h. m.	h. m.	° °
6	B.A.C. 7097	6	22 33	23 42	142 312
8	e^2 Aquarii...	5½	22 29	near approach	39 —

Oct.	h.	
9	17	Jupiter in conjunction with the Sun.

Variable Stars

Star	R.A.	Decl.			
	h. m.	° ′		h. m.	
Algol ...	3 0·8	40 31 N.	Oct.	4, 21 20	*m*
λ Tauri ...	3 54·4	12 10 N.	,,	6, 4 9	*m*
ζ Geminorum ...	6 57·4	20 44 N.	,,	4, 2 0	*M*
δ Libræ ...	14 54·9	8 4 S.	,,	5, 1 19	*m*
U Coronæ ...	15 13·6	32 4 N.	,,	5, 21 10	*m*
S Scorpii ...	16 10·9	22 37 S.	,,	9,	*M*
U Ophiuchi ...	17 10·8	1 20 N.	,,	3, 2 6	*m*
		and at intervals of		20 8	
U Sagittarii ...	18 25·2	19 12 S.	Oct.	5, 0 0	*m*
			,,	8, 0 0	*M*
β Lyræ ...	18 45·9	33 14 N.	,,	5, 3 0	*m*
R Aquilæ ...	19 0·9	8 4 N.	,,	6,	*M*
δ Cephei ...	22 24·9	57 50 N.	,,	9, 0 0	*M*

M signifies maximum; *m* minimum.

Meteor Showers

The coming week is usually a somewhat less fruitful one for meteors than the one just past. The *Arietids*, October 7, R.A. 31°, Decl. 9° N., form the principal shower; a radiant in Musca, R.A. 46°, Decl. 26° N., and another near Polaris, R.A. 133°, Decl. 79° N., are also active at this time.

METEORITES, METEORS, AND SHOOTING-STARS[1]

YOU are kindly giving to me an hour to-night in which I may speak to you. I do not have enough confidence in myself to justify me in speaking to such an audience as this upon one of those broad subjects that belong equally to all Sections of the Association. The progress, the encouragements, and the difficulties in each field are best known to the workers in the field, and I should do you little good by trying to sum up and recount them. Let me rather err, then, if at all, by going to the opposite extreme.

Two years ago your distinguished President instructed and delighted us all by speaking of the pending problems of astronomy, what they are, and what hopes we have of solving them. To one subject in this one science, a subject so subordinate that he very properly gave it only brief notice, I ask your attention. I propose to state some propositions which we may believe to be probably true about the meteorites, the meteors, and the shooting-stars.

In trying to interest you in this subject, so remote from the studies of most of you, I rely upon your sense of the unity of all science, and at the same time upon the strong hold which these weird bodies have ever had upon the imaginations of men. In ancient times temples were built over the meteorite images that fell down from Jupiter, and divine worship was paid them; and in these later days a meteorite stone that fell last year in India became the object of daily anointings and other ceremonial worship. In the fearful imagery of the Apocalypse, the terrors are deepened by there falling "from heaven a great star burning as a torch," and by the stars of heaven falling "unto the earth as a fig tree casteth her unripe figs when she is shaken of a great wind." The "great red dragon having seven heads and ten horns, and upon his head seven diadems," is presented in the form of a huge fire-ball. "His tail draweth the third part of the stars of heaven, and did cast them to the earth." Records of these feared visitors, under the name of flying dragons, are found all through the pages of the monkish chroniclers of the Middle Ages. The Chinese appointed officers to record the passage of meteors and comets, for they were thought to have somewhat to say to the weal or woe of rulers and people.

By gaining in these later days a sure place in science, these bodies have lost their terrors; but so much of our knowledge about them is fragmentary, and there is still so much that is mysterious, that men have loved to speculate about their origin, their functions, and their relations to other bodies in the solar system. It has been easy, and quite common too, to make these bodies the cause of all kinds of things for which other causes could not be found.

They came from the moon; they came from the earth's volcanoes; they came from the sun; they came from Jupiter and the other planets; they came from some destroyed planet; they came from comets; they came from the nebulous mass from which the solar system has grown; they came from the fixed stars; they came from the depths of space.

They supply the sun with his radiant energy; they give the moon her accelerated motion; they break in pieces heavenly bodies; they threw up the mountains on the moon; they made large gifts to our geological strata; they cause the auroras; they give regular and irregular changes to our weather.

A comparative geology has been built up from the relations of the earth's rocks to the meteorites; a large list of new animal forms have been named from their concretions; and the possible origin of life in our planet has been credited to them.

They are satellites of the earth; they travel in streams, and in groups, and in isolated orbits about the sun; they travel in groups and singly through stellar spaces; it is they that reflect the zodiacal light; they constitute the tails of comets; the solar corona is due to them; the long coronal rays are meteor streams seen edgewise.

Nearly all of these ideas have been urged by men deservedly of the highest repute for good personal work in adding to human knowledge. In presence of this host of speculations it will not, I hope, be a useless waste of your time to inquire what we may reasonably believe to be probably true. And if I shall have no new hypotheses to give you, I offer as my excuse that nearly all possible ones have been already put forth. This Association exists, it is true, for the advancement of science, but science may be advanced by rejecting bad hypotheses as well as by framing good ones.

I begin with a few propositions about which there is now practical unanimity among men of science. Such propositions need only be stated. The numbers that are to be given express quantities that are open to revision and moderate changes.

(1) The luminous meteor tracks are in the upper part of the earth's atmosphere. Few, if any, appear at a height greater than one hundred miles, and few are seen below a height of thirty miles from the earth's surface, except in rare cases where stones and irons fall to the ground. All these meteor tracks are caused by bodies which come into the air from without.

(2) The velocities of the meteors in the air are comparable with that of the earth in its orbit about the sun. It is not easy to determine the exact values of those velocities, yet they may be roughly stated as from fifty to two hundred and fifty times the velocity of sound in the air, or of a cannon-ball.

(3) It is a necessary consequence of these velocities that the meteors move about the sun and not about the earth as the controlling body.

(4) There are four comets related to four periodic star-showers that come on the dates April 20, August 10, November 14, and November 27. The meteoroids which have given us any one of these star-showers constitute a group, each individual of which

[1] Address to the American Association for the Advancement of Science, at Buffalo, August 18, 1886, by Prof. H. A. Newton, of New Haven, the retiring President of the Association.

moves in a path which is like that of the corresponding comet. The bodies are, however, now too far from one another to influence appreciably each other's motions.

(5) The ordinary shooting-stars in their appearance and phenomena do not differ essentially from the individuals in star-showers.

(6) The meteorites of different falls differ from one another in their chemical composition, in their mineral forms, and in their tenacity. Yet through all these differences they have peculiar common properties which distinguish them entirely from all terrestrial rocks.

(7) The most delicate researches have failed to detect any trace of organic life in meteorites.

These propositions have practically universal acceptance among scientific men. We go on to consider others which have been received with hesitation, or in some cases have been denied.

With a great degree of confidence, we may believe that shooting-stars are solid bodies. As we see them they are discrete bodies, separated even in prolific star-showers by large distances one from another. We see them penetrate the air many miles, that is, many hundred times their own diameters at the very least. They are sometimes seen to break in two. They are sometimes seen to glance in the air. There is good reason to believe that they glance before they become visible.

Now these are not the phenomena which may be reasonably expected from a mass of gas. In the first place a spherical mass of matter at the earth's distance from the sun, under no constraint, and having no expansive or cohesive power of its own, must exceed in density air at one-sixth of a millimetre pressure (a density often obtained in the ordinary air-pump), or else the sun by his unequal attraction for its parts will scatter it. Can we conceive that a small mass of gas with no external restraint to resist its elastic form, can maintain so great a density?

But suppose that such a mass does exist, and that its largest and smallest dimensions are not greatly unequal; and suppose further that it impinges upon the air with a planetary velocity; could we possibly have as the visible result a shooting-star? When a solid meteorite comes into the air with a like velocity, its surface is burned or melted away. Iron masses and many of the stones have had burned into them those wonderful pittings or cupules which are well imitated, as M. Daubrée has shown, by the erosion of the interior of steel cannon by the continuous use of powder under high pressure. They are imitated also by the action of dynamite upon masses of steel near which the dynamite explodes. Such tremendous resistance that mass of gas would have to meet! The first effect would be to flatten the mass, for it is elastic; the next to scatter it, for there is no cohesion. We ought to see a flash instead of a long burning streak of light. The mass that causes the shooting-star can hardly be conceived of except as a solid body.

Again, we may reasonably believe that the bodies that cause the shooting-stars, the large fire-balls, and the stone-producing meteor, all belong to one class. They differ in kind of material, in density, in size. But from the faintest shooting-star to the largest stone-meteor, we pass by such small gradations that no clear dividing lines can separate them into classes. See wherein they are alike:—

(1) Each appears as a ball of fire traversing the apparent heavens, just as a single solid but glowing or burning mass would do.

(2) Each is seen in the same part of the atmosphere, and moves through its upper portion. The stones come to the ground, it is true, but the luminous portion of their paths generally ends high up in the air.

(3) Each has a velocity which implies an orbit about the sun.

(4) The members of each class have apparent motions which imply common relations to the horizon, to the ecliptic, and to the line of the earth's motion.

(5) A cloudy train is sometimes left along the track, both of the stone-meteor, and of the shooting-star.

(6) They have like varieties of colours, though in small meteors they are naturally less intense and are not so variously combined as in large ones.

In short, if the bodies that produce the various kinds of fire-balls had just the differences in size and material which we find in meteorites, all the differences in the appearances would be explained; while, on the other hand, a part of the likenesses that characterise the flights point to something common in the astronomical relations of the bodies that produce them.

This likeness of the several grades of luminous meteors has not been admitted by all scientific men. Especially it was not accepted by your late President, Prof. J. Lawrence Smith, who by his studies added so much to our knowledge of the meteorites. The only objection, however, so far as I know, that has been urged against the relationship of the meteorites and the star-shower meteors, and the only objection which I have been able to conceive of that has apparent force is the fact that no meteorites have been secured that are known to have come from the star-showers. This objection is plausible, and has been urged, both by mineralogists and astronomers, as a perfect reply to the argument for a common nature to all the meteors.

But what is its real strength? There have been in the last hundred years five or six star-showers of considerable intensity. The objection assumes that if the bodies then seen were like other meteors we should have reason to expect that among so many hundreds of millions of individual flights a large number of stones would have come to the ground and have been picked up.

Let us see how many such stones we ought to expect. A reasonable estimate of the total number of meteors in all of these five or six star-showers combined makes it about equal to the number of ordinary meteors which come into the air in six or eight months. Inasmuch as we can only estimate the numbers seen in some of the showers, let us suppose that the total number for all the star-showers was equal to one year's supply of ordinary meteors. Now the average annual number of stone-meteors of known date from which we have secured specimens has, during this hundred years, been about two and a half.

Let us assume, then, that the luminous meteors are all of like origin and astronomical nature; and further assume that the proportion of large ones, and of those fitted to come entirely through the air without destruction, is the same among the star-shower meteors as among the other meteors. With these two assumptions, a hundred years of experience would then lead us to expect two, or perhaps three, stone falls from which we secure specimens during all the half-dozen star-showers put together. To ask for more than two or three is to demand of star-shower meteors more than other meteors give us. The failure to get these two or three may have resulted from chance, or from some peculiarity in the nature of the rocks of Biela's and Tempel's comets. It is very slender ground upon which to rest a denial of the common nature of objects that are so similar in appearance and behaviour as the large and small meteors.

It may be assumed, then, as reasonable that the shooting-stars and the stone-meteors, together with all the intermediate forms of fire-balls, are like phenomena. What we know about the one may with due caution be used to teach facts about the other. From the mineral and physical nature of the different meteorites, we may reason to the shooting-stars, and from facts established about the shooting-stars we may infer something about the origin and history of the meteorites. Thus it is reasonable to suppose that the shooting-stars are made up of such matter and such varieties of matter as are found in meteorites. On the other hand, since star-showers are surely related to comets, it is reasonable to look for some relation of the meteorites to the astronomical bodies and systems of which the comets form a part.

This common nature of the stone-meteor and the shooting-stars enables us to get some idea, indefinite but yet of great value, about the masses of the shooting-stars. Few meteoric stones weigh more than 100 lbs. The most productive stone-falls have furnished only a few hundred pounds each, though the irons are larger. Allowing for fragments not found, and for portions scattered in the air, such meteors may be regarded as weighing a ton, or it may be several tons, on entering the air. The explosion of such a meteor is heard a hundred miles around, shaking the air and the houses over the whole region like an earthquake. The size and brilliancy of the flame of the ordinary shooting-star is so much less than that of the stone-meteor that it is reasonable to regard the ordinary meteoroid as weighing pounds, or even ounces, rather than tons.

Determinations of mass have been made by measuring the light and computing the energy needed to produce the light. These are to be regarded as lower limits of size, because a large part of the energy of the meteors is changed into heat and motion of the air. The smaller meteors visible to the naked eye may be thought of without serious error as being of the size of gravel stones, allowing, however, not a little latitude to the meaning of the indefinite word "gravel."

These facts about the masses of shooting-stars have important

consequences. The meteors, in the first place, are not the fuel of the sun. We can measure and compute within certain limits of error the radiant energy emitted by the sun. The meteoroids large enough to give shooting-stars visible to the naked eye are scattered very irregularly through the space which the earth traverses; but in the mean each is distant two or three hundred miles from its near neighbours. If these meteoroids supply the sun's radiant energy, a simple computation shows that the average shooting-star ought to have a mass enormously greater than is obtained from the most prolific stone-fall.

Moreover, if these meteoroids are the source of the solar heat, their direct effect upon the earth's heat by their impact upon our atmosphere ought also to be very great: whereas the November star-showers, in some of which a month's supply of meteoroids was received in a few hours, do not appear to have been followed by noticeable increase of heat in the air.

Again, the meteoroids do not cause the acceleration of the moon's mean motion. In various ways the meteors do shorten the month as measured by the day. By falling on the earth and on the moon they increase the masses of both, and so make the moon move faster. They check the moon's motion, and so, bringing it nearer to the earth, shorten the month. They load the earth with matter which has no momentum of rotation, and so lengthen the day. The amount of matter that must fall upon the earth in order to produce in all these ways the observed acceleration of the moon's motion, has been computed by Prof. Oppolzer. But his result would require for each meteoroid an enormous mass, one far too great to be accepted as possible.

Again, the supposed power of such small bodies,—bodies so scattered as these are, even in the densest streams,—to break up the comets or other heavenly bodies, and also their power, by intercepting the sun's rays, to affect our weather, must, in absence of direct proof to the contrary, be regarded as insignificant. So, too, their effect in producing geologic changes by adding to the earth's strata has, without doubt, been very much over-estimated. During a million of years, at the present rate of, say, 15,000,000 of meteors per day, there comes into the air about one shooting-star or meteor for each square foot of the earth's surface.

To assume a sufficient abundance of meteors in ages past to accomplish any of these purposes is, to say the least, to reason from hypothetical and not from known causes. The same may be said of the suggestion that the mountains of the moon are due to the impact of meteorites. Enormously large meteoroids in ages past must be arbitrarily assumed, and, in addition, a very peculiar plastic condition of the lunar substance, in order that the impact of a meteoroid can make in the moon depressions ten, or fifty, or a hundred miles in diameter, surrounded by abrupt mountain walls two, and three, and four miles high, and yet the mountain walls not sink down again.

The known visible meteors are not large enough nor numerous enough to do the various kinds of work which I have named. May we not assume that an enormous number of exceedingly small meteoroids are floating in space, are falling into the sun, are coming into our air, are swept up by the moon? May we not assume that some of these various results, which cannot be due to meteoroids large enough for us to see as they enter the air, may be due to this finer impalpable cosmic dust? Yes, we may make such an assumption. There exist, no doubt, multitudes of these minute particles travelling in space. But science asks not only for a true cause, but a sufficient cause. There must be enough of this matter to do the work assigned to it. At present we have no evidence that the total existing quantity of such fine material is very large. It is to be hoped that through the collection and examination of meteoric dust we may soon learn something about the amount which our earth receives Until that shall be learned, we can reason only in general terms. So much matter coming into our atmosphere as these several hypotheses require would, without doubt, make its presence known to us in the appearance of our sunset skies and in a far greater deposit of meteoric dust than has ever yet been proven.

A meteoroid origin has been assigned to the light of the solar corona. It is not unreasonable to suppose that the amount of the meteoroid matter should increase toward the sun, and that the illumination of such matter would be much greater near the solar surface. But it is difficult to explain upon such an hypothesis the radial structure, the rifts, and the shape of the curved lines, that are marked features of the corona. These seem to be inconsistent with any conceivable arrangement of meteoroids in the vicinity of the sun. If the meteoroids are arranged at random, there should be a uniform shading away of light as we go from the sun. If the meteoroids are in streams along cometary orbits, all lines bounding the light and shade in the coronal light should evidently be projections of conic sections of which the sun's centre is the focus. There are curved lines in abundance in the coronal light, but, as figured by observers and in the photographs, they seem to be entirely unlike such projections of conic sections. Only by a violent treatment of the observations can the curves be made to represent such projections. They look as though they were due to forces at the sun's surface rather than at his centre. If those complicated lines have any meteoroid origin (which seems very unlikely), they suggest the phenomena of comets' tails rather than meteoroid streams or sporadic meteors. The hypothesis that the long rays of light which sometimes have been seen to extend several degrees from the sun at the time of the solar eclipse are meteor streams seen edgewise, seems possibly true, but not at all probable.

The observed life of the meteor is only a second, or at most a few seconds, except when a large one sends down stones to remain with us. What can we learn about its history and origin?

Near the beginning of this century, when small meteors were looked on as some form of electricity, the meteorites were very generally regarded as having been thrown out from the lunar volcanoes. But as the conviction gained place that the meteorites moved not about the earth, but about the sun, it was seen that the lunar volcanoes must have been very active to have sent out such an enormous number of stones as are needed, in order that we should so frequently encounter them. When it was further considered that there is no proof that lunar volcanoes are now active, and that when they were active they were more likely to have been open seas of lava, not well fitted to shoot out such masses, the idea of the lunar origin of the meteorites gradually lost ground.

But the unity of meteorites with shooting-stars, if true, increases a hundredfold the difficulty, and would require that the comets have the same origin with the meteorites. No one claims that the comets came from the moon.

That the meteorites came from the earth's volcanoes is still held by some men of science, particularly by the distinguished Astronomer-Royal for Ireland. The difficulties of the hypothesis are, however, exceedingly great. In the first place, the meteorites are not like terrestrial rocks. Some minerals in them are like minerals in the rocks. Some irons are like the Greenland terrestrial irons. But no rock in the earth has yet been found that would be mistaken for a meteorite of any one of the two or three hundred known stone-falls. The meteorites resemble the deep terrestrial rocks in some particulars, it is true, but the two are also thoroughly unlike.

The terrestrial volcanoes must also have been wonderfully active to have sent out such a multitude of meteorites as will explain the number of stone-falls which we know, and which we have good reason to believe, have occurred. The volcanoes must also have been wonderfully potent. The meteorites come to us with planetary velocities. In traversing the thin upper air they are burned and broken by the resisting medium. Long before they have gone through the tenth part of the atmosphere the meteorites usually are arrested and fall to the ground. If these bodies were sent out from the earth's volcanoes, they left the upper air with the same velocity with which they now return to it. What energy must have been given to the meteorite before it left the volcano, to make it traverse the whole of our atmosphere and go away from the earth with a planetary velocity. Is it reasonable to believe that volcanoes were ever so potent, or that the meteorites would have survived such a journey?

No one claims that the meteors of the star-showers, or their accompanying comet, came from the earth's volcanoes. To ascribe a terrestrial origin to meteorites is, then, to deny the relationship of the shooting-star and the stone-meteor. Every reason for their likeness is an argument against the terrestrial origin of the stones. To suppose that the meteors came from any planets that have atmospheres involves difficulties not unlike to, and equally serious with, those involved in the theory of a terrestrial origin.

The solar origin of meteorites has been seriously urged, and deserves a serious answer. The first difficulty which this hypothesis meets is that solid bodies should come from the hot sun.

Besides this, they must have passed without destruction through an atmosphere of immense thickness. Then there is a geometric difficulty. The meteorite shot out from the sun would travel, under the law of gravitation, nearly in a straight line out and back again into the sun. If in its course it enters the earth's atmosphere, its relative motion—that which we see—should be in a line parallel to the ecliptic, except as slightly modified by the earth's attraction. A large number of these meteors, that is, most if not all well-observed fire-balls, have certainly not travelled in such paths. These did not come from the sun.

It has been a favourite hypothesis that the meteorites came from some planet broken in pieces by an internal catastrophe. There is much which mineralogists can say in favour of such a view. The studies of M. Stanislas Meunier and others into the structure of meteorites have brought out many facts which make this hypothesis plausible. It requires, however, that the stone-meteor be not regarded as of the same nature as the star-shower meteor, for no one now seriously claims that the comets are fragments of a broken planet. The hypothesis of the existence of such a planet is itself arbitrary; and it is not easy to understand how any mass that has become collected by the action of gravity and of other known forces should, by internal forces, be broken in pieces, and these pieces sent asunder. The disruption of such a planet by internal forces, after it has by cooling lost largely its original energy, would be specially difficult to explain.

We cannot, then, look to the moon, nor to the earth, nor to the sun, nor to any of the large planets, nor to a broken planet, as the first home of the meteoroids, without seeing serious if not insuperable objections. But since some of them were in time past certainly connected with comets, and since we can draw no line separating shooting-stars from stone-meteors, it is most natural to assume that all of them are of a cometary origin. Are there any insuperable objections that have been urged against the hypothesis that all of the meteoroids are of like nature with the comets, that they are in fact fragments of comets, or it may be sometimes minute comets themselves? If such objections exist, they ought evidently to come mainly from the mineralogists, and from what they find in the internal structure of the meteorites. Astronomy has not as yet furnished any objections. It seems strange that comets break in pieces, but astronomers admit it, for it is an observed fact. It is strange that groups of these small bodies should run before and follow after comets along their paths, but astronomers admit it as fact in the case of at least four comets. Astronomically there would seem to be no more difficulty in giving such origin to the sporadic meteor, and to the large fire-ball, and to the stone-meteor, than there is in giving it to the meteor of the star-shower. If, then, the cometic origin of meteorites is inadmissible, the objections must come mainly from the nature and structure of the meteoric stones and irons. Can the comet in its life and history furnish the varied conditions and forces necessary to the manufacture or growth of these peculiar structures?

It is not necessary, in order to answer this question, to solve the thousand puzzling problems that can be raised about the origin and the behaviour of comets. Comets exist in our system, and have their own peculiar development, whatever be our theories about them. It will be enough for my present purpose to assume as probably true the usual hypothesis that they were first condensed from nebulous matter; that that matter may have been either the outer portions of the original solar nebula, or matter entirely independent of our system and scattered through space. In either case, the comet is generally supposed, and probably must be supposed, to have become aggregated far away from the sun. This aggregation was not into one large body, to be afterwards broken up by disruption or by solar action. The varieties of location of the cometic orbits seem inexplicable upon any such hypothesis. Separate centres of condensation are to be supposed, but they are not *a priori* unreasonable. This is the rule rather than the exception everywhere in Nature.

Assume, then, such a separate original condensation of the comet in the cold of space, and that the comet had a very small mass compared with the mass of the planets. Add to this the comet's subsequent known history, as we are seeing it in the heavens. Have we therein known forces and changes and conditions of such intensity and variety as the internal structure of the meteorites calls for? What that structure is, and, to some extent, what conditions must have existed at the time and place of its first formation, and during its subsequent transformations, mineralogists rather than astronomers must tell us. For a long time it was accepted without hesitation that these bodies required great heat for their first consolidation. Their resemblance to the earth's volcanic rocks was insisted on by mineralogists. Prof. J. Lawrence Smith, in 1855, asserted without reserve that "they have all been subject to a more or less prolonged igneous action corresponding to that of terrestrial volcanoes." Director Haidinger, in 1861, said, "With our present knowledge of natural laws, these characteristically crystalline formations could not possibly have come into existence except under the action of high temperature combined with powerful pressure." The likeness of these stones to the deeper igneous rocks of the earth, as shown by the experiments of M. Daubrée, strengthened this conviction. Mr. Sorby, in 1877, said, "It appears to me that the conditions under which meteorites were formed must have been such that the temperature was high enough to fuse stony masses into glass; that the particles could exist independently one of the other in an incandescent atmosphere subject to violent mechanical disturbances; that the force of gravitation was great enough to collect these fine particles together into solid masses, and that these were in such a situation that they could be metamorphosed, further broken up into fragments, and again collected together."

Now, if meteorites could come into being only in a heated place, then the body in which they were formed ought, it would seem, to have been a large one. But the comets, on the contrary, appear to have become aggregated in small masses. The idea that heat was essential to the production of these minerals was at first a natural one. All other known rock-formations are the result of processes that involve water or fire or metamorphism. All agree that the meteorites could not have been formed in the presence of water or free oxygen. What conclusion was more reasonable than that heat was present in the form of volcanic or of metamorphic action?

The more recent investigations of the meteorites and kindred stones, especially the discussions of the Greenland native irons and the rocks in which they were embedded, are leading mineralogists, if I am not mistaken, to modify their views. Great heat at the first consolidation of the meteoric matter is not considered so essential. In a late paper M. Daubrée says:—"It is extremely remarkable that, in spite of their great tendency to a perfectly distinct crystallisation, the silicate combinations which make up the meteorites are there only in the condition of very small crystals, all jumbled together as if they had not passed through fusion. If we may look about us for something analogous, we should say that, instead of calling to mind the long needles of ice which liquid water forms as it freezes, the fine-grained texture of meteorites resembles rather that of hoar-frost, and that of snow, which is due, as is known, to the immediate passage of the atmospheric vapour of water into the solid state." So Dr. Reusch, from the examination of the Scandinavian meteorites, concludes that "there is no need to assume volcanic and other processes taking place upon a large heavenly body formerly existing, but which has since gone to pieces."

The meteorites resemble the lavas and slags on the earth. These are formed in the absence of water, and with a limited supply of oxygen, and heat is present in the process. But is heat necessary? Some crystallisations do take place in the cold; some are direct changes from gaseous to solid forms. We cannot in the laboratory reproduce all the conditions of crystallisation in the cold of space. We cannot easily determine whether the mere absence of oxygen will not account fully for the slag-like character of the meteorite minerals. Wherever crystallisation can take place at all, if there is present silicon and magnesium and iron and nickel, with a limited supply of oxygen, there silicates ought to be expected in abundance, and the iron and nickel in their metallic form. Except for the heat, the process should be analogous to that of the reduction of iron in the Bessemer cupola, where the limited supply of oxygen combines with the carbon and leaves the iron free. The smallness of the comets, should not, then, be an objection to considering the meteoric stones and irons as pieces of comets. There is no necessity of assuming that they were parts of a large mass, in order to provide an intensely heated birth-place.

But although great heat was not needed at the first formation, there are many facts about these stones which imply that violent forces have in some way acted during the meteorites' history. The brecciated appearance of many specimens, the fact that the fragments in a breccia are themselves a finer breccia, the frac-

tures, infiltrations, and apparent faultings seen in microscopic sections and by the naked eye—these all imply the action of force. M. Daubrée supposes that the union of oxygen and silicon furnishes sufficient heat for making these minerals. If this is possible, those transformations may have taken place in their first home. Dr. Reusch argues that the repeated heating and cooling of the comet, as it comes down to the sun and goes back again into the cold, is enough to account for all the peculiarities of structure of the meteorites. These two modes of action do not, however, exclude each other. Suppose, then, a mass containing silicon, magnesium, iron, nickel, a limited supply of oxygen, and small quantities of other elements, all in their primordial or nebulous state (whatever that may be), segregated somewhere in the cold of space. As the materials consolidate or crystallise, the oxygen is appropriated by the silicon and magnesium, and the iron and nickel are deposited in metallic form. Possibly the heat developed may, before it is radiated into space, modify and transform the substance. The final result is a rocky mass (or possibly several adjacent masses), which sooner or later is no doubt cooled down throughout to the temperature of space. This mass, in its travels, comes near to the sun. Powerful action is there exerted upon it. It is heated. How intense is that heat upon a cold rock, unprotected apparently by its thin atmosphere, it is not possible to say. We know that the sun's action is strong enough to develop that immense train, the comet's tail, that sometimes spans our heavens. It is broken in pieces. We have seen the portions go off from the sun, to come back, probably, as separate comets. Solid fragments are scattered from it to travel in their own independent orbits. What is the condition of the burnt and crackled surface of a cometic mass or fragment as it goes out from the sun again into the cold? What changes may not that surface undergo before it comes back again, to pass anew through the fiery ordeal? We have here forces that we know are acting. They are intense, and act under varied conditions. The stones subject to those forces can have a history full of all the scenes and actions required for the growth of such strange bodies as have come down to us. Some of our meteors, those of the star-showers, have certainly had that history. What good reason is there for saying that all of them may not have had the like birth-place and life?

The pieces which come into our air in any recurring star-shower belong to a group whose shape is only partly known. It is thin, for we traverse it in a short time. It is not a uniform ring, for it is not annual, except possibly the August sprinkle. How the sun's unequal attraction for the parts of a group acts as a dispersive force to draw it out into a stream, those most beautiful and most fruitful discussions of Signor Schiaparelli have shown. The groups that we meet are certainly in the shape of thin streams.

It has been assumed that the cometic fragments go continuously away from the parent mass, so as to form, in due time, a ring-like stream of varying density, but stretched along the entire elliptic orbit of the comet. The epochs of the Leonid star-showers in November, which have been coming at intervals of thirty-three years since the year 902, have led us to believe that this departure of the fragments from Tempel's comet (1866, I.) and the formation of the ring was a very slow process. The meteors which we met near 1866 were therefore thought to have left the comet many thousands of years ago. The extension of the group was presumed to go on in the future until, perhaps tens of thousands of years hence, the earth was to meet the stream every year. Whatever may be the case with Tempel's comet and its meteors, this slow development is not found to be true for the fragments of Biela's comet. It is quite certain that the meteors of the splendid displays of 1872 and 1885 left the immediate vicinity of that comet later than 1840, although at the time of those showers they had become separated two hundred millions of miles from the computed place of the comet. The process, then, has been an exceedingly rapid one, requiring, if continued at the same rate, only a small part of a millennium for the completion of an entire ring, if a ring is to be a future form of the group.

It may be thought reasonable in view of this fact about Biela's comet, established by the star-showers of 1872 and 1885, to revise our conception of the process of di integration of Tempel's comet also. The more brilliant of the star showers from this comet have always occurred very near the end of the thirty-three year period. Instead of there being a slow process which is ultimately to produce a ring along the orbit of the comet, it certainly seems more reasonable to suppose that the compact lines of meteors which we met in 1866, 1867, and 1868 left the comet at a recent date. A thousand years ago this shower occurred in the middle of October. By the precession of the equinoxes and the action of the planets, the shower has moved to the middle of November. One half of this motion is due to the precession, the other half to the perturbing action of the planets. Did the planets act upon the comet before the meteoroids left it, or upon the meteoroid stream? Until one has reduced the forces to numerical values, he may not give to this question a positive answer. But I strongly suspect that computations of the forces will show that the perturbations of Jupiter and Saturn upon that group of meteoroids hundreds of millions of miles in length,—perturbations strong enough to change the node of the orbit 15° along the ecliptic,—would not leave the group such a compact train as we found it in 1866. If this result is at all possible, it is because the total action is scattered over so many centuries. But it seems more probable that the fragments are parting more rapidly from the comet than we have assumed, and that, long before the complete ring is formed, the groups become so scattered that we do not recognise them, or else are turned away so as not to cross the earth's orbit.

Comets, by their strange behaviour and wondrous trains, have given to timid and superstitious men more apprehensions than have any other heavenly bodies. They have been the occasion of an immense amount of vague, and wild, and valueless speculation by men who knew a very little science. They have furnished a hundred as yet unanswered problems which have puzzled the wisest. A world without water, with a strange and variable envelope which takes the place of an atmosphere, a world that travels repeatedly out into the cold and back to the sun, and slowly goes to pieces in the repeated process, has conditions so strange to our experience, and so impossible to reproduce by experiment, that our physics cannot as yet explain it. But we may confidently look forward to the answer of many of these problems in the future. Of those strange bodies, the comets, we shall have far greater means of study than of any other bodies in the heavens. The comets alone give us specimens to handle and analyse. Comets may be studied, like the planets, by the use of the telescope, the polariscope, and the spectroscope. The utmost refinements of physical astronomy may be applied to both. But the cometary worlds will be also compelled, through these meteorite fragments,—with their included gases and peculiar minerals,—to give up some additional secrets of their own life, and of the physics of space, to the blowpipe, the microscope, the test-tube, and the crucible.

THE BRITISH ASSOCIATION

SECTION D—BIOLOGY

Initiation of a Discussion upon the Value of the "Type-system" in the Teaching of Botany, by Prof. Bayley Balfour.—The speaker remarked that within the last fifteen years there had been a complete revolution in the method of teaching botany and zoology. The old method was practical teaching based on classification. In fact, in the olden times it was taught by means of object-lessons, which were sporadically chosen. In that method the real significance of plant life was completely overlooked, and also the position of the plants in Nature and their relationship to the animal kingdom. The result was that they had naturalists bred who had a wide range of knowledge of plant forms, and able to recognise and name a great number of plants, but of the life-history and sequence of events they were in the dark. The knowledge was a wide but superficial one. The new system was the natural outcome of the progress of the science, and as more knowledge of the minuter forms were obtained, it became necessary to select individual forms to be made types for special study. Thus by degrees a system of teaching was introduced which consisted in the selection of a few characteristic forms, and those were thoroughly studied in their structural and physiological relationship. Thus accurate knowledge of a few types was obtained, and the work now, instead of being in the field, was transferred to the laboratory. That new method was greatly used at the present time, and promised to be more widely introduced by the publication of new text-books running along the lines of that teaching. The old system he did not think produced good results, but he thought that teaching from types, combined with a certain amount of old teaching, would be effective.

In the discussion which followed, Prof. Bower said that in the elementary schools it would be well to give first the classification of the higher plants, and then, if the students succeeded in that part, they might pass to the more strict laboratory learning.—Prof. Hartog condemned the use of the type-system with children under sixteen, and, referring to the college instruction, lamented that the study of botany should have to be regulated by the requirements of the medical students.—Dr. Trimen thought the type-system was apt to give the students a false impression of the vegetable kingdom. They were apt to think that those types covered the whole matter to be studied. It would be well if the system could be extended. As to the question of medical students, they certainly did not require a complete course of technical botany. The teaching of botany in some of the London schools was a mere farce.—Prof. Marshall Ward remarked that the type-system has done good service to education, and pointed out how necessary it is to obtain exact knowledge from the study of actual objects, and how valuable is the training due to their careful investigation. The types should be real, and not imaginary or badly-selected ones.—Dr. Shaw observed that it would be a great mistake to drop biology out of the curriculum of the medical student.—Prof. Hillhouse pointed out that the type-system gave the student the advantage of commencing with simplicity and working up to complexity. The system, to be successful, must be carefully arranged and the selection of types judicious.

Remarks on "Physiological Selection, an Additional Suggestion on the Origin of Species," by G. J. Romanes, F.R.S., by Henry Seebohm.—This was a criticism of the above paper, and was followed by a short discussion, the general conclusion arrived at being to the effect that the paper referred to does not contribute anything essentially new to the theory of Charles Darwin. In criticising this theory, Mr. Seebohm pointed out that its author not only demanded an impossible number of coincidences, but coincidences of such a character that, once granted, the additional coincidence of fertility *inter se* but sterility outside the family was almost, if not quite, an unnecessary incumbrance to it.

On the Morphology of the Mammalian Coracoid, by Prof. G. B. Howes.—The author seeks to show that the importance of a third centre of ossification of the mammalian coracoid has escaped attention; he claims that it is the representative of the true coracoid bar of the lower vertebrata, the coracoid process being held to answer to the epicoracoid plate of the monotreme. He further upholds the view that the mammalian shoulder-girdle has been derived from a primarily expanded sheet-like form.

Some Experiments upon the Acquisition of an Unpleasant Taste as a Means of Protecting Insects from their Enemies, by E. B. Poulton.—This paper dealt with experiments upon the acquisition of an unpleasant taste as a means of protecting insects from their enemies. The author remarked that Darwin thinking of the use of colour in animals, and deciding that it was of use in courtship, came across the bright colours of caterpillars, which were sexless. He directed Wallace's attention to the subject, and he ventured a prediction that the bright colours would be associated with an unpleasant taste or smell, so that lizards, &c., refused to eat them. Experiments proved that this was correct, but, on thinking the subject over, it seemed to the writer that some limitations were required. If an insect was distasteful to a lizard, the former would either be starved or would have to put up with an unpleasant taste. It might probably acquire a relish for what hitherto was disagreeable, and then the distasteful organisms being brilliant and conspicuous would be easily caught and exterminated. Mr. Poulton therefore determined to experiment upon them, believing that it would be found that protection by a disagreeable taste was not so complete as was supposed. He obtained lizards from Italy, but found that that was the case. They often refused an insect at first, and took it afterwards unless they were fed on other things which they liked better. It was found that the small lizards refused a large moth, such as the privet hawk, although entirely harmless and undoubtedly palatable. The larger lizards disposed of it at once, and the former were evidently afraid of it, from its size bearing some comparison to their own. Further, the brilliant black and red moth, the cinnabar, was eaten by the tree-frog, and a second specimen was eaten directly afterwards. It was quite clear that the frog did not dislike the taste, but the moths disagreed with the frog, and they were afterwards found floating in the aquarium. The moth of the buff tip, which was protected by resembling a piece of broken rotten wood, was evidently disliked by the lizards, although they ate it in the end. In some cases disagreeable insects were eaten with a relish by those particular animals, such as the larvæ of the common Crœsus found on birch. The protection was therefore less perfect than was supposed to be the case.

On the Germination of the Spores of "Phytophthora infestans," by Prof. Marshall Ward.—One of the objects of this communication was to bring before the meeting copies of some careful drawings of all the stages of germination. These were obtained by actually watching the development, escape, and germination of the zoospores from the "conidia," following all the phases in *one* individual. The curious effects of light and of abnormal conditions upon the development of the zoospores were also pointed out, and the author showed diagrams of other forms of germination obtained by interfering with the conditions. In the short discussion which followed Prof. Marshall Ward referred to some points in the development and escape of the zoospores of the Saprolegnia.

On the Flora of Ceylon, especially as affected by Climate, by Henry Trimen, M.B., F.L.S.—Attention was first called to the fact that the Island of Ceylon was practically known to Europeans only by its south-west part, being about one-fifth of the whole area, but including the chief European centres, the planting districts of the hills, and the railway system. The remainder of the country is thickly covered with jungle, thinly inhabited, and rarely visited by Europeans, save Government officials and sportsmen. This difference was shown to be due to climate, especially to rainfall. The distribution of the rain, so far as is shown by annual amount, was exhibited by a map, in which the great advantage to the south-west of the lofty forest-clad escarpment of the central mountain-mass of over 7000 feet was exhibited. The south-west monsoon wind commencing at the end of May deposits an immense quantity of rain here, especially in the neighbourhood of Adam's Peak. In the rest of the island this wind becomes dry, and the country is parched and arid until the arrival of the north-east monsoon, which commences in October. This wind brings rain to the whole island, and is the only rain which the dry districts get; in many places it all falls in a few weeks, when the country is completely under water, though parched with drought for the rest of the year. This is very different to the well-known south-west of Ceylon, where, save in February or March, a fortnight's drought is a very rare event. In some parts over 200 inches falls in the year. In these respects Ceylon is an epitome or continuation of the Southern Indian peninsula. The peculiarities of the flora were then gone through in some detail, taking first the low country of the wet districts up to 3000 feet—in which the number of introduced tropical plants was commented upon; then of the lower hills, the principal home of the planting enterprise and tea and coffee estates; and next of the higher or true mountain districts above 5000 feet. In the low country the forest has been much destroyed by the indolent and improvident native mode of cultivation called "chena," and but little virgin forest remains in this portion of Ceylon. From 3000 to 5000 feet the agent of destruction has been European planting, and the forest has almost wholly disappeared. Above 5000 feet, land is no longer sold by Government. Attention was specially called to the concentration of endemic species in this wet district—over 800, or nearly 30 per cent. of the whole flora—and to the strongly Malayan, as distinguished from Peninsular Indian, type of these and of the whole flora. There are no Alpine plants in the Ceylon hills; dense forest covers their summits, but a number of temperate genera are represented. This flora is entirely Indian in type, with no *genus* represented which is not also found in the Nilghiris, but the number of endemic *species* is very remarkable, only about 200 being common to both mountain-ranges. A few remarks were then made upon the naturally open grass lands, called "patanas," in the hills, and their peculiar vegetation. The flora of the great dry tracts of Ceylon was then considered. It is completely distinct from that already considered, being mainly the same as that of the Carnatic or Coromandel coast of India, with no Malayan admixture, and very few endemic species. The whole country is covered with forest, apparently primæval; but in reality much of it is secondary, and not more than 800 or 1000 years old, as is reported by native tradition, and evidenced by the vast remains of temples, tanks, and ancient buildings now overgrown with trees. Most of the timbers of importance in trade are obtained in these districts, and, owing to a very faulty forest conservancy,

there is now but little first-class timber remaining, save in very remote places. The botanical characters of this forest, which is everywhere evergreen, were given; and the paper concluded with a few remarks on the coast flora, which is very uniform throughout the tropical belt of the world.

On "Humboldtia laurifolia" as a Myrmecophilous Plant, by Prof. Bower.—It had been found that there were considerable numbers of plants in tropical countries which were pre-eminently associated with ants. The Italian botanist Picari propounded a general view with regard to the subject that the association was mutually advantageous to the ants and to the plants. He found that the plants gave shelter to the ants, and in certain cases supplied them with food. No one would deny the statement that the relation was advantageous to the ants themselves, but the converse case was not so clear. In some cases it had been found that the ants served to protect the plants, and drove off other insects. Picari also pointed out that in certain cases the plants derived nutriment from the excreta of the ants, but whether that was the case was a view open to considerable discussion. He (Prof. Bower) had come to the conclusion that the ants derived all the benefit, and that there was no advantage to the plants. Not only were the ants provided with a capital lodging, but it might be fairly assumed that from the glands of the plants the insects derived food as well.

On the Artificial Production of a Gilded Appearance in Chrysalises, by E. B. Poulton.—The author remarked that some years ago Mr. T. W. Wood brought before the notice of the Entomological Society of London some proofs that certain chrysalises imitated the colour of the surfaces upon which they threw off their caterpillar skin. The intimation was received with some amount of credulity by leading entomologists, but evidently without sufficient reason. For some years the writer had been working upon the colour of caterpillars in relation to the colour of their surroundings, and he had shown that the colour could be modified in one generation by the alterations of their surroundings. It seemed certain that through some sensory surface, possibly the eye, caterpillars were affected by their external relations, and a corresponding effect was produced in colour. Mr. Wood's experiment was but a special case of some general method of production. He explained the results by supposing that the moist surface of a fresh chrysalis was photographically sensitive to the colour of surrounding surfaces. That appeared to be merely a metaphor, and was unsupported by proof. It was more probable that the colour was produced by the effect upon the caterpillar before it turned to the chrysalis. Experiments were therefore made by the writer to put the fact itself beyond dispute. That was done first by the use of caterpillars of the peacock butterfly and the common tortoiseshell butterfly. It was found that by allowing them to turn to chrysalises upon a white or a black screen very different results were produced. Those upon white paper were often brilliantly golden, although the chrysalises of the tortoiseshell were not quite so golden. Gilded specimens were sometimes found, but their appearances seemed to be produced as a disease. While that was the case of chrysalises found in the fields, the specimens experimented with by the writer were perfectly healthy, and produced healthy butterflies. He then saw that, although a white paper produced a golden appearance, a gilded surface would produce the same effect to a greater extent. That bore in a most important manner on the use of the metallic tints of many of the exposed chrysalises of butterflies: which were thus seen to have harmonised with some metallic surroundings. The next point was to ascertain the period during which the caterpillar was sensitive to the colour of the surrounding surfaces, and the nature of the surface which was affected. The former end was achieved by carefully watching the caterpillars between the time at which they ceased feeding and that at which they turned to chrysalises. It was found that they were sensitive for many hours, even more than a day, before the change took place. The other object was attained by placing the larvæ suspended downwards for ten or twelve hours before the change took place in a tube, of which the upper part was golden and the lower black, the two being separated by a perforated disk. The caterpillar's head was turned round so that it could not see through the aperture, and the result showed that the chrysalises were the colour of the chamber in which the head was placed. Hence it seemed that the sensory surface must have existed upon that area. The full results, however, had not yet been obtained.

The Nervous System of Sponges, by Dr. R. von Lendenfeld.—The author gives an account of his discoveries on this subject up to date. Sensitive and ganglia cells have been observed by him in a good number of sponges. Their locality varies, their shape is constant. They are mesodermal, and appear to preside over the movements of the membranes and pore-sieves, and so regulate the water current. The great difference between sponges and higher cœlenterates is, that in the former the most important organs are mesodermal, whilst in the latter they are ecto- or ento-dermal. He divides the type Cœlenterata accordingly into *Cœlenterata Mesodermalia*, or sponges, and *Cœlenterata Epithelaria* or Cnidaria, as Lulitypes.

The Function of Nettle-Cells, by Dr. R. von Lendenfeld.—The author gives a detailed account of the structure of the nettle-cells, or cnidoblasts, and discusses some biological facts regarding their function. He comes to the conclusion that the nettle-cells are exploded by direct reflex action when the cnidocil is touched, but that the animal can counteract this reflex action by a centrifugally acting nervous irritation in a similar manner as reflex actions are controlled by higher nervous centres in man.

Note on the Floral Symmetry of the Genus Cypripedium, by Dr. Maxwell T. Masters, F.R.S.—In this note the author adverts to so much of the normal structure of Orchids in general, and of Cypripedium in particular, as is necessary for the elucidation of his subject, and proceeds to describe a case of regular peloria in *Cypripedium caudatum*, which shows a reversion to the typical form of Orchids, and goes to prove that the so-called genus Uropedium is only a pelorian form of Cypripedium. The construction of the andrœcium in these plants is then alluded to, and illustrations given of all intermediate stages from monandry to hexandry. The frequently observed tendencies to a dimerous condition, and to the development of the inner row of stamens, are alluded to, and the significance of these changes pointed out. The morphological changes consequent upon hybridisation, and the inferences to be derived from them, are passed under review. The paper concludes with a general summary of the teratological changes observed in the tribe Cypripedieæ.

Notes on Australian Cœlenterates, by Dr. von Lendenfeld.—The author describes the extraordinary mode of development of *Phyllorhiza punctata*, a rhizostomous Medusa discovered by him in Port Jackson. The Ephyra has eight, the next stage twenty-four, the next sixteen, and the adult again eight marginal bodies. If the umbrella margin is injured and newly formed, marginal bodies appear between *all* the newly-formed flaps. Further, the migrations of *Crambessa masaica* at the breeding time are described. This and other species of that genus of rhizostomous Medusæ migrate far up the rivers, like the salmon, to deposit their young. A remarkable change in the colour of *C. masaica*, which has taken place in Port Jackson since the observations of Huxley about fifty years ago, is described. A new variety, which is brown, seems to have been produced or to have immigrated and superseded the *blue* form, which was observed by Huxley and others in that locality. In Port Phlilip the blue variety is still found. The author has found in examining the lower freshwater animals that the freshwater Hydroids and Sponges, as also the freshwater Rhizopoda of Australia, are very similar to the European, whilst the *marine* species of these groups differ very much in the two localities. He concludes that these freshwater forms are very old and conservative, and may be supposed to be the unchanged offspring of old ancestral forms, as such possessing particular systematic importance.

Bugio; the Biological Relations of an Atlantic Rock, by Michael C. Grabham, M.D., F.G.S., F.R.C.P.—Region almost unknown, but interesting as being typical of flora distribution, and of variation in isolation. Author proposed to illustrate present knowledge by reference to prominent forms, animal and vegetable, existing at Bugio, the most unknown of the Dezerta islets.

Dezerta.—Physical characters: Foundation on a narrow ledge; dimensions never much greater; no evidence of ancient contact; not survivals of an ancient continent, but islands in a Miocene sea, deriving their first colonists from Miocene Europe.

Description of Bugio.—Difficulty of access; central volcanic dyke; large proportion of tufas; no sections of old river-beds or surface obliterations; summit showed deep clay-beds and surface deposits of calcareous sand and earth.

Flora.—How related to Madeira; arbitrary distribution; absence of ea-ily wafted forms; *Senecio incrassatus*, Madeiran and Canarian varieties; *Echium fastuosum*, maritime form of; hybrid with *E. simplex*, remarkable perpetuation of perennial growth, and other changes; several instances of fitful distribution; *Chrysanthemum dæmatomma*, a distinct and only species; remarks on cognate Madeira forms; *Monizia edulis*, Dezertan, Salvagic, and Madeiran examples; Miocene origin of.

Fauna.—Rabbit, identical with that of Porto Santo, described by Darwin as having acquired specific characters in shortened length, and colour of skin. *Sea-birds* breeding at Bugio: *Sternus hirundo, Thalassidroma bulwerii*, and many others. *Procellaria anglorum* dominant to the exclusion of *P. major* and *P. obscura*. Influence of birds in migration of plants and mollusks. *Testacea:* Distribution and affinities—*Helix crystallina*, affinities of; *H. erubescens*, distribution of; *H. punctulata*, modification of; *H. Leonina*, area of, and relations; *H. vulgata*, dwarfed example of; *H. polymorpha*, distinct races of; connections of *H. tiarella, H. coronula*, and *H. grabhami*. *Coleopterous Deuca'ion*, isolated species, now related to a Salvagic form.

Summary.—Showing the difficulties attending the determination of the origin and migration of species to be equally great in the component rocks of a group of islands as in the archipelago itself. Agency of man, chiefly in extinction and destruction, illustrated by introduction of opposing or contaminating forms; ravages of Eupatoria and *Phylloxera vastatrix* in Madeira; surviving vigour of Miocene plants. Author's paper only meant to be indicative of those branches and details which might singly occupy the attention of the Section.

The Multiplication and Vitality of certain Micro-organisms, Pathogenic and otherwise, by Percy F. Frankland, Ph.D., B.Sc., F.C.S., F.T.C., Assoc.Roy.Sch.Mines.—In this paper the author records a number of experiments which he has carried out on the multiplication of the micro-organisms present in natural waters, and also on the vitality of certain pathogenic organisms when purposely introduced into similar media. These phenomena have been studied by aid of the method of gelatine-plate cultivation, originally devised by Koch. The first part of the paper treats of the influence of storage in sterilised vessels, upon the number of micro-organisms present in the unfiltered water of the Rivers Thames and Lea, in the waters of these rivers after sand filtration by the companies supplying the metropolis, and in deep-well water obtained from the chalk. Of these three different kinds of water, at the time of collection the unfiltered river-waters are the richest in micro-organisms, containing, as they do, several thousand microbes, capable of being revealed by plate-cultivation, in 1 cubic centimetre of water, whilst the filtered river-waters have this number generally reduced by about 95 per cent., and the number present in the deep-well water rarely exceeds ten per cubic centimetre. On storage in sterilised vessels at 20° C., however, a great change in the relationship of these numbers soon takes place, for whilst the number of organisms in the crude river-water undergoes but little change, or even suffers diminution, that in the filtered river-water exhibits very rapid multiplication, and this increase is even still more marked in the case of the deep-well water. The author suggests that the differences in the rate of multiplication exhibited by these three kinds of water is dependent upon the number of different varieties of micro-organisms which they contain. Thus in the unfiltered river-waters the organisms belong to a number of different kinds; the filtered river-waters exhibit fewer varieties; whilst in the deep-well water the number of varieties is still more limited, the gelatine-plates having generally the appearance of almost pure cultivations. The microbes in the deep-well water will thus be less hampered in their multiplication by hostile competitors than those in the filtered river-waters, and these again less than those in the crude river-waters, in which an equilibrium must have already been established between the various competitors. When the waters were exposed to a temperature of 35° C., the multiplication was in all cases very much more rapid, but both at 20° C., as well as at 35° C., the multiplication was, on prolonged storage, followed by reduction. The pathogenic forms which have been studied by the author are: (1) Koch's "*Comma*" *spirillum* of Asiatic cholera, (2) Finkler-Prior's "*Comma*" *spirillum* of European cholera, and (3) the *Bacillus pyocyaneus*, which produces the greenish-blue colouring-matter frequently present in abscesses. The vitality of these organisms has been studied by introducing minute quantities of their cultivations into sterilised distilled water, deep-well water, filtered Thames water, and London sewage. In these media they present some very striking differences. Thus the *Bacillus pyocyaneus* was found to flourish in all; even in distilled water it was present in largely multipled numbers after fifty-three days. Koch's "*Comma*" *spirillum*, on the orher hand, when introduced into deep-well water was no longer demonstrable after the ninth day, whilst in sewage it was still found in enormously multiplied numbers after twenty-nine days. Finkler-Prior's "*Comma*" *spirillum*, although showing such far greater vital activity than Koch's in gelatine cultures, possesses far less vitality than the latter when introduced into water. Thus in the above-mentioned media it was in no case demonstrable after the first day.

SCIENTIFIC SERIALS

American Journal of Science, September.—A post-Tertiary elevation of the Sierra Nevada, shown by the river-beds, by Joseph Le Conte. In further elucidation of his already published speculations regarding an upheaval of the Sierra Nevada towards the close of the Tertiary epoch, the author here brings forward much additional evidence, also correlating this movement with a contemporaneous elevation in other parts of the western half of the continent. He endeavours to show that the upward movement, which seems to have affected all high latitude regions at that time, but which was oscillatory and therefore temporary on the eastern side of North America and in Europe, on the Pacific slope was permanent, and has largely determined the orographic structure of that region.—The strain effect of sudden cooling, as exhibited by glass and by steel (second paper), by C. Barus and V. Strouhal. In their first communication the authors compared the strains experienced by glass and steel on sudden cooling, by aid of the density variations observed when the bodies carrying strain were annealed, *as a whole*. Here they seek to confirm their earlier inference relative to the temper-strain of glass. They also investigated the density-relations of consecutive similar shells of the Prince Rupert drop, and the optical character of the successive cores. In general it is shown that the optical effect of the temper-strain in glass may be regarded as the analogue of the electrical effect of the temper-strain in steel. In a further communication a more specific inquiry will be made into the causes of hardness itself, with a view to throwing some light on the mysterious transformations of carbon.—Devonian Lamellibranchiata and species-making, by Henry S. Williams. In connection with the publication of Prof. James Hall's monograph on Devonian Lamellibranchs, completing vol. v. part 1 of the "Palæontology of New York," it is pointed out that fossil species, and even genera, are unduly multiplied on totally inadequate data. Species and genera cannot be regarded as established so long as the author himself is unable to distribute the typical specimens, twice alike, without reference to the original labels.—Note on the composition of certain "Pliocene sandstones" from Montana and Idaho, by George P. Merrill. While lately classifying the rocks collected in Montana and Idaho by Dr. A. C. Peale in 1871, the author's attention was called to some fragments labelled as "Pliocene" sandstones. A glance, however, showed that they strongly resembled compacted volcanic dust and sand, and a microscopic examination made it evident that the stones consisted very largely of minute flakes of pumiceous glass sufficiently compacted to be readily broken out into hard specimens, but extremely friable. The specimens are fully described and some speculations offered as to their probable origin. It is added that in Kansas and Nebraska these dusts are collected and sold as "diamond polishing powder," or used in the preparation of the so-called "geyserite" scouring-soap.—Contributions to mineralogy, by W. Earl Hidden, with crystallographic notes by A. Des Cloizeaux. The paper deals with the ipodumene, black tourmaline, xenotime, and twin crystals of monazite from North Carolina; a remarkable crystal of herderite found in 1884 near Stoneham, Maine; a twin crystal of molybdenite from Renfrew, Canada; and the phenacite from Florissant, El Paso County, Colorado.—Turquois from New Mexico, by F. W. Clarke and J. S. Diller. A full analysis and microscopic study is given of some specimens from the turquois mines of Los Cerillos, New Mexico, about 22 miles south-west of Santa Fé. The turquois-bearing rock appears to be eruptive, and probably of Tertiary age, while the small size of the veins and their limited distribution show that the turquois is of local origin, possibly the result of alteration of some other mineral.—On the electrical resistance of soft carbon under pressure, by T. C. Mendenhall. In reply to Prof. Sylvanus P. Thompson's objections, the author describes some fresh experiments fully confirming his views regarding the change in the resistance of carbon due to change of pressure. In the form of compressed lampblack the electrical resistance of carbon varies greatly with the pressure to which it is subjected, and the variation is mainly due to a real change in the resistance of the carbon itself.—Com-

parison of maps of the ultra-violet spectrum, by Edward C. Pickering. Prof. Rowland's recently published photograph of the solar spectrum is compared with Draper's map of the ultra-violet portion of the spectrum prepared in 1873, with which it is shown to agree very closely. The mean difference for the seventy-six lines compared was 0·012, corresponding to about 1/800 inch upon the Draper map. It may therefore be assumed that the probable error of a wave-length derived from this map will not exceed 1/100 unit if the correction here given be first applied.—On two hitherto undescribed meteoric stones, by Edward S. Dana and Samuel L. Penfield. One of these meteorites was found, in 1869, between Salt Lake City and Echo, Utah; the other, in 1846, near Cape Girardeau, South-West Missouri. Olivine is the most prominent constituent of the former, while the latter is a light gray chondrite.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, September 20.—M. Fizeau in the chair.—Kinematic analysis of human motion, by M. Marey. In the figure accompanying this paper are represented the successive attitudes of the lower right limb while describing a complete step. This action is shown to be divided into two periods, a rest and a rise, which are again subdivided into four unequal phases, of which the last three belong to the period of rise. The simultaneous movements of ankle, knee, and hip are explained, and it is pointed out that, whatever be the velocity of the pace, the form of the various trajectories here described is maintained in their salient features. But, the more rapid the motion, the more is the tendency of the centre of gravity to approach a straight line parallel with the surface of the ground. —"Modern Kinetics and the Dynamism of the Future," by M. G. A. Hirn. This is the title of a new work, which the author presents to the Academy with some remarks explaining its general purpose. After replying to the various objections raised against his general principles, he deals with the arguments which, as he maintains, render henceforth indefensible the kinetic theory of the gases, referring to molecular movements most of the properties of these bodies. Three arguments are advanced of such a nature that he believes future physicists will wonder how this kinetic theory could ever have been accepted for a single moment. Even were it correct, it would not follow that light, radiant heat, electricity, magnetic attraction and repulsion, and gravitation were due to movements of ponderable matter, far less that thought itself was nothing more than a molecular movement. But the reverse is not true, so that with the collapse of the kinetic theory of the gases fall the kinetic theories in general, which claim to explain all possible phenomena of the universe by invisible movements of matter. The doctrine here substituted for kinetic force, he thinks, explains quite as easily, and much more rationally, the universal phenomena of the physical world. He does not, however, hope at once to convince all minds of what they should have long ago been themselves convinced. Interpretations formulated *a priori*, and apart from experience and observation, have unfortunately more vitality than truths gained to science by the patient study of Nature.—Observations of Winnecke's comet made at the Observatory of Nice (Gautier equatorial), by MM. Perrotin and Charlois. The results of these observations, which extend over the four days from August 27 to September 1, are embodied in tables showing the positions of the stars 25339 Lalande, 25588 Lalande, 4989 Schjellerup, 5004 Schjellerup, and the apparent positions of the comet.—On the transformation of algebraic surfaces in themselves, by M. Emile Picard. A proposition analogous to that of Schwarz is thus formulated: Algebraic surfaces capable of being transformed in themselves by a bi-rational substitution, including two arbitrary parameters, are of the genus zero, or one.—On a class of differential non-linear equations, by M. Roger Liouville.—Historical note on a series whose general term is of the form $A_n (x - a_1)(x - a_2) \ldots (x - a_n)$, by M. G. Eneström.—Researches on the structure of the nerve-centres in the Arachnidæ, by M. G. Saint-Remy. Having in a previous communication dealt with the structure of the brain of the scorpion, the author here extends his observations to the spider family, and more particularly to *Tegenaria domestica*, *Epeira diadema*, and *Phalangium opilio*. In these groups he shows that the brain offers the same plan of organisation as that of the Scorpionidæ.—Fresh researches on the configuration and extent of the Carmaux Coal-measures, by MM. Alfred Caraven-Cachin and Grand. In this basin, which extends for nearly six miles from Rozières to Saint-Quentin, there are in some districts three successive coal-deposits with a joint thickness of over 31 metres underlying Tertiary formations 156 metres thick. They appear to have been deposited horizontally, always in shallow water, the land subsiding sometimes slowly, sometimes intermittently, during the whole period of their formation.—Note on the affinities of the Oolitic floras in the West of France and in England, by M. L. Crié. In this paper the author communicates the first results of his studies of the Oolitic floras of these regions. The conifers are represented at Mamers (Sarthe) and at Scarborough (Yorkshire) by traces of Brachyphyllum, which present a remarkable identity. Certain imprints at Scarborough also show a strong resemblance, in the disposition of the foliage, and especially in the veinous system, to *Otozamites marginatus*, Sap., which is so characteristic of the Mamers flora. About the middle of the Oolitic period this group must have covered certain upheaved tracts in the Venetian Alps, in the neighbourhood of Mamers, and at Scarborough.—The waterspout of September 14 at Marseilles, by M. Barthelet.

BOOKS AND PAMPHLETS RECEIVED

"How Readest Thou? or the First Two Chapters of Genesis," by E. Dingle (Partridge and Co.).—"The Chalk and Flint Formation," by W. B Galloway (Low and Co).—"Life-History of Plants," by Prof. D. M'Alpine (Sonnenschein)—"Tobacco: a Farmer's Plant," by P. M. Taylor (Stanford).—"Therapeutics founded upon Organopathy and Antipraxy," by W. Sharp, M.D. (Bell and Sons).—"Report of the Iowa Weather Service, January to April 1883," by Dr. G. Hinrichs.—"Scientific Romances: No. v. Casting out the Self," by C. H. Hinton (Sonnenschein).—"Lessons in Elementary Dynamics," by H. G. Madan (Chambers).—"Studies in Ancient History," N.E., by J. F. McLellan (Macmillan).—"Manual of the New Zealand Coleoptera," parts 3 and 4, by Capt. T. Brown (Didsbury, Wellington).—"School of Forest Engineers in Spain," by Dr. J. C. Brown (Oliver and Boyd).—"Hand-book of Mineralogy," by J. C. Foyr (Van Nostrand, N.Y.).—"Monographs U.S. Survey," vol. ix. (Washington).—"Hommage à M. Chevreul, à l'Occasion de son Centenaire" (Alcan, Paris).—"The Handy Natural History," by J. G. Wood (Religious Tract Society)—"General Report on the Operations of the Survey of India Department," 1884-85, by Col. G. De Prée (Calcutta).—"Notes on the Bones of a Species of Sphenodon," by W. Colenso.—"The Economical Aspects of Agricultural Chemistry," by H. W. Wiley (Wilson, Camb., Mass.)—"Report on the Decapod Crustacea of the *Albatross* Dredgings off the Coast of the United States," by S. I. Smith (Washington).—"Metodo per Misurare la Dilatazione Termica dei Corpi Solidi: Memoria di F. Artimini (Firenze).—"The Cause of Electricity, with Remarks on Chemical Equivalents," by G. T. Carruthers (Benares).

CONTENTS

THURSDAY, OCTOBER 7, 1886

ORCHIDS

Reichenbachia: Orchids Illustrated and Described. By F. Sander, assisted by Eminent Scientific Authorities. Magnificently Illustrated in Colours. (London: Sotheran and Co, 1886.)

THE first two parts of a new illustrated work on orchids are now before us. It is called "Reichenbachia," in honour of Prof. Reichenbach, of Hamburg, our greatest living authority on the Orchideæ. The author of the work, which is to be published in monthly parts in an "ordinary," and an "imperial" edition limited to one hundred copies, is Mr. F. Sander, a well-known importer of, and dealer in, orchids at St. Alban's. While justice should be done to the author's energy and enterprise in undertaking so sumptuous and, so far as it has gone, so promising a production, it should be remembered that his business interests are connected with it.

Each part of the larger edition contains four plates in imperial folio, with botanical and horticultural descriptions in English, French, and German, geographical and cultural details being also given. The botanical descriptions are by Prof. Reichenbach, who is responsible also for the dissections. Why, we may ask, are the descriptions of the dissections sometimes in English and sometimes in Latin? This query leads to the remark that the custom of giving botanical descriptions in Latin has led to the creation of what we venture to designate as a most extraordinary, barbarous, and unintelligible jargon. In this work, as in any other botanical book where the descriptions are in Latin, words are to be found in numbers which are in no sense Latin. Scientific descriptions may require the invention of words or terms; but the supposed convenience of their being understood, when in Latin, by men of science of all nations is hardly a sufficient justification for the wholesale creation of such a language. We may add that we submitted one of the Latin descriptions in this work to the head master of a great school, who was unable even to suggest a meaning for some of the terms.

The plates in "Reichenbachia" are unquestionably superior, taking them one with another, to those in any modern botanical work we are acquainted with. They are far in advance both in drawing, and in truth and delicacy of colouring, to the "Orchid Album" of Messrs. B. S. Williams and Sons, or the "Lindenia" of the Continental Horticultural Company of Ghent. They do great credit to Mr. H. G. Moore, one of the best of our young horticultural artists, and to Mr. J. L. Macfarlane, whose work as a natural history lithographer it would be difficult to surpass. We are disposed to take exception to a remark made by Prof. Reichenbach, under Tab. I., *Odontoglossum crispum*, that Bateman's "Monograph of Odontoglossum" is "perhaps the finest book on orchids that has ever been issued." In our judgment it will not compare with the earlier work by the same author, the splendid "Orchidaceæ of Mexico and Guatemala." Of the eight plates in the two parts under review we consider Tab. IV., "*Odontoglossum Rossii rubescens*," the truest to nature, as well as the most artistic. The least satisfactory is Tab. VI., *Calogyne cristata maxima*," in which the hairs on the tip, though shown in the dissection, are not even suggested. We must here, with all respect and deference to Prof. Reichenbach, demur to the varietal name *maxima*. The flowers of this variety may be a little larger than the type, but if it be named *maxima*, what are we to call a larger variety, should one turn up, as is by no means unlikely? These superlatives are more in keeping with the aims and objects of trade than with those of scientific nomenclature. The nurseryman naturally revels in such adjectives as "superbissima," "brilliantissima" (!), "delicatissima," "magnifica," &c., but science should repudiate them. Tab. III. represents a recently introduced Venus's Slipper, from the Malayan Archipelago, called *C. Sanderianum*, which has not been seen in flower, we believe, outside Mr. Sander's nursery. We hope the remarkable drawing does not do more than justice to it. The plant is especially interesting in that it is an almost exact Eastern counterpart of the now well-known *Cypripedium caudatum* from South America, which was first flowered by the late Mrs. Lawrence, of Ealing Park, about fifty years ago. Tab. I. represents the most beautiful and popular, and what is now the commonest and cheapest of the Andean cool orchids, *Odontoglossum crispum*. We cannot admit that there is any scientific distinction maintainable between *O. crispum*, *O. Bluntii*, and *O. Alexandræ*, such differences as there are being purely horticultural. In view of the latest achievement of the busy laboratory of Messrs. Veitch and Sons, where during recent years more species of one genus, Cypripedium, have been created than have been gathered for us from nature by the whole army of collectors, botanists will have to reconsider, it would seem, not only the species of orchids, but the genera. Messrs. Veitch have now in flower a hybrid between *Sophronius grandiflora* and *Cattleya intermedia!* At the Conference last year, and again at the recent Provincial Show of the Royal Horticultural Society at Liverpool, the attention of botanists and horticulturists was drawn to the confusion into which the nomenclature of orchids had fallen, a confusion rapidly becoming worse confounded. We have examples of this in *Odontoglossum crispum* with its many synonyms and varieties, and in *Cattleya Dowiana*, Tab. V., which was re-christened *C. aurea*, simply because it was found in a new habitat. A new name should never be accepted, any more than a new genus or species, unless it be stamped with the approval of a recognised botanical authority. At present the latest and rawest recruit among the rapidly increasing band of orchid growers thinks nothing of coining a new specific or varietal name, which generally takes the shape of a supposed Latinisation of his own name.

The cultural directions are generally judicious. It is impossible to lay too much stress upon the necessity of giving orchids, whether Mexican or East Indian, the period of rest they have in nature. Mr. Sander proscribes houses with a north aspect for cool orchids. Some of the finest Odontoglossa and cool Oncidia we ever saw were grown in a house facing due north. We cannot indorse the recommendation of cocoa-nut fibre refuse or peat-moss manure either on or under stages. They rapidly decay, and become covered with fungi, and full of wood-lice and

other pests. One of the great difficulties in growing orchids and other plants under artificial conditions is due to the injury caused by insects. Any one with a knowledge of chemistry and of vegetable physiology would find, we believe, a profitable field of inquiry in this direction. Amateurs and the trade alike have dealt with this difficulty by rule of thumb for generations, and confined themselves to smoking plants with tobacco in various shapes, and treating them with quack insecticides. So far as tobacco-smoke is effectual, its effect is probably due to the nicotine it contains. There can be no insuperable difficulty in charging the air of a closed house with nicotine fumes sufficiently to destroy insects, and in thereby getting rid of the pungent and injurious smoke produced by burning coarse tobacco and brown paper. In conclusion we may express a hope that this work will not come to a premature end, like some of its predecessors, but live to fulfil the promise of the parts here noticed.

ARC AND GLOW LAMPS

Arc and Glow Lamps. A Practical Hand-Book on Electric Lighting. By Julius Maier, Ph.D. (London: Whittaker and Co., and G. Bell and Sons, 1886.)

WE should have been glad if it had been possible to speak more favourably of Dr. Maier's work than can be done after a conscientious reading of it; for Dr. Maier has made himself so thoroughly master of our language, and has taken such obvious pains to acquaint himself with the literature of electric lighting, that we cannot help wondering how so able a man has produced such a disappointing treatise. Much of the work appears to have been translated from Merling's and other German books on electric lighting. Perhaps it is to this composite origin that the defects are due which a reviewer is bound to point out.

The first 82 pages are occupied by generalities such as the laws of production of heat in the circuit, the efficiency of dynamos, electric and photometric measurements. Then come 60 pages upon arrangements of leading wires and of lamps in installations for electric light, including the so-called "secondary generators" or induction coils for distributing alternating currents. At p. 140 we at last reach arc lamps, the principal types of which are described with care. Twelve pages are allotted to the now almost obsolete electric "candle," and at p. 263 we enter upon the glow lamps. These are described all too briefly, especially so far as relates to the details of manufacture; but the data as to tests of efficiency and durability of the lamps are most satisfactorily summarised. This chapter includes an abstract of the tests made by the Philadelphia Committee, whose method of testing the lamps and of deducing the "mean spherical intensity" of the illumination is perhaps more scientific than that of any of the numerous Exhibition committees who have reported on electric lamps. The book concludes with special chapters on the application of electric light to lighthouses, ships, mines, railway trains, photography, and operative surgery.

There are several contradictory statements in Dr. Maier's book. On p. 270 he tells us that after the inventions of Greener and Staite in 1846 electric lighting fell completely into oblivion till 1873, when Lodyguine took up the question. Yet on p. 148 we find a description of improvements made in 1857 by Lacassagne and Thiers, and on p. 362 we find that electric light has been used for stage purposes ever since the production of Meyerbeer's opera of "Le Prophète" in 1846. The records of the English Patent Office between 1846 and 1873 show abundant evidence to negative Dr. Maier's statement. In that interval came the invention of the lamps of Serrin, Chapman, Way, and Browning, and the successive improvements of Holmes, Siemens, the Varleys, Wheatstone, Wilde, and Gramme in the magneto- and dynamo-electric generators for lighting purposes. On p. 32 it is stated that the drawback of the system of arranging lamps in series in one circuit lies in the fact that the individual lamps are not independent of one another; yet on p. 94 and p. 184 it appears that there are means by which any lamp is made quite independent of all the other lamps in the series.

We object entirely to Dr. Maier's classification of arc lamps into "monophotal" and "polyphotal"; these high-sounding names being respectively applied by him to lamps that will not work, and those that will work, when more than one is placed in series in the same circuit. The distinction is entirely misleading: for the question whether one or many lamps can be worked together depends quite as much on the dynamo as on the lamps. Every one knows that modern dynamos are so designed as to work under one of two standard conditions: they must either yield a constant current in the line, or else must maintain a constant difference of potential between the distributing mains. As a rule arc lamps arranged to work in series in a constant-current circuit will not work if set in parallel across the mains of a constant-potential network, and *vice versâ*. The true classification of lamps should therefore be into constant-current lamps which will work many in series, and constant-potential lamps which will work many in parallel. Probably the only lamp that will not fall in one of these two categories is the old regulator of Duboscq and Foucault. Most of the lamps classified by Dr. Maier as monophotal, and which according to him can only be worked each with its own separate dynamo, will work perfectly well in parallel with one another on a constant-potential system of mains. One consequence of Dr. Maier's curious classification is that when he comes to the Gülcher lamp, which is an excellent lamp for lighting in parallel, he cannot put it under either head, and it is relegated to miscellaneous lamps. Of the lamps which he describes as polyphotal, the very first is Lontin's modification of the Serrin lamp; but curiously enough the lamp figured and described in the text is *not* Lontin's but is the old unmodified Serrin, which, though stated by Dr. Maier to be monophotal, is really exactly in the same category as Gülcher's lamp. The lamp of Street and Maquaire is stated to be different from all other arc lamps in employing a vibratory principle: the author appears not to know that the lamps of Clark Bowman, Newton, and Pieper also have vibrating mechanism. Amongst other erroneous points it is stated on p. 321 that Edison was the first to point out the advantage of high electromotive force in the glow lamp; on p. 324 that the Lane-Fox pump gives "infinitely better results" than the Sprengel or Geissler pumps; on p. 305

that carbon is deposited "electrolytically" in the flashing process of heating in a hydrocarbon vapour; on p. 31 that it requires "several" dynamos instead of one to yield an electromotive force of 2500 volts; on p. 27 that Ohm's law is true for the whole circuit only, and not for its parts; on p. 91 that a shunt coil was first used in an arc lamp by Von Hefner Alteneck; on p. 14 that "the velocity of a body falling through a vacuum" is "9·81 metres in one second." The definition of the ohm as originally fixed in 1862 by the Committee of the British Association was certainly not "equal to the resistance of a column of mercury of 104 centimetres in length and 1 square millimetre section," as stated on p. 13. There is a grossly misleading extract from Merling's work given in pp. 15 to 28 on the distribution of heat in an electric circuit. The old battery rule of arranging internal resistance equal to the external to get maximum current is trotted out, without a word of warning that this is an arrangement always to be avoided on account of its bad economy; and as if this were not bad enough, an algebraic corollary is added showing that this arrangement of maximum current is such as to make the maximum rate of output of heat in an external conductor of given resistance one-quarter of the output that there would be in the circuit if the battery were short-circuited. The student will at once draw the erroneous conclusion that at least three-quarters of the heat must necessarily always be wasted. But to make matters still worse, on p. 24, where the grouping of the battery is still under discussion, it is stated that in "no case can we obtain more heat," in the conductor of given resistance, than the quarter previously mentioned. This is entirely untrue; for if the cells be grouped all in parallel so as to reduce the internal resistance to a minimum, then a very high percentage of the heat of the current—practically all of it—will be obtained in the conductor of high resistance, and the zincs of the battery will consume more slowly. There are several other matters to which exception must be taken: a crude assertion that a Gramme dynamo is better than a Siemens, which may have been true in 1878; crude statements true perhaps of a particular dynamo or a particular lamp, but not true of dynamos or lamps in general; crude advice to makers to arrange their lamps so as to keep the resistance of the arc constant; crude arguments in favour of using shunt-wound electro-magnets in arc lamps, all reasons being given except the right one. Three times Dr. Werner Siemens's name is erroneously given as Wilhelm Siemens. Lastly, electric engineers will be surprised to find amongst the practical hints nothing about the "striking" of the arc, or about the "hunting" action of lamps and its avoidance. It is to be hoped that these matters will be remedied when the author comes to rewrite his book for a second edition.

S. P. T.

DISORDERS OF DIGESTION

On Disorders of Digestion. By T. Lauder Brunton, M.D., F.R.S. (London: Macmillan and Co., 1886.)

THIS book is a reprint of a series of disconnected papers which the author has contributed during the last thirteen years to various periodicals and Societies, and is prefaced by the Lettsomian Lectures, "On Disorders of Digestion," in which the author has collected together into one homogeneous whole many of the observations and illustrations which he had introduced into his earlier papers. These lectures give a most admirable *résumé* of the latest advances in our knowledge of the complicated processes of digestion and of mal-digestion, and the succeeding papers form a most interesting study of the gradual development of the author's views.

The main idea which runs throughout the whole work, and which the author more than any one else in this country has developed, is that, in mal-digestion, products are formed which in their passage through the liver disorder its functions, and on reaching the general circulation act more or less as poisons, producing languor, listlessness, heaviness of the limbs, great depression of spirits, and headache.

Brieger especially has worked out both chemically and physiologically the products of digestion and decomposition of food-stuffs. He has been able more or less successfully to separate several alkaloids which have most powerful effects when administered to animals. Several resemble very closely in their effects muscarin, the active principle which Prof. Schmiedenberg has separated from several species of mushrooms. This when administered to animals causes vomiting, purging, dyspnœa, and prostration, and it has been found that atropia is an efficient antidote. Many of these alkaloids are detained by the liver, and excreted with the bile into the intestine, again to be later on re-absorbed by the portal circulation, and may thus circulate in the portal system without ever entering the general circulation. Lead, copper, and other minerals, when administered by the mouth, often circulate in this manner, and the same process is offered as an explanation of the trivial effects of curara when swallowed, while its subcutaneous injection is lethal.

The author's views are corroborated by recent researches which allot a most important part in all digestive processes to bacteria and other micro-organisms. Cultivations carried on at Leipzig have shown that twenty-five micro-organisms are commonly found in the mouth, and that these under certain conditions may occasionally develop in various parts of the alimentary canal. Some develop large volumes of gas, and others lactic, caproic, caprylic, butyric acids, or other complex bodies during their growth. It is extremely probable, therefore, that the excessive multiplication of these micro-organisms generates the products of mal-digestion. A healthy condition of the alimentary canal and its secretions is inimical to their growth. A slight degree of acidity—less even than that normally present in gastric juice—is quite sufficient to check their growth.

These experiments open up quite a new field for the treatment of dyspepsia: the actions of old well-established remedies receive a new explanation, and new drugs will be pressed into the service.

Bitters are said to be beneficial because they check the secretion of mucus, which is a suitable nidus for some bacteria; mineral acids, mercury and salicylic acid are more strongly recommended than ever because of their antiseptic properties; charcoal, bismuth, and alkalies base their claim for support on their stimulation of the gastric secretion.

The title of the book is used in a very wide sense, and at the end is a series of articles on renal secretion, its disorders and treatment. Nowhere does the author better show his powers of dealing with complicated problems—marshalling together his facts, and then in the most lucid and pleasant manner, often assisted by apt illustration, setting forth his view of the question. Great stress is laid upon the distinct blood-supplies of the glomeruli, and of the convoluted and straight tubes, and upon the distinct regulating mechanisms, while an attempt is made to classify diuretics accordingly.

ALCHEMY

Die Alchemie in älterer und neuerer Zeit. By Hermann Kopp. Pp. 685. (Heidelberg: Carl Winter, 1886.)

THIS work is in two parts; the first part treats of alchemy up to the year 1775, and the second part subsequent to this date.

It is not written purely for the chemist, and indeed the student who looks here for the minor details of alchemy will be disappointed. These have already received ample treatment at the hands of Prof. Kopp in his "Beiträge zur Geschichte der Chemie," and the object of the present work is rather to lay stress upon the philosophy of the alchemists, to give some account of the organisations from which they received support, and to point out that such organisations had wider aims and a more distinguished following than is perhaps generally known. It will be read with as much pleasure by the student of literature and history as by the chemist. The difficulty of dividing a work of this nature into chapters is no doubt great, but we cannot help thinking that such a division would have been conducive to clearness; there is, however, a table of contents, a full and well-arranged index, and change of subject is indicated in the text by a break in the paragraphing. We have presented to us alchemy as a search after scientific truth under the guidance of principles which if ill-founded were yet sufficiently real to attract philosophers as well as representatives of wealth and power; as a science which survived persecution and failure, and whose allurements outlived the discredit brought upon it by dupes and swindlers. The relation between alchemy and medicine is traced in an interesting and careful manner. Although it has been by no means the author's intention to act as biographer, yet we have, if we may so speak, medallions of many of the more distinguished alchemists, in which the features calculated to indicate the growth and progress of principles are brought out with great clearness. Of such a type is the sketch of Leonhard Thurneysser, whose chequered career is indeed a romance of real life. In the second volume we have an interesting account of the "Rosenkreuzerbund," a secret society founded by Christian Rosenkreuz at the beginning of the fifteenth century. In this and kindred societies the "brothers" were encouraged to travel, gaining experience and knowledge which at their periodical meetings were retailed for the common interest and instruction of the members. Considerable latitude was allowed to the individual, and the following lines, by one who was himself connected with such a society, show that there was not always a servile respect for tradition :—

DER WEISE UND DER ALCHYMIST

Gesund und fröhlich, ohne Geld
Lebt einst ein Weiser in der Welt.
Ein Fremder kam zu ihm und sprach : "Auf meinen Reisen
Hört ich von deiner Redlichkeit ;
Du bist ein Phönix unsrer Zeit.
Nichts fehlt dir als der Stein der Weisen.
Ich bin der Trismegist, vor dem sich die Natur
Stets ohne Schleier zeigt ; ich habe den Merkur,
Dadurch wir schlechtes Blei in feines Gold verkehren—
Und diese Kunst will ich dir lehren."
"O dreimal grösster Trismegist !—
Versetzt der Philosoph—du magst nur weiter reisen !
Der ist ein Weiser nicht, dem Gold so schätzbar ist.
Vergnügt sein ohne Gold, das ist der Stein der Weisen."

The constitution of these societies is explained, and we are initiated into the mysteries of the various grades of rank: the juniors, the theorists, the practitioners, the philosophers, the minors, the majors, the adepts, the magister, and finally that rarest honour, the magus.

A considerable space is occupied with the history of one who was ever active in the welfare of such organisations—Georg Forster—born near Danzig in 1754. He was a remarkable man in every respect, unstable to a degree, holding peculiar opinions on religious topics, unskilled in all which contributes to success in the general occupations of life, and yet wielding powerful influence in the circle within which he moved. His earlier travels led him to England, where he was at the age of thirteen engaged in teaching French and German in a school at Warrington; then he passed into the East India Company's service; whilst at the age of eighteen we find him as a companion of Cook in his second voyage round the world; later he returned to the Continent, and became Professor of Natural History at Halle. Some thirty pages are devoted to a charming sketch of Georg Forster's character, and we do not know any other passage which affords such enjoyable reading.

S. Th. Sömmering was Forster's bosom friend and companion, a man not unlike him, and who shared all his trials and difficulties. In the concluding pages of the book it is shown how with the dawn of dynamical and quantitative ideas in chemistry at the close of last century, alchemy underwent modification, and, ultimately, rapid decline. One is tempted to wish that the author had given a concluding chapter on the rise and development of the principles of chemistry as shown by an examination of the doctrines of the alchemists.

Notes and references have been freely used throughout the work, and where remarks of such a length are required as to interfere with the continuity of the text, these are arranged in an appendix. These addenda occupy over 200 pages, and constitute in fact a most valuable contribution to the history of alchemy. The style and general character of the work will appear from the remarks that have been already made, and if there are occasionally passages that are somewhat abstruse, yet on the whole we have a clearness and picturesque delineation excelling in our opinion, anything that has hitherto appeared on the subject, and we congratulate the veteran author on his success in a new phase of literary effort. Our English readers will be glad to know that the book is printed in Roman type, and can be had bound in boards.

G. H. BAILEY

OUR BOOK SHELF

La Terre des Merveilles. Par Jules Leclercq. (Paris: Librairie Hachette et Cie., 1886.)

IN this volume M. Jules Leclercq describes a visit made by him a few years ago to the Yellowstone National Park, during which he saw all the sights of this *terre des merveilles.* The writer is already well known in his own country as an accomplished writer of popular books of travel, and accordingly he makes the most of the Yellowstone region and its wonders. His sketches of these are preceded by a very interesting chapter on the early explorations of the territory, from the visits of the first adventurous trappers. There are two maps—one a detailed map of the "Park," the other a general map of part of the United States to show the position of the Yellowstone region. There is also a considerable number of illustrations. The volume is published in Hachette's "Collection des Voyages illustrés," and is a clever, well-written popular account of a district full of natural wonders.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Cereals of Prehistoric Times

MY friend Mr. Carruthers in the interesting address delivered recently to Section D of the British Association, makes a remark which I confess surprises me. He says (NATURE, September 9, p. 453):—"It is remarkable that in our own country, with all the appliances of scientific cultivation and scientific farming, we have not been able to appreciably surpass the grains which were harvested by our rude ancestors of 2000 years ago." He mentions in support of this conclusion that "the wheat from lake-dwellings in Switzerland for which I am indebted to Mr. J. T. Lee, F.G.S., are fair samples."

This is certainly a striking fact. The persistence of specific and even of varietal types in a country like Egypt is what we might expect, because the very preservation of the material evidence is a proof that the physical conditions have persisted likewise. But that cultivated plants have remained unaltered since the Stone Age seems a conclusion difficult to accept in the face of every-day experience as to what can be done in modifying them. The data collected by Prof. A. De Candolle ("L'Origine des Plantes cultivées," pp. 284, 285) leads me, I think, to the conclusion that Mr. Lee's specimen must be exceptional. Prof. De Candolle mentions three varieties of wheat as cultivated in the Stone Age; of these he says:—"Aucune n'est identique avec les blés cultivés de nos jours. On leur a substitué des formes plus avantageuses." Two of these have been obtained from lacustrine dwellings. He remarks that the most ancient lacustrine people of Western Switzerland cultivated a wheat with small grains, which Heer has carefully described and figured under the name of *Triticum vulgare antiquorum.* These people he regards as contemporaries of the Trojan war, if not older. The culture of this kind of wheat persisted in Switzerland till the Roman conquest. Unger found the same form in a brick of the pyramid of Dahschûr in Egypt of the date B.C. 3359. The other variety (*Triticum vulgare compositum muticum,* Heer) was less common in Switzerland in the first age of stone, but is the one most frequently found in the less ancient lake-dwellings of Western Switzerland and Italy.

W. T. THISELTON DYER

Physiological Selection and the Origin of Species

HAVING written for the *Fortnightly Review* a full reply to Mr. Wallace's article in that journal, I will not here anticipate what I have there to say. But, seeing that he has repeated in these pages the substance of his criticism, I will here also repeat the substance of my reply. On the present occasion, therefore, it is enough to remark that I have never made the "extraordinary statement that, during his whole life, Mr. Darwin was mistaken in supposing his theory to be a theory of the origin of species." On the contrary, as I shall hereafter show, so far as this matter is concerned, both my opinions and my statement of them are in full agreement with those presented in Mr. Darwin's works.

Without wishing to discuss with Mr. Francis Darwin the meaning of the sentence which he quoted from the "Origin of Species," I feel it is only due to my own understanding to give the following explanation. If any one will turn to the sentence in question (p. 247, 6th ed.), he will find that it constitutes an integral part of an argument showing that sterility between species cannot have been brought about by natural selection. The argument is that, *even supposing sterility with parent forms to be an advantage,* it is an advantage which could not be seized upon by natural selection, and hence that some other explanation of such sterility must be found. Now, so far as I can see, there is here not only no shadow of the theory of physiological selection, but the whole argument is proceeding on totally different lines. For the very essence of this theory is that the sterility in question *need not be supposed to be an advantage,* and therefore that any variation in the way of such sterility *does not require* to be selected through the struggle for existence, being of its own nature a variation which survives. In no part of Mr. Darwin's writings can I find even the most distant allusion to the possibility of this particular variation being thus a variation *sui generis*—itself a cause of specific differentiation, and, as such, independent of natural selection. Least of all can I find evidence of any such allusion in the pasage referred to, seeing that the argument here consists in expressly regarding the variation of sterility as resembling variations in general, and therefore in *not* regarding it as possibly presenting the highly peculiar quality of being survivable *per se.* And, considering how fully Mr. Darwin has given his reasons for rejecting many ideas much less feasible, I confess it appears to me a most extraordinary and unaccountable thing that he should nowhere have so much as mentioned this alternative, had it ever been familiar to his mind. I may add that, if any reasonable ground can be shown for supposing this to have been the case, it would cause me to abandon the whole research.

Mr. J. H. A. Jenner's remark cannot apply to the particular kind of variation with which alone my theory is concerned, because, if so, it would amount to saying that the more sterile the variety is with its parent form the more will this sterility be increased by intercrossing with that form, which is absurd. But with regard to many other kinds of *beneficial* variation the remark of course is true.

I am greatly obliged to Mr. Evershed for directing my attention to Mr. Catchpool's letter in NATURE (vol. xxxi. p. 4). Having obtained a copy of the issue referred to, I find, as he says, that "the theory of physiological selection is very clearly put forward." Moreover, the difficulties against the theory of natural selection on account of inutility and sterility are very clearly stated. I may take this opportunity of requesting any of your readers who may know of any previous publications of the theory—no matter how vague or sketchy—to be kind enough to furnish references.

GEORGE J. ROMANES

Geanies, Ross-shire, September 18

Cooke's "Chemical Physics"

IN your issue of September 2 (p. 405) I find under the cover of a review of Cooke's "Chemical Physics" that Prof. Armstrong has been good enough to quote a passage from my "Lessons in Elementary Chemistry," though without naming the source, concerning Avogadro's law, about which he asks the question, "Could anything be more misleading and inaccurate?" My friend appears to be no exception to the well-known rule as to critics failing to read the books they review, for a note on the same page (55) disposes of the "inaccuracy," whilst the "misleading" statement is explained further on (p. 154). On the other hand, Dr. Armstrong has not followed the usual practice of critics, who, not being authors, escape from the danger of a retort courteous from those whom they find fault with; and hence I feel sure he will forgive me in saying that, whilst fully agreeing with him in the statement that a knowledge of mathematics is advisable for a chemist if he is to understand physics and physical

methods, I still am bold enough to ask whether anything can be "more misleading and inaccurate" than the formula for reduction for temperature and pressure given in both editions of his "Organic Chemistry" under the description of Dumas's vapour-density method. And to add that no excuse can here be found of a correction given elsewhere, or of the fact that it may be desirable sometimes to state a case broadly to begin with and to define it more closely afterwards. HENRY E. ROSCOE

The Tangent-Galvanometer

ATTENTION has recently been drawn more than once (notably by Sir William Thomson, and by Mr. W. H. Preece and Mr. Kemp) to the advantages offered, in certain cases of the use of the tangent-galvanometer, by placing the instrument so that the plane of the coils makes a greater or less angle with the plane of the magnetic meridian. It may not be amiss, therefore, to point out that, in 1869, M. Bertin showed that the sensitiveness of the tangent-galvanometer for strong currents may be increased and the usable range of deflection doubled by placing the circle in a vertical plane inclined at an angle of 45° to the magnetic meridian (*Annales de Chimie et de Physique*, 4th series, vol. xiv. p. 27).

When readings are taken with the current traversing the galvanometer, first in one direction and then in the other, as would always be done if accuracy were important, the expression for the strength of current is almost as simple when the coils make an angle with the magnetic meridian as when they are parallel to it. In the former case the strength of current is

$$C = \tfrac{1}{2}k \cos \alpha \,(\tan \theta + \tan \theta'),$$

where α is the (fixed) angle between the plane of the coil and the magnetic meridian, and θ and θ' the deflections of the needle from the plane of the coil due to the current in the two directions. θ is reckoned positive in the direction from the plane of the coil towards the plane of the meridian, and θ' is reckoned in the opposite direction. To determine the angle α we have

$$\tan \alpha = \tfrac{1}{2} (\tan \theta - \tan \theta'),$$

and it is probably most convenient to determine the numerical value of $\cos \alpha$ in the first expression from this by tables. If the current be adjusted so as to make the second deflection $\theta' = 0$, we have simply $\tan \alpha = \frac{1}{2} \tan \theta_1$, if θ_1 be the corresponding deflection with the current reversed. G. CAREY FOSTER

University College, London, September 30

Alligators in the Bahamas

IN Catesby's "Natural History of Florida, Carolina, and the Bahama Islands," published in the latter half of the eighteenth century, the author, usually a most accurate observer, states that the mangrove-swamps on the Island of Andros presented a loathsome appearance owing to the remains of fish having been left there, half eaten, by the *alligators*. During a fairly complete exploration of Andros, which I made in the early part of last year, I observed no traces of these animals, though, as I was not then aware of Catesby's statement, I did not make any special search for them. In response, however, to inquiries made in the local paper, I lately received from the Rev. W. L. de Glanville, Rector of Inagua, Bahamas, a letter from which the following is an extract :—

"Legendary stories of alligators having been floated to this island on logs of mahogany are numerous here. I have not succeeded in verifying any of them. On July 21, while on a visit to our North-West Point Settlement, a man exhibited to me the skin of an alligator which he had shot a day or two previously. Length all over, from tip to tip, 8 feet ; from line at right angles to tip of snout to angle of jaw, 13 inches ; greatest frontal diameter, about 11 inches.

"Logs of mahogany have been drifted recently on these shores, but no one saw an alligator arrive. That shot was on shore, and seen more than once."

As the distance from this to Inagua is more than 400 miles, I have not been able to make inquiries on the spot. But there seems to be no doubt that the alligator must have been carried by the current from the south-east on a log of mahogany or other wood from San Domingo to Inagua. It seems likely that alligators have frequently been drifted to that island, though the absence of a suitable environment has prevented their surviving. The conditions in Andros would suit them better, since about that island there is a considerable extent of fresh and brackish water in lakes, lagoons, and creeks. But it is not easy to understand how they could arrive at Andros, unless we suppose that the Gulf Stream carried them from the north-west coast of Cuba and cast them on the west side of the Great Bahama Bank, whence small local currents and the wind might bring them to the west side of Andros. The distance travelled would in this case be about 300 miles, or about twice as great as that from San Domingo to Inagua.

I trust that this note may be useful as affording further proof of the fact that oceanic currents take some part in the dispersion of even large animals. JOHN GARDINER

Nassau, Bahamas, September 15

Meteors—The September Taurids

ON September 22, 1886, 10h. 26m., I observed a fine meteor about equal to Jupiter, pursuing a path of some 7° in the extreme east region of Aries. It left a streak and moved somewhat slowly, being evidently foreshortened near its radiant point in Taurus. At 10h. 46m. the same night I noticed another meteor from the same direction.

On September 21, 1879, I counted 92 meteors, including several from this radiant in Taurus, and on September 22, 1884, two others were seen amongst 29 registered on that date.

A comparison of the paths shows a well-defined radiant at 63° + 23° (about 8° N.N.W. of Aldebaran), and I believe the shower is rather an important one, though not well visible until late in the night.

I have recognised several radiants from this position in Taurus in October and November, and in August Mr. Greg derived a shower at 64° + 22° from the observations in 1867-74, collected by the Luminous Meteor Committee of the British Association. On October 17-19, 1877, I found a radiant at 63° + 22° ; on November 20, 1876, at 62° + 22½° ; and on November 27, 1880, at 63° + 21°. There are also many other contemporary showers slightly south-west at about 59° + 20°, and it is in November that the display of meteors from Taurus reaches a maximum.

In September, during the last half of the month, I have determined some other showers in the region of Taurus, supplying meteors of much the same character. The chief additional centres of radiation seem to be at 74° + 14°, 70° + 4°, and 53° + 3°, and there is a fairly active shower also from a point further east, at 89° + 19°, in Orion. The first of these, near 11 Orionis, was splendidly defined from 8 bright meteors on September 27, 1886 ; and on September 22, 1871, Lieut.-Col. Tupman saw it at 75° + 15°. The Orionids at 89° + 19° were well seen in September 1877, and confirmed in September 1884, but this is essentially a morning shower, as the radiant does not reach a fair altitude for the dispersion of its meteors until the few hours preceding sunrise. W. F. DENNING

Bristol, October 3

Action of Light upon Diastases

IN 1878, in conjunction with Mr. T. P. Blunt (*Proc. Roy. Soc.* No. 191), I showed that the *inversive ferment* of cane-sugar is destroyed by oxidation on prolonged exposure to sunlight. Lately I have extended this observation to other ferments of the like kind, with similar result.

Twenty-five cubic centimetres of very active solutions of *malt diastase*, *pancreatic diastase*, and *trypsin* respectively were rendered inert by insolation in 50 cc. flasks for one month (August 25 to September 25).

A solution of *pepsin* was likewise destroyed, but in this case the ferment had been badly prepared, and was not very potent to commence with.

On the other hand, 25 cubic centimetres of solution of *rennet*, though distinctly enfeebled by insolation, still retained its specific properties at the end of the month. This immunity, however, was only relative, for a more dilute solution in a shallower stratum was almost entirely destroyed by one week's exposure to light (August 31 to September 7).

In all cases the contents of similar flasks kept under like conditions of temperature, &c., but in the dark, were found to be still active at the termination of the experiment.

In determining the peptonising power of the trypsin and pepsin, Grützner's method (carmine-stained fibrin) was very useful. I find, however, that it is more convenient and econo-

mical to keep the prepared fibrin by drying it in a current of air than to preserve it under ether, as usually recommended.

September 27 ARTHUR DOWNES

Note on Actinometry by Oxalic Acid

OXALIC acid is entirely oxidised by light (*Proc. Roy. Soc.* No. 191, and *Chemical News*, October 8, 1880), and affords, by reason of its own physical properties and those of the products of the reaction, an excellent medium for actinometry. A preliminary series of experiments on the physical conditions which modify this oxidation show that, *c.p.*, the effect is greater in direct proportion to the extent of exposed surface of the solution but inversely as its depth. It is greater also in proportion to the strength of the solution; and it would appear—I speak for the present quite provisionally—that in this relation the reaction follows a definite law, being *as the square root of the mass.*

September 27 ARTHUR DOWNES

Humming in the Air caused by Insects

IN a letter to the Hon. Daines Barrington (letter lxxx.) the Rev. Gilbert White, the well-known author of the "Natural History of Selborne," mentions a strange humming sound in the air. He writes:—"There is a natural occurrence to be met with upon the highest parts of our downs in hot summer days which always amuses me much without giving me any satisfaction with respect to the cause of it: and that is a loud audible humming as of bees in the air, though not one insect is to be seen. This sound is to be heard distinctly the whole common through from the Money Dells to my avenue gate. Any person would suppose that a large swarm of bees was in motion, and playing about over his head. This noise was heard last week on June 28."

It is singular that no explanation has been offered by any one for such a common phenomenon. I am convinced that the humming sound mentioned by Gilbert White was nothing more than the noise occasioned by the vibrations of millions of insects' wings in the air. In hot summer evenings in particular I have heard these peculiar humming sounds, and know them to be caused by immense hordes of gnats and midges which fill the air with their numbers. W. HARCOURT BATH

The Limes, Sutton Coldfield, near Birmingham, October

Mimicry in Snakes

A CURIOUS fact has been lately brought to my notice by a friend of mine, Mr. H. M. Oakley, in connection with the *Dasypeltis scaber*, Linn., or egg-eating snake—the "Eijer eter" of the Dutch colonists—which, if not already well known, may prove of interest to some of your readers. The specimen obtained by Mr. Oakley was caught at Hout Bay some twenty miles from Cape Town, and is about 3 feet in length, and its size, markings, and colour bear sufficient resemblance to those of the Berg Adder (*Clothos atropos*, Linn.) to be easily mistaken for that snake. It also has keeled scales, generally characteristic, at the Cape, of venomous species. Its head has, however, the long lacertine shape distinctive, here, of harmless snakes, but, when aroused and alarmed or irritated, it flattens it out until it assumes the usual viperine shape of the "club" in a playing card. It then coils as for a spring, erects its head with every appearance of anger, produces a hissing noise with its scales, not unlike the hiss of a puff adder or cobra, and darts forward as if to strike its fangs into its foe, and in every way exactly simulates the motions of an irritated berg adder. This snake has, however, neither fangs nor teeth (which, indeed, would not be required for egg-swallowing), and is not poisonous, a fact which was placed beyond doubt by Mr. Oakley repeatedly placing his finger in the reptile's mouth. This seems a clear instance of mimicry of another species for defensive purposes, but I am not aware of another instance among ophidians.

W. HAMMOND TOOKE

Cape Town, Cape of Good Hope, September 8

THE COLONIAL AND INDIAN EXHIBITION

CONTINUING our review of the most noteworthy or interesting vegetable products now being exhibited at South Kensington, we find in close contiguity to those from British Guiana, described in NATURE, July 15, p. 242, the exhibits from

Mauritius.—The entire collection, though not large, is one of some interest, and the vegetable kingdom plays by far the most important part in the exhibits. The collection of fibres will attract attention as much for its completeness as for the care with which they have been prepared. Many are of scientific interest only, being obtained from plants that could never be turned to commercial account, such, for instance, as *Hyophorbe Verschaffeltii*, *Latania commersonii*, *Lodoicea sechellarum*, *Macrozamia spiralis*, &c. This collection is exhibited by the Botanical Gardens, as is also a collection of woods of similar interest. Some notes on these woods are useful. Thus, we are told that *Tecoma pentaphylla* has a soft white wood, not much used, and that it is a moderate-sized shade-tree of rapid growth. Samples of the wood, however, grown in the West Indies, show a close and even grain, and are fairly hard—so hard, indeed, as to suggest its suitability for wood-engraving, for which purpose it has been tried in this country, and though not by any means equal to boxwood, was reported upon as likely to be found useful for some kinds of work. The soft wood of *Ficus mauritiana* is used in the colony both for firewood and for hollowing out for canoes, while the hard wood of the Ebony (*Diospyros Ebenum*), which is described as being either black or sometimes streaked with yellow and brown, is used for inlaying, furniture, and ornamental turnery. The Bois Maigre (*Nuxia verticillata*) is said to produce a short-grained timber which decomposes rapidly, but when young it makes excellent walking-sticks, which are much sought for. *Terminalia Benzoin*, a large tree, which has become scarce in Mauritius, produces a wood valued for many purposes. It would seem that the wood is sometimes fragrant, for it is said that "some parts of the tree were once much burnt in Mauritius as an incense." Another odoriferous wood is *Noronhia Broomieana*, called here Bois Sandal. The Carambole (*Averrhoa Carambola*), valued in India for the sake of its acid fruits, is planted in Mauritius for the same purpose, the fruits being eaten either raw or made into tarts.

Probably the exhibits that attract most attention in the Mauritius Court are the fine samples of vanilla pods, covered as they are with an abundant coating of crystals, and shedding forth, even through the glass cases which cover them, the delicate fragrance for which vanilla is celebrated.

Seychelles.—Amongst these exhibits the Double Cocoa-Nut, or Cocoa de Mer (*Lodoicea sechellarum*) is the most prominent. The double form of the fruit, which is its normal condition, is well shown, as well as a triple-lobed nut, which is not very uncommon. Here also are fine samples of vanilla, nearly, if not quite, equal to those from Mauritius. Dried papaw juice and some remarkably good specimens of essential oils are shown, all of which are extremely creditable to the colony.

Cyprus.—The vegetable products shown in this Court are not numerous, nor is there anything of novelty excepting perhaps a peculiar black substance described as honey, from the Carob or Locust Bean (*Ceratonia Siliqua*). The pods themselves are also exhibited, and their production, it seems, has greatly increased in recent years, stimulated by an increasing demand, especially in this country, where they are used very largely in making the patent compound cattle-foods, in consequence of their saccharine and nutritious character. Low freights have much encouraged the trade in carobs, enabling shippers to sell them at moderate prices. The quantity of carobs exported from Cyprus in 1884 amounted to 30,000 tons, about one-half of which came to England. The finest quality is produced in Limassol and Lefcara, and obtain relatively higher prices than those of Kyrenia. The average price realised for these pods is about 3*l.* per ton.

Another product of great importance to Cyprus up to

the year 1873 was madder from *Rubia tinctorum*, an article of considerable profit to landowners. It was largely used in dyeing Turkey-red yarns. The discovery of coal-tar dyes seriously interfered with the demand for madder, so that its growth has much decreased. In consequence, however, of the mineral dyes being much inferior in fastness, madder is being again sought after, and should the demand continue, there is a prospect that madder will again assume its former importance. Among other interesting exhibits, the rude native cart of the form in use for over 2000 years, and still in use, attracts much attention, as does the threshing-board, the same as was in use in patriarchal times. It is studded with flints on the under-side, and is drawn by bullocks or horses over the grain, by which means the seed is separated from the ears and the straw reduced to small particles. This is said to be the only system employed for threshing in Cyprus.

A few well-known woods, such as Olive (*Olea europea*), Cedar of Lebanon (*Cedrus Libani*), Bay Laurel (*Laurus nobilis*), Chian Turpentine, (*Pistacia Terebinthus*) are exhibited, as well as the concrete resin of the latter, or crude Chian turpentine, under the name of Trimithia gum, in curious small greenish-coloured pots.

Malta.—The Maltese exhibits will be best remembered by the fine show of lace and silver filigree work. A good show is also made of preserved fruits, and tobacco of very fine quality and varied forms is exhibited; besides these there are very few other vegetable products.

JOHN R. JACKSON

GREEK GEOMETRY

WE have before us parts 6, 7 (?), of Dr. Allman's "Greek Geometry from Thales to Euclid,"[1] in which we are brought almost into touch with Euclid. There is then but little wanting to complete the task commenced by the author in 1877, in the performing of which so much light has been thrown upon the contributions of the early Greek mathematicians to geometrical science.

In NATURE (vol. xxx. pp. 315, 316) we gave an account of Archytas and Eudoxus; the present parts commence with a discussion of the claims of Menæchmus, "pupil of Eudoxus, associate of Plato, and the discoverer of the conic sections." In the forefront are placed translations of eleven fragments which contain what is known of Menæchmus. The various points which arise are most carefully reasoned out, with considerable detail, but we cannot attempt here to compress what is already concisely given. The notes are very valuable, and show over what a wide field of reading Dr. Allman's researches have taken him. We note only the prominence given to M. Tannery's papers, as we have frequently had occasion in these pages to draw attention to this mathematician's valuable memoirs on Greek geometry. The last part (which we have numbered 7) opens with an account of Dinostratus, brother of Menæchmus, whose name occurs in connection with the quadratrix. Dr. Allman states the case of Dinostratus *versus* Hippias: "The result of the whole discussion seems to be that the quadratrix was invented, probably by Hippias of Elis, with the object of trisecting an angle, and was originally employed for that purpose; that subsequently Dinostratus used the curve for the quadrature of the circle, and that its name was thence derived." Sporus (or Porus) comes in for a mention, and then we come to Aristæus, who wrote on the conic sections, and is the author of the theorem, "The same circle circumscribes the pentagon of the dodecahedron and the triangle of the icosahedron, these solids being inscribed in the same sphere." This occurs in his "Comparison of the Regular Solids." Bretschneider thinks the thirteenth Book of Euclid's Elements is "a recapitulation, at least partial, of this work of Aristæus" (cf. also Dr. C. Taylor's "Conics," p. xxxiii.).

Of Aristæus, in closing, our author writes: he "may, therefore, be regarded as having continued and summed up the work, which, arising from the speculations of Philolaus, was carried on by his successors—Archytas, Eudoxus, and Menæchmus. These men were related to one another in succession as master and pupil, and it seemed to me important that the continuity of their work should not be broken in its presentation."

We hope another year will suffice to bring this sketch of Greek geometry to a close, and that then the author will collect these parts, whose appearances have been extended over nearly ten long years, in one volume, with such additional notes as his subsequent reading will enable him to append.

We can only commend these two parts, as we have the previous ones, to the careful study of all who are interested in these researches: they have taken a high place in the estimation of foreign mathematicians, even in cases where the author's conclusions have not been unhesitatingly accepted.

[1] The following references to the several parts may be of service:—Hermathena, part 1, vol. iii. No. 5, pp. 160-75; part 2, same number, pp. 175-207; part 3, vol. iv. No. 7, pp. 180-228; part 4, vol. v. No. 10, pp. 186-212; part 5, same number, pp. 212-235; part 6, vol. v. No. 11, pp. 403-32; part 7 (?), vol. vi. No. 12, pp. 105-30.

THE HYGIENE OF THE VOCAL ORGANS[1]

THE cultivation of the voice and the means of maintaining it in a state of excellence under the varying strain of daily life, are subjects of interest to us all, but become of paramount importance to those who are professionally brought before the public as speakers or singers. Although the laryngoscope is invaluable in the recognition and treatment of disease, it is surprising how little it has up to the present time added to our knowledge of the physiology of the larynx.

The difficulties of examining the larynx during singing are so great that a large number of singers have to be examined to obtain a complete view of the whole process by even the most expert laryngoscopist. The results of the examination of some three or four hundred persons with fine voices, including most of the best singers of the day, form not the least interesting portion of the book.

There is no question that the voice, whether the note be high or low, whether a chest or head note, whether bass or falsetto, is produced by vibration of the free edges of the vocal cords, which are two movable ligamentous bands about half an inch long stretched from back to front of the larynx. In other words, the only place where all notes, whatever their character may be, can be produced, is in the larynx.

These bands are attached anteriorly in contact with one another, but their posterior fourth is attached to the small pyramidal-shaped arytenoid cartilages, which can move laterally. The glottis, the space between them, is thus divided into a ligamentous and a cartilaginous portion. There is the greatest difference of opinion among authorities as to the position of the cords and arytenoid cartilages, and as to how much of the cord vibrates in the production of the various sounds.

Dr. Mackenzie divides the range of the voice into two registers, viz. one (chest) in which the pitch is raised, by means of increasing tension and a (consequent trivial) lengthening of the cords, as the voice sings upwards; the other (head), by which a similar result is brought about by gradual shortening of the vibrating reed, which is still tense, though less so than in the chest register. These fundamental divisions are the so called chest and head modes of production, and the falsetto corresponds to the head register of the female voice, of which it is an imitation.

[1] "The Hygiene of the Vocal Organs." By Morell Mackenzie, M.D. Pp. 223. (London: Macmillan & Co., 1886.)

Speaking generally, it may be said that the cartilaginous glottis is generally open in the lower and gently closed in the upper notes of the chest, and that a segment of the ligamentous glottis is *tightly* closed in the head voice. The two registers may be called the long-reed and the short-reed, according to the length of the cord vibrating. It has also been noticed that the blast of air is much feebler with the head than with the chest voice.

The new and important observation which Dr. Mackenzie has made and amply verified is, that in the head note of women and in falsetto singing only the anterior third of the vocal cords, as shown in Figs. 3 and 4, vibrate, and that the remainder of the cords are in firm contact with one another. Only twice has he observed a vibration limited to the middle third of the cords, which has often been described as the usual one. Some observers have asserted that in falsetto only the extreme edge of the cords vibrates; but, as 12 inches is the nearest distance at which a good image can be obtained by the

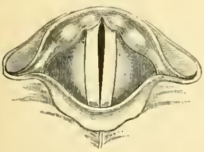

Fig. 1.—The position of the vocal cords for the lower range of chest notes.

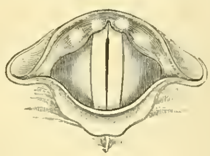

Fig. 2.—The position of the vocal cords for the higher range of chest notes

Fig. 3—The position of the vocal cords for head notes.

Fig. 4.—The position of the vocal cords for falsetto notes.

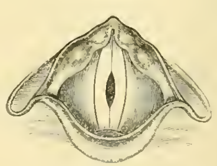

Fig. 5.—A very exceptional position of the vocal cords for head notes.

laryngoscope, only the mean position of the cords can be observed, and it is impossible to detect any vibration when a high note is being sung; and hence the author rejects the statements of those who profess to have observed vibrations limited to the edge.

The long-reed or chest voice is generally used by sopranos, Figs. 1 and 2 showing the position of the cords in the case of Mesdames Nilsson, Albani, and Valleria; on the other hand, the high notes of mezzo-sopranos and contraltos are short-reed, *e.g.* Madame Patey, as shown in Fig. 3. Tenors use both reeds, while the long only is used by the basses, and commonly by the barytones.

Alto singers among men use the short-reed, whilst boys always use the long.

In falsetto the false vocal cords, which are movable bands of tissue superior to the true vocal cords, also approximate considerably.

The quality of a voice depends upon the accuracy with which the vocal cords are moved, on their tension, on the regulation of the respiration, on the position of the soft palate and fauces, and on the movements of the tongue and lips.

These points are all touched upon, but the details of the methods by which they are to be trained do not fall within the scope of this book, but belong to the domain of the singing-master.

A certain amount of vocal discipline is recommended for children as early as the age of five or six, or even younger, but it should be limited to simple airs of limited compass, such as those by Louisa Gray, published by Messrs. Wood and Co. They are warranted to contain "no love and no high notes," and may therefore be trusted not to inflame either the infant's tender heart or its delicate larynx.

Observations on 500 choristers have disproved the idea that "cracking" or "breaking" of the voice is an essential stage, for in only 17 per cent. did the voice become cracked in changing to the adult condition; and in these cases the cords were congested, and the state was due to over-exertion or to cold.

The hygiene recommended for vocalists may be summed up in the adoption of such a mode of life as is most beneficial to the general health; prompt treatment of any cold or hoarseness; and, if their faith is pinned on the virtues of raw eggs, champagne, or any of the thousand and one things recommended for the voice, they may be permitted, if they are not absolutely injurious.

The latter part of the book is occupied with the training of the speaking voice, an account of its various defects, and the methods of diminishing them.

Stammering, which depends on a defective control over the respiration and over the tension of the vocal cords, may be much improved by training; while stuttering, which depends on a spasm of the tongue and on imperfect control over the lips, is rarely benefited, except in the slighter cases. Strychnine is occasionally of use, while tobacco intensifies the difficulty.

Valuable hints for training the voice are given, and great stress is laid on the importance of a most accurate acquirement of the vowel sounds, when the consonantal follow readily.

A NEW CASE OF PARTHENOGENESIS IN THE VEGETABLE KINGDOM

EIGHT years ago I discovered in the *Quebrada* (*i.e.* ravine) of Guarenas, about nine miles to the east of Caracas, and approximately at 650 metres above the level of the sea, one of those charming groups of tropical vegetation, which are equally interesting to the botanist and to the lover of the picturesque beauties of nature. A magnificent specimen of *Pogonopus Ottonis* was all aglow with its large rosy sepals; up to its highest branches a luxuriant *Vitis caribæa* had ascended in graceful festoons, laden with blackish grapes, and displaying now and then in the gentle breeze the silvery glimmer from the under side of its palmate leaves; whilst in the damp shade underneath throve a colony of *Gloxinia pallidiflora*, a plant which from its bruised leaves gives out a smell identical with that of the spearmint.

My attention, however, was especially attracted to a tall suffrutescent climber with dark-green ivy-like foliage, and large drooping clusters of bright-red fruits, which I was sure I had never met with before. The general *facies* was certainly that of a menispermaceous plant; but the structure of the fruit proved to be utterly discrepant from anything I knew of this family. Flowers I found none, and a prolonged search in the neighbourhood for another specimen was to no effect. I gathered a quantity

of fruits, some of which were sown in the little garden belonging to the house I inhabited at the time in Caracas. Others I sent to several leading botanists in Europe, requesting them to give me their opinion about the plant; the result, however, was negative, the fruit being to all of them a puzzle, just as it had been to myself.

Meanwhile some of the seeds had germinated; I planted out three seedlings, which grew very vigorously, and in time produced an abundance of flowers, *all female ones*. Their structure gave additional weight to the supposition that the plant belonged to the Menispermaceæ, and believing it to be a new genus, I was anxious to discover the male plant. In this pursuit the botanical interest went hand in hand with that of the horticulturist; for although the plant is highly ornamental on account of its foliage, its principal merit as a decoration in a tropical garden consists in the striking contrast between the dark-green leaves and the large number of scarlet fruits, which bear the greatest resemblance to the half-ripe berries of the coffee-tree.

For a long time all my efforts were unsuccessful. I had moreover occasion to convince myself that the plant was extremely rare in our flora, as only in two places of the above-mentioned ravine a few specimens hitherto have been found by myself and my collectors.[1]

In 1881 at last one of my men brought me a plant which he pronounced to be a male one. The plant was set between two female ones, and after a couple of months I had the great satisfaction of beholding for the first time the *male* flowers that for several years had been the object of many a fatiguing search in the dense thickets of the little river of Guarenas. Both female plants produced now a large crop of fruits, though I was unable to make out by what agency the transport of pollen was effected. Of animals I noticed on the plants only a small species of a green *Tettigonia* (rather plentiful), some common mosquitoes, and a few caterpillars of *Ophideres cacica*.[2] It is not impossible that the mosquitoes were of some significance for the purpose of fecundation. I thought once the flowers might be of the anemophilous type; but this is certainly a mistake, as they are so much hidden amongst the foliage that the wind can hardly reach them.

I sent male and female flowers, as well as fruits, to Kew, and following the advice of Sir J. D. Hooker, likewise to Prof. Eichler, of Berlin, who recognised the plant as belonging to his genus *Disciphania*, and described it afterwards under the name of *Disciphania Ernstii* in *Jahrbuch des Kön. Botan. Gart. zu Berlin*, vol. ii., 1883, pp. 324-29, giving at the same time the analytical details of the flowers and fruit on Plate XII. of the volume referred to. As this work may not be easily accessible to all who take an interest in the facts I have to present hereafter, I deem it convenient to insert a summary description of the species, giving, however, a fuller development to certain structural particulars which, in my opinion, possibly may have some bearing upon the chief point of my communication.

Like many other Menispermaceæ, *Disciphania* bears root-tubers, which are, in good-sized plants, as large as a man's fist, and weigh 1 lb. and even more. They contain a considerable quantity of ovoid starch grains, the largest measuring 0·04 by 0·026 millim. The stem exhibits the compound structure usually found in climbing plants; it soon becomes woody in its lower part, whilst at the same time the periderm increases greatly in thickness, and forms a very irregular spongy bark. The younger parts of the stem, as well as the branches, generally wind around any support they may encounter; but sometimes they climb by means of the petioles, the basal part of which is rather abruptly thickened and variously curved, so that it serves as a hook, more or less, as in some species of *Clematis*. There are other branches which do not climb at all, but either hang down without showing any sign of torsion, or run in a straight line on the soil. The cortical system is very much developed, and the central parenchyma abounds in laticiferous cells, each one containing a great many nuclei and a viscid latex.

The leaves are most singularly polymorphous on the same plant, as will be seen from Figs. 12 to 16, which are copied, as likewise all the others, from Prof. Eichler's plate. The palmatisect leaf (Fig. 16) exhibits the basal thickening and inflection of the petiole, which I mentioned before.

The flowers are strictly diœcious, and in both sexes arranged in axillary, centripetal, and drooping spikes, measuring from 8 to 25 centimetres in length. The rachis of the female spikes is very thin at the basal end, where it is scarcely 1 millimetre thick; but it increases gradually in thickness, and in many cases it measures at the opposite end 3 millimetres by 2, so that the extreme transverse sections are in the ratio of 1 to 6 approximately. Its tissue is gorged with latex, especially in the thicker part, which has an appearance as if it were due to a kind of normal hypertrophy. Not having actually any fresh male inflorescences, I am unable to say whether their rachis presents the same structure; amongst my old notes I cannot find anything referring to it.

The flowers are so crowded that they touch each other, and hide the rachis and their little bracts completely. This is especially the case towards the apex of the spikes, where not unfrequently two or three empty bracts are found, undoubtedly indications of as many suppressed or aborted flowers. Figs. 1 to 3 represent the male flower; Figs. 4 to 7 the female one. Both have six large sepals and six very small petals; in the former there are three stamens, in the latter three pistils, but in none the slightest rudiment of the organs of the other sex is to be seen. I have carefully examined hundreds of female flowers, and can most positively assert that this rule holds good in every case, nor are there on the female plants any rudimentary male flowers, auxiliary stamens, or any other contrivances that could be considered as pollen-producing organs.

Fig. 8 represents an almost full-grown fruit in its natural size; Fig. 9 is the seed, viewed from the flat side, magnified 25 times; Fig. 10 is its longitudinal section, with the straight embryo *in situ*; and Fig. 11 is a transverse section of the same. The very peculiar wings of the seed develop gradually, as indicated by the letters *a*, *b*, and *c*, in the last-mentioned figure.

I have thought it necessary to give a rather lengthy exposition of introductory matter, before entering upon the principal object of the present paper. The question of parthenogenesis in the vegetable kingdom is still strenuously objected to by many botanists, although it is a thoroughly well-established fact in the domain of zoological science, so that there is *a priori* really no reason for denying its existence in plants. It is, however, of the greatest importance to give in any particular case a most substantial and complete record of the leading facts, as well as of the concurrent circumstances, so as to enable the reader to get a full view of the matter, and form his own judgment accordingly. I hope to do the second, as far as it is pertinent, and shall now proceed to relate my observations on, and experiments with, *Disciphania Ernstii*, from which I have come to the conclusion that in this species we have really *a new case of parthenogenesis in the vegetable kingdom*.

Long before I got the male plant of *Disciphania*, I had noticed that on my female plants appeared now and then a few fruits. Very naturally the idea crossed my mind that I might have before me a case of accidental

[1] It is a singular coincidence that in one of these places another very curious Menispermacea was found, viz. a female plant of *Odontocarya hederæfolia*, Miers.

[2] The caterpillar and pupa of this beautiful moth were on this occasion described for the first time. See a note, "Jugendstadien von *Ophideres cacica*," in Karsch, *Entomologische Nachrichten*, 1885, pp. 6, 7.

parthenogenesis; I was, however, too much interested in obtaining, first, the missing sex, in order to make sure what species of plant was the subject of my researches.

In December 1881 I removed to another house, but before doing so I totally destroyed all my specimens of *Disciphania*, as I wished to begin my further investigations on two young plants which had not yet flowered. These I procured early in 1882, and, most fortunately, both proved to be females. I had some lattice-work placed against the wall of an inner court-yard, and there the plants were left to over-run the whole wall, which is 4·5 metres by 4. As soon as the flowers

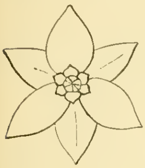

FIG. 1.

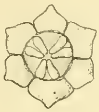

FIG. 2.

FIG. 3.

FIG. 4.

FIG. 5.

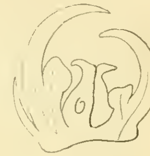

FIG. 6.

FIG. 7.

FIG. 8.

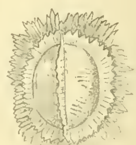

FIG. 9.

FIG. 10.

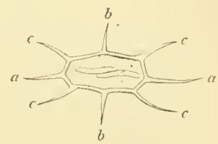

FIG. 11.

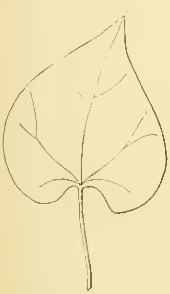

FIG. 12.

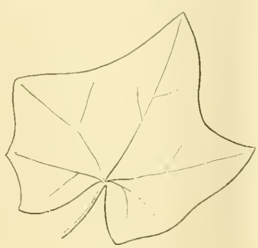

FIG. 14.

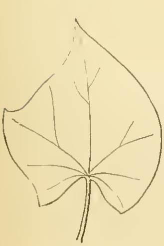

FIG. 13.

FIG. 15.

FIG. 16.

Disciphania Ernstii, Eichl. (After Eichler.)

appeared I proceeded to examine them as carefully as possible, in order to see whether there were any male organs concealed either within or in their neighbourhood, but I searched in vain. In March 1883 the plants had covered the whole space allotted to them, and were then cut down. Branch after branch was once more submitted to the closest inspection, and the existing fruits, numbering 20, were collected. Of these 20 fruits I examined 5, and found that 3 had perfect embryos; 10 were sown in pots, but only 3 germinated; the remainder of the crop I still have in my carpological collection.

The plants soon began to sprout anew, and were again carefully searched whenever time and weather would allow it. They were cut off in December 1884, giving a crop of 54 fruits, which were disposed of in the following manner:—Examined, 10, 7 having perfect embryos; sown, 20; germinated, 9; kept in salt solution, 24.

The third period lasted till February 1886. Both plants were very vigorous, and had produced a large number of flowers, so that I collected no fewer than 137 fruits, of which 10 were examined (5 were good), 20 were sown, (8 germinated, the young plant being now about 6 inches high); the remainder will be sent to different botanical gardens within the tropics, in order that my experiments and observations may be repeated under the most favourable circumstances possible.

Since February the plants have grown a good deal; one is flowering and has already four fruits.

From the very outset of my observations I had noticed that these fruits appeared only on the thickened end of the flowering rachis, which is always the lowest part of the pendent inflorescence, as I have described in the first section of this paper. I cannot help thinking that this circumstance has something to do with the production of these fruits in general.

I must observe that the numbers 20, 54, and 137 of the fruits collected are not the total numbers of fruits produced by the plants in each period; for the earlier fruits fell off and got lost long before I gathered the crop. Whatever may have been this loss, it is certain that there was a constant and very notable increase of fruits. I am sorry I omitted counting all the flowering spikes at the time when the plant were cut down, so as to be able to compare their number with that of those bearing fruits, and to find out, at least approximately, whether both groups of numbers were reciprocally proportionate, or not so.

Be this as it may, *there is no denying the fact that many female plants produced in three successive years an increasing number of fertile fruits without the operation of any fertilising pollen from a male flower.*

The reader will remember that the nearest group of *Disciphania* is at a distance of about nine miles from Caracas. I am quite positive about this point, being well acquainted with the vegetation in the environs of the city. Now it is incredible that under such circumstances and in this species, which has neither showy flowers nor any perceptible smell, the pollination could have been effected by insects. It is furthermore certain that my plants of *Disciphania* are the *only* ones in Caracas, and that for this reason there is no specimen nearer to them than the few spontaneous ones which may grow in the ravine of Guarenas.[1] And even if there had been any pollination from outside, how is it that plants, which are known to be extremely prolific under normal circumstances, should produce such very scanty crops, although grown under the most favourable conditions?

For these different reasons I hold that the possibility of pollination from a male plant is entirely out of the question, and may fairly be discarded

But it is likewise impossible that pollination could have taken place with material produced by the plants themselves, as no rudimentary male flowers, nor auxiliary stamens, have been discovered on them during the most scrutinising search in three successive years; nor was there ever found one single grain of pollen on the hundreds of stigmas that were inspected in the course of this investigation. I well remembered Karsten's criticism of Alexander Braun's paper on parthenogenesis in *Cœlebogyne*, and was, accordingly, very scrupulous to establish beyond all question the absence of any pollen-producing organs. I am fully convinced that I should have found them, if any had really existed.

In another respect I was unable to arrive at a positive result. I could not make sure whether the embryo is developed as an outgrowth from a cell of the nucellus, as Strasburger has found in *Cœlebogyne*, or whether it is the development of an unfertilised oosphere. As, however, the former case appears to be always connected with polyembryony, which does not occur in the seeds of *Disciphania*, it seems to me more probable that in this plant we have an instance of the second case, or of true parthenogenesis, certainly not as the rule and normal *modus* of reproduction, but as an exception, and a very rare one, a kind of makeshift of nature, as it were, when the co-operation of the fertilising material cannot be realised.

In Prof. Weismann's essay, "The Continuity of the Germ-Plasma," there is a chapter on the nature of parthenogenesis, which abounds in suggestions which, in my opinion, throw much light on the case under consideration. I quote the following sentence from Prof. Moseley's abstract in NATURE, vol. xxxiii. p. 157:—"If a special supply of nourishment reaches the germ-plasma, this increases in amount by growth, and thus obtains the mass requisite to start the ontogenetic process, with the result that parthenogenetical development takes place." Strasburger had already pointed out that an analogous result may be arrived at by specially favourable conditions in the supply of food, which counteracts the insufficiency of the germ-plasma. May we not suppose that something similar happens in *Disciphania?* Besides the herbaceous branches, the club-shaped rachis of the spicate inflorescences abound in proteids, and consequently there must be a specially abundant supply of food, which may have some particular influence on the growth of the ovary and its contents. And it is on those places we find precisely the fruits for which I claim a parthenogenetic origin.

Caracas, June 7 A. ERNST

P.S.—Since the foregoing article was sent to England, I have made the following observation, which, I think, gives additional strength to my view as to the probable cause of parthenogenesis in the case under consideration. On one of my plants I had noticed two rather short inflorescences (4 and 5 centimetres long), with but 3 and 4 flowers respectively, *but having a very much thickened, almost club-shaped, rachis*, which measured at the apex nearly 3 millimetres each way. The idea struck me that on these spikes very likely some fruits would appear, and I marked them out for the sake of further inspection. *My anticipations have been fully realised*, as on each of the two there is now one ovary increasing in size, measuring already 3 millimetres by 2, so that there is every reason to expect their final development.

At the same time I may mention that on the second plant, which has just begun to flower, I have found on one spike a *tetramerous*, and on another a *pentamerous*, female flower; both spikes were very small, and had only two flowers. A. ERNST

Caracas, June 23

[1] It is interesting that the second species of *Disciphania* (*D. lobata*, Eichl.) appears likewise to be an exceedingly rare plant, as may be inferred from an observation of of Prof. Eichler's, in the article mentioned before.

OUR ENGLISH TEMPERATURES

A PERIOD of warm weather, lasting for three weeks, set in over the whole of the United Kingdom about August 24, and continued until September 14, over nearly

the whole country. Throughout this period there was no part of England in which the temperature was not above the average in each week, as shown by the returns issued by the Meteorological Office.

The persistent low temperature which preceded this warm weather, and which had so long continued, was described in NATURE for August 12 (p. 341). This cool weather continued till August 23, when, fortunately, the conditions entirely changed, and a warm spell of exceptional length for either summer or early autumn set in. It is necessary to go back to February 1885, eighteen months ago, before we find so long a period with the temperature above the average, and since that time there have not even been two weeks in succession which were warm generally over the whole country.

In recent years there has been but very little really settled warm weather during the three months July to September. Last year there was only one week, ending July 27, which could in any way be termed warm generally over the country during the whole period of three months. In 1884 finer weather was experienced, and there were four consecutive weeks, ending August 25, with the temperature above the average over Great Britain; there was also a period of three consecutive weeks, ending September 29, with warm weather, and two consecutive weeks ending July 14. In 1883 there was only one week, ending September 24, in which the temperature was above the average over the whole of the British Islands, but there were other weeks during the three months in which the temperature was high in several districts; there was, however, no continuous warm weather. In 1882 the week ending August 14 was the only instance with the temperature generally above the average, and in this period a deficiency was shown in the East of England. In 1881 there was not a single week in the three months with the temperature generally above the average. In 1880 there were five consecutive weeks, ending September 13, with the temperature above the normal value, and warm weather was also enjoyed in the week ending September 27. In 1879 temperature was continuously low throughout the period, and the deficiency generally amounted to several degrees; there was not a single district over the whole of the United Kingdom with the temperature above the mean for a single week.

From this it is seen that during the last eight years there were but two years, 1880 and 1884, which can in any way compare with this year for warm weather during the three months referred to, and in the remaining five years there was not a longer period than a single week with continuous warm weather.

The varying conditions with which warm weather occurs in England is exceedingly puzzling. This year it has accompanied weather of a cyclonic type, and has changed to cooler weather with the anticyclonic conditions which set in about September 14. To attempt an explanation of these conditions from observations for our own limited area, or even from the observations over Europe, would be but labour lost. For such an inquiry it is necessary to wait the issue of the synoptic charts for the northern hemisphere which are compiled by the United States Signal Service from the international synchronous meteorological observations. Doubtless a careful study of these will throw some light on the cause of the prolonged irregularities in the distribution of temperature.

CHAS. HARDING

NOTES

THE American Association at the Buffalo meeting unanimously passed a resolution expressing its gratification at hearing of Dr. Gould's proposed revival of the *Astronomical Journal*, and its good wishes for its success.

MR. G. T. PRIOR, B.A. of Magdalen College, Oxford, has been appointed an Assistant in the Department of Mineralogy in the British Museum, to fill the vacancy caused by the death of Dr. Flight. The two vacancies in the staff of the Zoological Department occasioned by the resignation of Mr. E. J. Miers on account of ill-health, and Mr. J. J. Quelch, appointed to the curatorship of the Demerara Museum, have been filled by the nomination after competitive examination of Mr. C. J. Gahan and Mr. Randolph Kirkpatrick. Mr. C. G. Crick has lately been appointed an additional assistant in the Department of Zoology.

WE regret to learn of the death of Dr. Clement Mansfield Ingleby, at the age of sixty-three years. The death is also announced of Admiral Bedford Pim.

WE regret to note the death of Prof. H. A. Bayne, Ph.D., of the Royal Military College, Kingston, Ontario, Canada. Dr. Bayne was a native of Nova Scotia. He graduated in Arts at Dalhousie College, Halifax, N.S., and afterwards spent five years in the special study of chemistry under Wiedemann at Leipzig, Bunsen at Heidelberg, and Dumas at Paris, taking his Doctor's degree at Heidelberg. Returning to his native land he first engaged in organising the Scientific Department of the Halifax High School, acting at the same time as Lecturer on Chemical Analysis at Dalhousie College. In 1879 he was appointed Professor of Chemistry at the Kingston Military College, which had just been founded. His work at Kingston was very onerous, and during the first few years of his professoriate he found little time for original research. At the last meeting of the Royal Society of Canada, of which he was a Fellow, he read a paper of practical value on chemical tests of the purity of silk. He had begun in Germany a series of experiments on the properties of certain of the rarer metals, in which he had been interested by Bunsen; and he hoped to continue them when leisure came. But he has been cut down at the very threshold of his work.

THE annual Exhibition of the Photographic Society was opened to the public on Monday.

NEWS of earthquakes and volcanic eruptions continues to arrive from all quarters. The North American earthquakes have not ceased. Three slight shocks were felt in Summerville on September 28, and at night several more occurred. From various parts of Central Germany, principally Thuringia, news arrives showing that in the night of the 27th, or morning of the 28th ult., there was a series of more or less violent shocks of earthquake. At Gera and other places in Thuringia, the windows, doors, cupboards, and other movable articles of furniture, were violently agitated, shaking and rocking to and fro. A despatch received at New York on Tuesday from Mexico states that a high hill in the vicinity of Chimalapa has been completely riven in two by the action of subterranean forces. The volcano of Colima, in Mexico, is in a state of eruption for the third time within a year. Information has been received at Lerwick stating that two shocks of earthquake had been experienced at Baltasno, Unst, Shetlands, on Monday night. The first shock was felt at eleven, the other two hours later. Several people were roused from sleep by houses trembling and china rattling. Both shocks lasted several seconds, but no damage occurred to property. A telegram from Melbourne, October 5, states that a volcanic eruption has occurred in the island of Niapu, in the Tonga group. Two-thirds of the island are completely covered with volcanic dust. Mount Pabloff, 300 miles south of Kodiak, in Alaska, is in eruption.

MR. S. K. SEKIYA writes from the Imperial University, Tokio, Japan:—"On July 23 quite a destructive earthquake visited Shinano, Echigo, and the neighbouring provinces, over-

throwing several houses, forming fissures on roads and hill-sides, and causing severe damage to household property. The shock also stopped the flow of a hot spring at Nozawa. The part most severely shaken lies among the mountainous district some 3000 feet above the sea, with the famous active volcano of Asama, besides many extinct ones—an interesting case in Japan, as most of the larger earthquakes extend along the sea-shores."

A RECENT number of the *Japan Weekly Mail* contains a short account of a night ascent of the active volcano Asamayama. The party left Karnisawa in the afternoon, and commenced the ascent from the eastern side about sunset. The sky was perfectly clear, and the summit was reached an hour before midnight. The wind, blowing from the south, carried the sulphurous vapour away to the northwards, and thus the ascent was made less uncomfortable. The party saw quite to the bottom of the crater, which presented the appearance of a furnace filled with glowing coals. The sound of the roaring, hissing, and bubbling is described as loud and awful. The walls of the crater are of a light-brown colour, and are composed of successive layers marked out with striking regularity like the seats in an amphitheatre. Allowing ten of these layers to each interval of 20 feet, the depth from the surface to the incandescent matter would appear to be 200 feet. The periphery of the crater is about half a mile, although the Japanese calculate it at two miles and a half.

MR. PERCY SMITH, Assistant Surveyor-General of New Zealand, has made an ascent of Tarawera, where the eruption recently took place. He found the mountain split across, the crack in some places being 5 chains wide. Mr. Smith is now making a minute examination of the district.

ACCORDING to intelligence received at Hamburg, advices from the waters of Spitzbergen now confirm the former news from Iceland and from the mouth of the Pechora, on the Siberian coast, to the effect that the ice in the Arctic Sea has this year extended unusually far southwards. Spitzbergen, the sealers report, was found to be surrounded with an ice-belt 5 to 8 miles broad, and there was firm pack ice from Hope Island to Forland, about 56 miles. The great bays on the Storfjord, Hornsund, Bellsund, and Isfjord were quite inaccessible, and the sealers, after waiting all the spring and most of the summer, returned at the end of August, as there was no prospect of the Polar ice dividing.

PROF. ALEXANDER, who has filled the Chair of Engineering at the Imperial College of Engineering, Tokio, for a number of years past, and who is about to leave the country, has been presented by his colleagues, past and present, with a handsome pair of bronze vases, inlaid with silver, and with an address. The graduates of the college on the same occasion presented him with four pieces of Japanese bronze work. The documents accompanying these presents show that the retiring Professor is much regretted by those whom he leaves behind—colleagues as well as pupils.

ACCORDING to the Report of the Superintendent of the Government Museum at Madras for the past year, the interest of the people in that institution is on the increase. The number of visitors is considerably larger, this being especially noticeable in the case of women and girls, which probably indicates some relaxation of the custom of seclusion, and an increasing interest on the part of Indian women in things beyond, and different from, their ordinary duties. In the work of the Museum there seems to have been an advance in almost every direction. The materials for catalogues of the departments of ethnology and antiquities were collected. Mr. Davison, the well-known Indian ornithologist, was engaged at the suggestion of the Governor for a period of six months to make a collection of South Indian birds for the Museum, and when this has been done a catalogue of the birds will be taken in hand. Similarly in other departments the work of the institution has been progressing. The Superintendent notes that the native visitors object to the drinking-fountain in the grounds, owing to the resemblance that the discharge of water from the mouth of a stoneware lion's head has to the act of vomiting!

THE total railway mileage of the United States is now 130,334, of which 12,116, or 9·2 per cent., is narrow gauge, and 187 broad gauge (5 feet and over). In view of the numerous breaks of gauge, transfer-apparatus enabling quick and easy change under car-bodies of trucks made for one gauge to trucks made for another is a desideratum. A committee of the Franklin Institute has just highly commended Ramsay's system for the purpose. In this, when, *e.g.*, a broad-gauge car is to be transferred to a narrow-gauge line, the car is brought with the aid of side trucks and cross bars over a depressed piece of line having both gauges, and an inclined approach and exit; and it there exchanges one kind of truck for the other. It may be noted that the narrow-gauge system in the United States, far from having "seen its best days," is constantly resorted to in the development of mountainous and sparsely populated districts, California taking the lead in this respect.

WE learn from a French source that Prof. Place, of the Cavalry School of Saumur, has recently applied electricity with great success to horses which prove refractory while being shod. It is known that a vicious beast will often give much trouble in the operation of shoeing, and may even have to be bound and made to lie down. M. Place's method renders it at once tractable, and permanently cures its aversion to the forge. The electric shock is given through a bridle of special form, from an induction-coil actuated by a dry pile.

THE effect of muscular exercise on the temperature of the body has been recently engaging the attention of M. Mosso in Italy. In thermometrical relations the nerves, he concludes, have much greater action than the muscles. Strong emotion will raise the rectal temperature of a dog 0°·5 to 2°, and the same with man. Pain has the same effect. During a walk of two days, M. Mosso observed that his temperature was not in proportion to the work done by his muscles. When dogs rest after long fatigue, one observes that their heat sinks below the normal level, though their muscular exertion has been great. Again, take the evidence of strychnine and curare; the former of which affects the nerve-centres, while the latter paralyses the muscular system. A frog poisoned with curare falls into complete paralysis, with lowering of heat. If a few milligrammes of strychnine be then injected, the paralysis does not cease, but the temperature immediately rises. Internal temperature, then, seems to depend chiefly on the nerve-centres and their greater or less excitation.

WE have received the "Proceedings and Addresses" at the Sanitary Convention held in March last in Howell, Michigan, under the direction of the State Board of Health. The various papers are of a very plain and practical character, free for the most part from technicalities. We have read with especial interest, in view of recent discussions in this country, Prof. Barnes's address on the sanitary conditions and needs of American schools. Speaking of the complaint in England that owing to the requirements of the present code the children of the very poor whose food is bad and insufficient become afflicted with sundry dangerous nervous disorders, Prof. Barnes says that if this be true for England, it certainly is not for the United States. But there as well as here there are critics who hold the system of education to be responsible for all the ills of youth. Prof. Barnes shows that this is absurd. Adequate heating and ventilation he regards as the chief respects in which American

schools are lacking. Indeed it appears from the papers that the other public buildings of the State of Michigan share these defects with the schools; the court-house, according to Mr. Waters, is little better than a death-trap, and the others are far from being what they should be.

WE have received the *Report and Proceedings* (vol. i. part 1) of the Bristol Naturalists' Society. The former is very satisfactory, as it shows an increase in the membership and an increased attendance at the meetings. The finances, too, are in a sound state, with the exception of the special library fund, which appears to be in debt. The Council urge the formation of similar Societies in neighbouring towns, and their affiliation to the Bristol Society, "for so much can be done for science by mutual intercourse and encouragement." The contents of the *Proceedings* (the part being profusely illustrated) reach a high standard. Prof. Lloyd Morgan publishes the third and fourth parts of his "Contributions to the Geology of the Avon Basin," dealing with the Portbury, Clapton, and Portishead districts. Mr. Wilson, Curator of the Bristol Museum, gives the history of the exploration of the bone-cave or fissure in Durdham Down, from which the series of mammalian remains in the Bristol Museum were obtained. Lists are given of the remains found, and of the principal publications referring to them. Mr. Bucknall publishes Part 9 of his "Fungi of the Bristol District," while Mr. Griffiths writes on the *Cicada septemdecim*, "the seventeen-years' locust" of America. There are several other more general papers. At the end is the continuation of the "Flora of the Bristol Coal-Field," edited for the Society by Mr. J. W. White, the compilation of which was commenced seven years ago, and which has continued steadily ever since. One thousand and three species have already been treated as inhabitants of the district. The present part, which is the sixth, treats of *Glumiferæ Gymnospermæ* and *Cryptogamæ Vasculares*. It is proposed to begin at once revising the earlier parts with a view to the preparation of a second edition, and an appeal is made to members and friends to aid in making the work as complete and accurate as possible. It is hoped in this way to lay before the Society a thoroughly worthy account of the botanical wealth of Bristol.

THE Otago Acclimatisation Society have, during the past year, been highly successful in their fish-culture operations in New Zealand. The utmost is being done to naturalise Salmonidæ to the waters of that country, and from all accounts the experiment is likely to succeed. The Society are about to try the experiment of retaining salmon in fresh waters at Marshall's Creek with a view to testing the assertion that it is not a physiological necessity for fish to repair to the sea to render them capable of reproduction.

NEWCASTLE-UPON-TYNE Free Library, though suffering from a fire which arose from the flue of a sunlight in the reference room getting overheated, and caused both reference and issue department to be closed for many days, nevertheless records general prosperity, boasting at the same time the issue of the smallest percentage of fiction of twelve of the largest free libraries in England. A new catalogue of 10,000 books added to the lending department during the five years and a half the library has been open, and another of the juvenile department, are being prepared to meet the want that must be so urgent in a library where crowded applicants can gain knowledge of the treasures offered them by no other channel. Only thirteen volumes, not worth 1*l*., have been lost during the same length of time, not reaching 1 in 100,000 issues; in the juvenile department only 1 in 185,000; and during the past year not one volume! Yet, as if this were not satisfactory, the Committee have decided to make every borrower (*i.e.* over 8500 persons) renew his ticket yearly, instead of once in five years, for the sake of correcting addresses. Balancing this enormous aggregate of trouble to the public, *plus* that of the library officials in carrying it out, against that of the latter in tracing borrowers who have removed without signifying the same to them, we cannot think that this shows either the wisdom of the serpent or the harmlessness of the dove! Science and other educational classes, so aptly carried on under a free library committee, were attended by 141 students in 1884–85, and, we are glad to see, by 186 in 1885–86. Three branch reading-rooms, however, we note with surprise, were not attended sufficiently to make it worth the expense of keeping them open.

THE intelligent community of Watford, to whose various clubs and classes, all working together under one committee, we called attention some time ago, find co-operation so successful that they have published a bound hand-book of 160 pages, containing, besides a library catalogue with supplementary lists down to September 1886, all the other varied information which they have to lay before the public. The principal item to notice is that they have succeeded in adding the University Extension lectures to the numerous branches of art and science already under their care. The name of "The Free Library 'College' of Science, Art, Music, and Literature" has now been adopted, as denoting the "collection" of schools of which it consists. Sir John Lubbock has accepted the new office of President of it, and on Tuesday, the 28th ult., he delivered an opening address. He chose the subject of ants and their ways; and, since natural history is among the scientific pursuits of the college, it was both a specially fitting and, we need not add, a generally interesting subject. The nucleus of all the various work carried on at Watford is a free library rate which even now only brings in 240*l*. a year. Such success should therefore encourage such smaller towns as cannot find separate committees each with its own work and expenditure also.

AMONGST the papers read at the International Congress of Orientalists at Vienna were one by Dr. Stein on the Hindu Kush and Pamir in Iranian geography, and one by Prof. Kuhn on the Indian dialects spoken in the Hindu Kush. Prof. de Lacouperie also read a paper on the aboriginal languages of China.

MESSRS. SPON have in the press: "Metal Plate Work, its Patterns and their Geometry," by C. T. Millis, M.I.M.E.; "A Practical Hand-Book on Pump Construction," by Philip R. Björling; "Spons' Mechanics' Own Book" (second edition); "Quantity Surveying," by J. Leaning (second edition, revised and enlarged); "A Treatise on Secondary Batteries," by Prof. Silvanus P. Thompson, D.Sc., B.A.

THE additions to the Zoological Society's Gardens during the past week include a Mona Monkey (*Cercopithecus mona*) from West Africa, presented by Mr. W. P. Hewby; a Malbrouck Monkey (*Cercopithecus cynosurus*) from East Africa, presented by Mr. Lionel R. Crawshay; a Rough Fox (*Canis rudis*) from British Guiana, presented by Capt. J. Smith; a Hairy-rumped Agouti (*Dasyprocta prymnolopha*) from Guiana, presented by Mrs. Otto Fell; a Common Hedgehog (*Erinaceus europæus*) British, presented by Madame Tina Mazzoni; two Moorhens (*Gallinula chloropus*), British, presented by Lord Moreton, F.Z.S.; a Horned Viper (*Vipera cornuta*) from South Africa, presented by Mr. C. B. Pillans.

OUR ASTRONOMICAL COLUMN

PERSONAL EQUATION IN OBSERVATIONS OF DOUBLE STARS.—M. Bigourdan, of the Paris Observatory, has taken the above as the subject of his Thesis for the degree of Doctor of Physical Science. In an historical review of the subject he refers to the labours of Dawes, W. Struve, O. Struve, Dunér, O. Stone, F. R. Helmert, and T. N. Thielo. He dwells particularly on the researches of

O. Struve, who has persevered so assiduously in the observations of artificial stars initiated by W. Struve. M. Bigourdan shows, however, that by this method it is not possible to eliminate the errors due to the instrument itself, and especially to the object-glass. But it is M. Bigourdan's opinion that it is difficult, if not impossible, to avoid the employment of artificial stars in determining personal equation in measures of double stars, and he has accordingly devised an apparatus, which he describes as simple and of moderate cost, by means of which observations of artificial stars can be made at any time, and which he considers to be free from the objections which have been urged against Struve's method. In this apparatus the plate pierced with holes which form the images of the "stars" and the viewing lenses, are carried by one tube which is movable round a horizontal axis, so as to vary the inclination of the eyes with respect to the horizon; the pierced plate rotating in its own plane so as to vary the angles of position of the artificial coupler. M. Bigourdan gives in the Thesis a great number of measures made with this instrument, deduces a formula for his personal equation in position-angle, and shows how the application of the corrections deduced from it improves the observations.

Publication der Astronomischen Gesellschaft, No. XVIII.—This is a paper by Herr Romberg, of Pulkowa, containing the approximate positions of stars whose places (chiefly as comparison-stars for observations of planets and comets) are given in vols. lxvii. to cxii. of the *Astronomische Nachrichten*, arranged in order of right ascension and reduced to the epoch 1855. This work is a continuation of a similar one executed by Schjellerup for vols. i. to lxvi. of the same periodical, forming No. VIII. of this series of publications, and is similar in form to it. The right ascensions are given to seconds of time, and the declinations to tenths of a minute of arc, and Herr Romberg expresses a hope that his Catalogue may be useful not only as an index to such a large number of observations of stellar positions, but also as a groundwork for accurate determinations of the star-places. The total number of stars in the Catalogue is about 8000, the great majority of them being fainter than the seventh magnitude.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *OCTOBER* 10–16

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on October 10

Sun rises, 6h. 17m.; souths, 11h. 47m. 1·6s.; sets, 17h. 17m.; decl. on meridian, 6° 43′ S.: Sidereal Time at Sunset, 18h. 34m.

Moon (Full on October 13) rises, 16h. 37m.; souths, 22h. 19m.; sets, 4h. 11m.*; decl. on meridian, 4° 7′ S.

Planet	Rises	Souths	Sets	Decl. on meridian
	h. m.	h. m.	h. m.	° ′
Mercury ...	7 7	12 20	17 33	9 57 S.
Venus ...	4 56	11 0	17 4	0 4 S.
Mars ...	10 46	14 49	18 52	21 50 S.
Jupiter ...	6 11	11 46	17 22	5 29 S.
Saturn ...	22 18*	6 20	14 22	21 22 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich)

Oct.	Star	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image
			h. m.	h. m.	° °
12	*f* Piscium	$5\frac{1}{2}$	23 58	0 15†	40 19
14	μ Ceti	4	18 11	19 1	46 272
16	θ^2 Tauri	$4\frac{1}{2}$	18 52	19 36	98 216
16	θ^1 Tauri	$4\frac{1}{2}$	19 1	19 27	127 186
16	80 Tauri	6	19 25	20 4	24 287
16	81 Tauri	$5\frac{1}{2}$	19 36	20 20	32 279
16	B.A.C. 1391	5	20 5	near approach	155 —
16	85 Tauri	6	20 5	20 59	53 255
16	Aldebaran	1	22 37	near approach	155 —

† Occurs on the following morning.

Variable Stars

Star	R.A.	Decl.		
	h. m.	° ′		h. m.
U Cephei ...	0 52·2	81 16 N.	Oct. 14,	5 32 *m*
S Cassiopeiæ ...	1 11·3	72 1 N.	,, 10,	*M*
λ Tauri ...	3 54·4	12 10 N.	,, 10,	3 1 *m*
			,, 14,	1 53 *m*
U Monocerotis ...	7 25·4	9 32 S.	,, 13,	*m*
U Coronæ ...	15 13·6	32 4 N.	,, 12,	18 52 *m*
			,, 16,	5 43 *m*
U Ophiuchi ...	17 10·8	1 20 N.	,, 13,	3 39 *m*
		and at intervals of	20	8
W Sagittarii ...	17 57·8	29 35 S.	Oct. 13,	2 0 *M*
U Sagittarii ...	18 25·2	19 12 S.	,, 11,	19 0 *m*
			,, 14,	19 0 *M*
R Scuti ...	18 41·4	5 50 N.	,, 12,	*M*
β Lyræ ...	18 45·9	33 14 N.	,, 14,	19 0 *M*
R Lyræ ...	18 51·9	43 48 N.	,, 13,	*M*
η Aquilæ ...	19 46·7	0 43 N.	,, 11,	22 0 *M*
V Cygni ...	20 37·6	47 44 N.	,, 15,	*M*
T Capricorni ...	21 15·7	15 38 S.	,, 13,	*M*
δ Cephei ...	22 24·9	57 50 N.	,, 12,	19 0 *m*
S Aquarii ...	22 51·0	20 57 S.	,, 12,	*M*

M signifies maximum; *m* minimum.

GEOGRAPHICAL NOTES

At a recent meeting of the Anthropological Institute in connection with the Colonial and Indian Exhibition, Mr. Pryer, of the Civil Service of British North Borneo, read a paper on the natives of this region. The main race, the Dusuns, are in all probability descendants of a mixed aboriginal and Chinese ancestry, and as we come nearer to the coasts the sub-tribes mix and blend with each other and with aliens, till on the east coast there is very little of the native type left at all, a race rapidly springing up there of very cosmopolitan origin. On the west coast there are more natives and fewer aliens, but much the same thing is occurring there on a smaller scale. It is difficult to say where the Dusun ends and the Dyak proper begins; the former were at one time head-hunters, but they are now settling down under the North Borneo Company quietly to rural toil. They are thriving and increasing, and there is no fear of their melting away and disappearing, as so many races have done when brought in contact with the white man. The same may be said of the sea-coast races, especially of the Bajans. The Sooloos are the principal fishermen, and, like the rest, are now settling down to a more quiet and orderly life. The first true tribe of the interior arrived at from the east coast is the Booloodoopy, a somewhat singular people, many of them having strangely Caucasian features, or, at all events, departing largely from the ordinary Mongolian type. Mr. Pryer describes several other tribes of the interior, and he inclines to the idea that the Chinese type is far more predominant in Borneo, even amongst the Dyaks, than has generally been supposed. He does not think that the Chinese went over in bodies to North Borneo, but rather that in the long course of Chinese trade with and settlement in the island, a slow and steady infiltration of Chinese blood, though not of Chinese speech or manners generally, took place.

At a late meeting of the Paris Geographical Society M. Hamy made a communication on an exhibition of collections from the French possessions in West Africa in the Jardin des Plantes, contributed by the Museum of Natural History and that of Ethnography. The exhibition, he said, was composed of two sections, one devoted to natural history, the other to ethnography. The first showed that the countries were rich in iron and copper, which had ever furnished the natives with indispensable implements and utensils. Trees and various plants abound, and game is not rare. Numerous races live and have lived in these regions. The exhibition shows how they are at present distributed in West Africa. There are four groups of Negroes:—(1) The Negrillos or Pygmies, the most western race. They live now only in small scattered colonies in the most unhealthy spots, such as the estuary of the Ogwe, and the interior of Mayonbo; they are called Okoa, Bongoa, Bakké-bakké, Babonko, &c. (2) The second group is brachycephalous, of middle height, and lives principally on the right bank of the Lower Congo, but particularly on the Loango. They work in iron, and are far advanced in the mechanical arts. (3) The Mpongos, or people of the Gaboon, who have been in contact

with Europeans since the end of the sixteenth century. M. Hamy referred in detail to the manners and industry of this people. (4) This is a slightly heterogeneous group, consisting of the Obambas, Ondumbas, &c., who live in the neighbourhood of Franceville. In conclusion the speaker referred to the interest of ethnological research in relation to the movements of peoples on the earth's surface. Here, he said, ethnography is especially bound up with geography.

THE Portuguese explorers, Col. Serpa-Pinto and Lieut. Cardoza (according to the *Colonies and India*) recently left the Cape Colony for Lisbon. These gentlemen have accomplished a most important scientific exploration in the Lakes region. Leaving Mozambique, they proceeded by land to Ibo, correcting many errors that had crept into the charts. From Ibo they advanced to Nyassa at the head of an Expedition 800 men strong, making as they went a geodetical triangulation of the country, using instruments of great precision. Col. Pinto, in consequence of a dangerous attack of illness, was obliged to leave the Expedition in Mebe's country, of which he had made a geological survey, occupying a long time. Lieut. Cardoza, who had been blind for fifty days, happily recovered his sight in time to take the command of the Expedition, continuing the work to Nyassa, from whence he went to Shirva and Blantyre, and by a new road to Quilimane. The whole party suffered from hunger on the way; all the dogs died of starvation, and the men narrowly escaped the same fate. The Expedition was accompanied by 200 Zulus, who rendered splendid services, being conspicuous for their courage and devotion. Besides their scientific work, the first of the kind done in that part of Africa, the leaders of the Expedition extended the Portuguese dominion over all the important chiefs visited during the journey, lasting twenty months. Everywhere the Expedition was heartily welcomed by the natives, the only place where the Mission was not cordially received being, it is said, the Blantyre Mission Station.

THE September issue of the *Scottish Geographical Magazine* contains a paper by Prof. Meiklejohn on the history, poetry, &c., in geographical names. Mr. Murray, of the *Challenger* Expedition, reviews the existing state of our knowledge of the Antarctic regions *à propos* of the project for exploring them. The Council have unanimously resolved to support any movement having for its object the careful exploration of the Antarctic regions, "as being certain to result in large and important accessions to our knowledge in geography, oceanography, meteorology, and other branches of physical science." They think the expedition should be undertaken at Government expense, but the co-operation of Australasian Governments might be invited. They suggest a conference of delegates of the leading scientific Societies to draw up a memorial to the Government on the subject.

ACCORDING to the latest intelligence received at Zanzibar from the interior of Africa, Dr. Wilhelm Junker, the African traveller, was at Msalala, south of the Victoria Nyanza, and was about to start for Zanzibar. Emin Bey was still at Wadely, and was in urgent need of supplies of ammunition and stores. The King of Uganda had murdered all the English and French converts, and the missionaries were in great danger and had asked for assistance.

PROF. BLUMENTRITT contributes to the last number of *Globus* (vol. l. No. 14) an interesting article on the Manguians of the island of Mindoro, in the Philippines, based on a Spanish work by Don Morera on the geography and natural history of that archipelago. Next to Luzon and Mindanao, Mindoro contains the greatest number of wild tribes. Those which live on the coast and along the banks of the rivers are known under the general name of Manguians, while the Bangot inhabit the plateaus, and the Buquil and Beribi have their villages amongst the high mountains of the interior, but these names vary greatly in different parts of the island. They exhibit a mixture of various races. Besides the Malays, there is Negrito blood in the Buquils, and in some places traces of Chinese descent also. Prof. Blumentritt confines himself to describing the manners, customs, dress, &c., of the Manguians. Incidentally, however, the paper tends to show the enormous complication and difficulty of ethnological questions relating to the Philippine Islands. The constant mingling of different races from China, Malaya, and parts of Melanesia and Polynesia has created a mixture of which the component parts are almost undiscernible. The vast variety of names given to tribes, which rarely mark any ethnical distinction, and which sometimes are given to the members of the same tribe, add to the confusion.

NOTES ON VESUVIUS FROM FEBRUARY 4 TO AUGUST 7, 1886

IN NATURE (vol. xxxiii. p. 367) I gave a description of the changes that had taken place in Vesuvius during the preceding months, and of the eruption of February 4. The lava that issued on that occasion continued to flow in abundance until the 11th, forming a brilliant streak on the northern slope of the cone. After that date the output of fused rock varied at intervals till about the end of the third week in March, when the outflow stopped.

On April 21, at about 5 p.m., lava rose from that portion of the fissure crossing the great crater plain on its south side, and which was the one by which the eruption of May 2, 1885, had taken place. The quantity that oozed out was comparatively small, and sufficed only to flow down the side of the great cone for about 100 metres, so as to just cover the point of exit of the lava that had issued from the same fissure in the spring of 1885. The new lava piled itself up into a kind of boss, and thus soon plugged its own passage.

A few days after, that is, on April 27, a new outburst occurred, again at a weak point—the upper limit of the fissure of 1881–82, above the buttress of lava formed subsequent upon that eruption. This was sufficient to carry off the overflow for some weeks. Slight variations, such as are constantly going on, were observable in the activity of Vesuvius and the outflow of its lava during the whole of the month of May. During the eruption of Etna, Vesuvius did not show the slightest sympathy—just what we should expect when our conception of a lateral outburst is that it is simply a mechanical result of changes that proceed in the upper part of the volcanic chimney, and usually of the mountain itself.

During the month of June the outflow of lava on the eastern side persisted, adding to the great boss, hump, or buttress formed during the years subsequent to the eruption of 1881–82. Owing to the height of the lateral outlet, and probably also from its narrowness, the level of the lava column in the chimney was very high, and, as a consequence, the numerous pasty lava-cakes added much to the size of the eruptive cone, which grew so rapidly during the month as to cover all the old remnants of crater-rings except a small portion of the northern rim of that of 1881–82. On June 29, when I visited the crater, I found a long continuous fissure extending right across the great crater plain in a westerly direction, and emitting an abundance of hot air, HCl, with vapour of chlorides, which were deposited in feathery bunches on the cooler edges of the fissure. This fissure no doubt corresponds to the upper limit of a radial dyke, as did the one existing for many months previous to the eruption of May 2, 1886, and probably does, like that one, indicate the direction of an eruption at some future time. When such an eruption takes place it will be unpleasant for the funicular railway, which, although a little south of the line of fissure, would be within reach of the outburst. In the above-mentioned visit it was possible to watch the eruptive mouth for some time from the edge of the cone of eruption, and to take an instantaneous photograph of it amidst an exciting bombardment of stones, not dangerous for one's self, but unfavourable to an inactive photographic camera. Unfortunately an accident happened to the negative, but I have since been successful in obtaining a permanent record of the eruptive mouth, though hardly such a successful picture. The diameter of the main vent was about 3 or 4 metres, and nearly circular.

The crater was again visited on June 5, but no marked change had taken place, and lava was always issuing on the east side and flowing first to one side and then to the other, always adding to the great buttress.

The cone of eruption, owing to its great increase of size during the last thirteen months, formed a very conspicuous mound, perched as it were on the flattened summit or crater plain of 1872, which truncates the great cone of Vesuvius. On June 28 it was observable from Naples that the cone was falling in, and the spine or boss forming the northern boundary of the crater of eruption had in part disappeared, and owing to the plugging of the passage the smoke only escaped in puffs. This crumbling in of the crater walls was no doubt due to the loss of

support of the column of lava within the chimney. This lowering of level probably produced the extension outwards of the eastern dyke, and the lateral outlet of lava was consequently lowered. This was confirmed by the much increased outflow of lava coincident with the falling in of the cone.

During the first week in July the volcano appeared from Naples to be very quiet; indeed, less vapour was escaping from the summit than at any time during the last seven years. From time to time the vapour was, from the gradually increasing pressure, able to burst its way through the loose materials that choked the outlet, when a puff of smoke would be visible of a dark purplish-black colour, due to its being charged with volcanic ash derived from the churning up and trituration of the lava fragments, scoria, and lapilli it had to traverse in its escape. At the same time a slight reflection was to be seen at night, indicating that the lava surface, although lowered, was not so to any great extent.

On this day, July 8, the lava which had always been gradually advancing, had crossed the southern end of the Val d'Inferno, and flowing down one of the wooded ravines on the property of the Prince of Ottajano, where it destroyed a number of trees, it continued its course, overwhelming some vine-gardens. On July 12 the number and quantity of the black smoke puffs was very great, and the crater was in the full ash-forming stage, and towards night the vent had been considerably cleared, so that the reflection was well marked. The next day the smoke issued freely and uncharged with ash. The change that took place on the 12th was no doubt due to the lava rising in the chimney consequent upon the lateral outlet getting choked; as on the 11th, the abundant flow of lava became very much diminished.

On July 20 the puffs of dark smoke again appeared, indicating a return of the crumbling in of the crater; this was again due to the lowering of the lava level, and, as was expected, the fluid rock issued in great abundance the following day, again destroying trees and vine-gardens. The next day the lava was still flowing in abundance, so as to form a bright streak on the slope of the great cone. The outflow continued to gradually diminish until the 30th, the crater above remaining inactive. On the latter date, however, the lava rose again sufficient within the chimney to cause the vapour to find a passage through the materials choking the main vent, so that on that day the puffs of black smoke were again abundant, and accompanied by the ejection of partially triturated subangular old lava and scoria fragments. In the evening bright bursts were well marked, showing that the vent was again cleared. The two following days the volcano maintained the third degree of activity (*Rep. Brit. Assoc.*, 1885, p. 395).

During the first week of August the lava again flowed rather freely from the mountain's side, whilst from its summit hardly any vapour escaped except from time to time a puff of black smoke. On August 7 a visit was paid to the crater. The cone of eruption has been reduced in height about 30 metres, and its remnants form a low crater ring inclosing a crater of oblong form having a diameter of about 80 × 60 metres. Its greater axis lies in a line from about E.S.E. to W.N.W., and its bottom is double, so that it seems to result from two craters closely overlapping each other. The crumbling-in process was still going on, and the trituration of the loose stones and the charging of the vapour puff by the ash or sand could be watched from a distance of a few yards. I was successful in obtaining two ordinary and two instantaneous photos of the interior of the crater, only the eastern half of which, however, was active.

The principal facts that may be gathered from the study of the phenomena of Vesuvius during these few months are rather confirmations of what the author has described as the mechanism of lateral eruptions, which may be summed up thus. The lowering of lava level within the chimney due to a lateral outlet removes the support the former gave to the walls of the crater and vent, which in consequence tumble in and choke more or less of the main outlet. Next the vapour contained in the lava may be compelled to escape laterally, but has a natural tendency not to do so, but rather to seek its path straight upwards. If the lateral outlet becomes choked, the lava immediately commences to rise in the chimney, and the escaping vapours burst through the loose materials in the chimney in puffs, grinding and triturating them, carrying upwards their dust, which tints the smoke of a dark colour, and, falling around the volcano, constitutes one of the forms of "volcanic ash," the chemical composition of which represents that of all the rocks triturated *plus* the saline substances condensed from the smoke. If one walks across this ash when damp, one may notice the immediate plating of their boot-nails with copper, showing the abundance of the chloride of that metal.

H. J. JOHNSTON-LAVIS

THE ADELAIDE BOTANIC GARDEN AND GOVERNMENT PLANTATION

THE report of Dr. Schomburgk on the progress and condition of the Botanic Garden and Government Plantation, Adelaide, during the year 1885 has just reached us. Speaking first of the rainfall, Dr. Schomburgk says that the year was one of the driest and most ungenial that he ever had to contend with, the rainfall being no more than 15·887 inches, which was 2·851 inches less than the fall of 1884, and 5·272 below the average rainfall during the previous forty years. During September, October, November, December, and January no more than 3 inches of rain fell, and the heat during these months was abnormally great. The drought and heat combined had an injurious effect upon the vegetation, especially upon many of the trees and shrubs in the Botanic Garden, natives of cooler countries; the losses sustained, however, were not so great as was expected, owing to an abundant supply of water. On the other hand, in May and June severe frosts were experienced, so that tropical and sub-tropical plants and shrubs suffered greatly.

On the question of the introduction and acclimatisation of new economic plants, Dr. Schomburgk records his experience with many that have been widely distributed through the agency of the Royal Gardens, Kew, and have become known and established in other colonies as well as in India, such, for instance, as the Kumara (*Ipomœa chrysorrhiza*), the tubers of which form an article of food in New Zealand. Dr. Schomburgk says he believes that the plant will grow well in the gullies, because the climate there is cooler and moister than on the plains, and to some extent approaches that of New Zealand. The Gingelly oil plant (*Sesamum indicum*) is also reported upon favourably. The seeds were sown in drills in the open ground in October, and came up in about fourteen days. Considering that neither the dry spring nor the summer heat affected the plants, there seems no doubt that the species can be successfully cultivated in South Australia. The plant is an annual, and is very largely grown in warm countries for the sake of the sweet limpid oil now so much used for mixing with olive oil.

Under the head of *Rhopala* sp. an announcement is made of the receipt from Kew of a parcel of seeds of a tree belonging to the above-named genus, a native of Columbia, with the following extract from a letter of Mr. W. T. Thiselton Dyer:—"The Rhopala is a small contorted tree growing to about twenty feet in height. It is remarkable for being absolutely indestructible by fire, in large districts where the dry pastures and bush are burnt twice a year. Its resistance to fire enables it to exist to the exclusion of all other trees and bushes as a perfect natural plantation. The periodical burning destroys everything except this tree. The resemblance to a plantation is moreover enhanced by the circumstance that the trees never form thickets, and they are thickly and almost systematically dispersed over the land. The tree delights in the most sterile soils, but always of a stony or shingly character. Sometimes it grows in places so barren that even grass cannot exist. This suggests the idea that it may be turned to account in sterile districts within the tropics." Dr. Schomburgk expresses some doubt whether the plant will thrive out of doors with them, but thinks it may do well in the Northern Territory.

The Herbarium and Museum have both been considerably enriched by additional specimens during the year, so that the utility and efficiency of the whole establishment are thoroughly maintained.

THE AMERICAN ASSOCIATION

FROM the report in *Science* of the Buffalo meeting of the American Association we condense the following brief summary:—

Prof. Gibbs's masterly address, in the Section of Mathematics and Astronomy, upon the subject of "Multiple Algebra," was too long and of too technical a nature for presentation in full to our readers. His opening remarks were as follows:—

"It has been said that 'the human mind has never invented a labour-saving machine equal to algebra.' If this be true, it is but natural and proper that an age like our own, characterised

by the multiplication of labour-saving machinery, should be distinguished by an unexampled development of this most refined and most beautiful of machines. That such has been the case, no one will question. The improvement has been in every part. Even to enumerate the principal lines of advance would be a task for any one—for me, an impossibility. But if we should ask in what direction the advance has been made, what is to characterise the development of algebra in our day, we may, I think, point to that broadening of its fields and methods which gives us 'multiple algebra.'"

The speaker then gave a critical historical review of the different contributions of Hamilton, Möbius, Grassmann, Saint-Venant, Cauchy, Cayley, Hankel, the Peirces, father and son, and Sylvester, to these new methods of mathematical analysis, showing the additions and developments made by each to the various subjects.

In the second part of the paper Prof. Gibbs criticised the methods of some modern writers on these subjects, showing how they failed to grasp the full significance and bearings of the matters they were dealing with, being too much hampered by the old ideas and methods of simple algebra.

In the third part of his paper Prof. Gibbs directed attention more critically to multiple algebra itself, and inquired into its essential character and its most important principles.

Then followed a long discussion of the fundamental conceptions and methods of modern mathematics, which nothing but publication in full could render intelligible, and that only to mathematicians.

The fourth part of the paper was devoted to consideration of some of the applications of multiple algebra. From this we quote the following:—"First of all, geometry, and the geometrical sciences which treat of things having position in space,—kinematics, mechanics, astronomy, crystallography,—seem to demand a method of this kind, for position in space is essentially a multiple quantity, and can only be represented by simple quantities in an arbitrary and cumbersome manner. For this reason, and because our spatial intuitions are more developed than those of any other class of mathematical relations, these subjects are especially adapted to introduce the student to the methods of multiple algebra. Here Nature herself takes us by the hand, and leads us along by easy steps, as a mother teaches her child to walk. In the contemplation of these subjects Möbius, Hamilton, and Grassmann formed their algebras, although the philosophical mind of the last was not satisfied until he had produced a system unfettered by any spatial relations. It is probably in connection with these subjects that the notions of multiple algebra are most widely disseminated. Maxwell's 'Treatise on Electricity and Magnetism' has done so much to familiarise students of physics with quaternion notations, that it seems impossible that this subject should ever again be entirely divorced from the methods of multiple algebra. I wish that I could say as much of astronomy. It is, I think, to be regretted that the oldest of the scientific applications of mathematics, the most dignified, the most conservative, should keep so far aloof from the youngest of mathematical methods; and standing, as I do to-day, by some chance, among astronomers, although not of the guild, I cannot but endeavour to improve the opportunity by expressing my conviction of the advantages which astronomers might gain by employing some of the methods of multiple algebra. A very few of the fundamental notions of a vector analysis, the addition of vectors and what quaternionists would call 'the scalar part and the vector part of the product of two vectors' (which may be defined without the definition of the quaternion)—these three notions, with some four fundamental properties relating to them, are sufficient to reduce enormously the labour of mastering such subjects as the elementary theory of orbits, the determination of an orbit from three observations, the differential equations which are used in determining the best orbit from an indefinite number of observations by the method of least squares, or those which give the perturbations when the elements are treated as variable. In all these subjects the analytical work is greatly simplified, and it is far easier to get the best form for numerical calculation than in the use of the ordinary analysis."

Then followed illustrations of the various methods of applying multiple algebra to different classes of problems.

Prof. Brackett's address on "The Seat of the Electromotive Force" was essentially a *résumé* of the history of the investigations to find the source of the current in galvanic batteries. No attempt was made to settle the question, which has been so long a bone of contention.

In his address to the Section of Biology, Dr. H. P. Bowditch, of Boston, concluded that investigations into the chemical changes, the heat production, and the fatigue of active nerves, all tend to results more favourable to a kinetic than to a discharging theory of nerve action.

In the Section of Anthropology a novel and ingenious method of getting an insight into the unconscious mechanism of authorship was described by Mr. T. C. Mendenhall, under the title "Characteristic Curves of Composition." The method consists in counting the number of words of each length, from one letter to fourteen, fifteen, or as long as were found, and plotting the result on a curve, in which the abscissæ represented the number of letters in the word, and the ordinates the number of words per thousand of each length. It was shown that while the curve resulting from each thousand words was not entirely regular, that resulting from five thousand was much more regular, and that from ten thousand almost entirely so. The inference from this was, that the phenomenon which the curve represented was a regular one, and that it was an expression of the peculiar vocabulary of the author. Moreover, by comparing the respective curves, one would be able to judge whether two works were written by the same author, and perhaps even decide the controversy whether Bacon wrote Shakespeare. Mr. Mendenhall's method was to count a thousand words at a sitting, and then turn to another part of the book. One soon acquired the art of counting at a glance the number of letters in each word, and, with an assistant to record the result, one thousand words could be counted in a half-hour. Curves derived from Dickens ("Oliver Twist") and Thackeray ("Vanity Fair") were remarkably similar, thus suggesting that the subject-matter might cause the peculiarity of the curve, while those from John Stuart Mill ("Political Economy" and "Essay on Liberty") differed from them in having more long words and fewer short ones, though words of two letters (prepositions mainly) were most abundant in Mill. The average length of the novelists' words was 4·38, and that of the philosopher 4·8.

The geological interest of the meeting at Buffalo naturally centred in the excursion to and discussion of the Falls and gorge of Niagara. Dr. Pohlman, of Buffalo, described the district to be visited on Saturday, and called particular attention to the occurrence of drift-filled antecedent channels on the line selected by the post-Glacial overflow of Lake Erie, which would gradually diminish the amount of rock-cutting required in the excavation of the present gorge, and thus reduce the time since the overflow began. The geological members of the excursion party therefore gave close attention to these matters, and, as a whole, regarded the heavy drift between the sloping rocky banks at the whirlpool, and the wide, open valley, with its plentiful drift at St. David's, as sufficient evidence of an old buried channel connecting these points, and probably heading up above the whirlpool towards the bridges. But there seemed no sufficient reason for any confident belief in a branching old valley from the whirlpool towards the Lewiston bluffs: in making this lower part of the gorge there must have been a long period of deep rock-cutting between the first leap of the Falls over the bluff and the time of their discovering the old drift-channel and the whirlpool. The estimate of the age of the Falls was presented by Messrs. Woodward and Gilbert, of the Geological Survey, and their remarks greatly interested a large audience that had gathered on the announcement of the discussion. Mr. Woodward had just completed a survey of the Horseshoe Falls, and by comparing his results with those of the State Survey in 1842, and of the Lake Survey in 1875, he found an average recession for the whole face of the Fall of about $2\frac{4}{10}$ feet per annum; but as the central parts of the curve, where the water is deepest, has retreated from 200 to 275 feet in the eleven years since 1875, an average retreat of 5 feet per annum does not seem at all improbable. Mr. Gilbert then discussed the beginning of the Falls as controlled by the drainage of the lakes. When the retreating ice-sheet stood so as to obstruct the St. Lawrence and Mohawk drainage channels to the east, a broad sheet of water, representing a confluent of Erie and Ontario, stood at a high level over the present Niagara limestone plateau, and probably drained south-westward to the Ohio. When further melting opened the Mohawk Channel, the great double lake fell to a lower level, and was separated into its two members, Ontario sinking to the level of its outlet at Rome in Central New York, but Erie being held higher by the rim of the Niagara plateau. This was the birth of the river and the Falls, and since then they have been at work on the gorge. The age of the falls thus carries us back to a tolerably definite point in

the decline of the Glacial period. On the supposition of a uniform rate of recession, the age of the Falls equals the length of the gorge divided by the annual recession; but the rate has been undoubtedly varied by changes in a variety of conditions, which must be allowed for. As thus qualified, Mr. Gilbert gave it as his conclusion that the maximum length of time since the birth of the Falls by the separation of the lakes is only 7000 years, and that even this small measure may need significant reduction.

In the Section of Chemistry, H. C. Bolton, of the Committee on Indexing Chemical Literature, after presenting their report showing the large amount of valuable work which was being done, read a paper on the confusion which exists in the abbreviations employed in chemical bibliography, and the desirability of uniformity in designations of scientific periodicals.

C. F. Mabery's paper "On the Products of the Cowles Electric Furnace," was of particular interest, and attracted much attention. He stated that the past year had been devoted more especially to the development of an increased commercial efficiency of the furnace, so that now 300 horse-power could by means of a large dynamo, be applied with greater economy in the results; and by coating the charcoal employed in the furnace with lime, by soaking it in lime-water, the production of graphite was largely avoided, and a marked improvement in the working of the furnace introduced. The results—although, as compared to what would eventually be accomplished by electric smelting, they may seem crude—have reached a stage where their commercial success can be demonstrated. It was also found that when the electrodes entered the mixture in a slanting position the product was increased. They are now also moved in and out with advantage, being gradually withdrawn as the resistance falls. Prof. Mabery replied to the criticisms of Hehner of Berlin, Siemens, and others, that no new principle was involved, showing that the Cowles furnace is quite different from all hitherto constructed, and the only one of practical application by which a dynamo of 300 horse-power could be used, as by means of a resistance-box and the arrangement of the furnace, the sudden breaking of the current is prevented from burning out the dynamo. The presence of copper for the reduction of aluminium was shown to be unnecessary; and, by complete exclusion of air from the furnace, buttons of the metal were easily obtained. A product which has attracted considerable attention during the past year is obtained by reducing aluminium in presence of iron. A cast iron is formed containing sometimes as much as 10 per cent. of aluminium, and this product is used to facilitate the working of crude iron, and to introduce into the various grades a small percentage of aluminium. In the reduction of aluminium in the presence of copper a yellow product is frequently taken from the furnace, which is composed of metallic aluminium to the extent of one-half or three-fourths, the balance being silicon and copper. It is also formed in the absence of copper, and then contains a higher percentage of aluminium, and always contains nitrogen. It has a resinous lustre, and decomposes water at 100°.

In the Section of Physics, Prof. T. C. Mendenhall prefaced his paper on "Electric Thermometry" by saying that the strictures upon the mercurial thermometer should not be carried too far. It has been of great value, though it may now fail to meet new demands. Electric thermometry is receiving especial investigation at the Signal Office, particularly from the meteorological stand-point, with some promising results. Prof. Mendenhall reported the progress which had been made in the study of atmospheric electricity during the past year. It is not time to begin to think of the origin of atmospheric electricity. The problem is its distribution and the relation, if there be any, to weather changes. Some very interesting results have been reached. In ordinary weather the electrical condition is undergoing constant and rather wide variations, which are very local, as two collectors only a few feet apart may give curves differing considerably, though similar in their wider variations. When an electrical storm occurs, the curves over a wide area may be similar in general outline. Prof. Mendenhall also noted a phenomenon entirely new to him; namely, that resistance-coils, after a current it passed through them for some time, upon short-circuiting will yield a reverse current for hours. This phenomenon can no doubt be classed under the general head of polarisation, yet by simple polarisation it would be difficult to account for persistence of current. This makes caution necessary in the use of resistance-coils, in order that any effects of this kind may be carefully noted. In one instance the apparent resistance of a coil was found to increase fourfold when the current was reversed.

A paper by Prof. Abbe created some discussion. The point of the paper was that, as the force of gravity varied from the equator to the poles, 30 inches of mercury in the barometer indicated a less gaseous pressure, and consequently less density of the atmosphere, at the equator than 30 inches at the poles, and hence a correction for latitude should be introduced in allowing for refraction. He showed that, for the difference of latitude of Pulkowa and Washington, it would make 0″·1 difference in the refraction at 45° of zenith-distance, and might be sufficient partly to account for differences in systems of star declinations which depended upon observations at great zenith-distances.

In the Section of Biology, the paper of Messrs. J. M. Coulter and J. N. Rose, giving a synopsis of the North American pines, based on leaf-structure, was of especial value from a systematic stand-point, from the fact that any species in this somewhat difficult group can at once be distinguished by the peculiarities of its minute leaf-structure; and the results of the author's observations are shown to be worthy of attention from the fact that a classification based on these characters is, in its broader features, closely like that of the late Dr. Engelmann, which, as is well known, took into consideration the whole tree.

The relations of germs to disease naturally occupied a prominent place in the proceedings of the Section, and the presence of over half a dozen investigators in this line made the discussions interesting. Dr. D. E. Salmon read two papers bearing on the causes of immunity from a second attack of germ diseases. There are three possible explanations:—(1) Something is deposited in the body during the attack which is unfavourable to the germ; (2) something has been withdrawn which is necessary to its development; (3) the tissues have acquired such a tolerance for the germ or for an accompanying poison that they are no longer affected by it. Dr. Salmon favoured the last view, and gave details of a large number of experiments to substantiate his opinion. He said that Metchinkoff's phagocyte theory was not wholly satisfactory, and that large doses of the germs were more powerful than small ones. He attributed their action to a poison which was a result of their growth, and thought that a large dose had a greater effect because the poison benumbed or killed the cells, thus giving the Bacteria a better chance to grow and to thus produce more poison.

Dr. Joseph Jastrow gave an account of some physiological observations on ants, in which he was able, by simple but ingenious means, to study the rate of walk of these insects, and stated that his results, so far as they went, confirmed the opinions of others that the smaller the animal the more rapid the step, and also the more quickly fatigue was produced. Dr. Jastrow also had some observations on the dreams of the blind, taken mostly from persons who had lost the sense of sight before the age of five. In these cases the dreams were all in terms of hearing. In the case of Laura Bridgeman the dreams were apparently based on touch. In persons who become blind between five and seven, sight terms played an important part in dreams. The relation of these facts to the development of the sight-centres was pointed out.

PHOTOGRAPHIC DETERMINATIONS OF STELLAR POSITIONS[1]

IT has been suggested that a short account of my work upon stellar photographs for the attainment of accurate observations might be acceptable to the astronomical section. My intention had been to attend this meeting as a listener and learner only, but I comply with the suggestion the more readily, since, by a notable coincidence, I spoke upon the same subject in this place just twenty years ago this week. It is true that my communication then was only an oral one, and never reduced to writing, for the successful establishment of the Atlantic cable, of which I had received notice that day, called me away suddenly, before the time fixed for the regular presentation; but an elaborate written memoir upon the subject had been presented to the National Academy, ten days previous, at Northampton.

The early history of celestial photography is demonstrably and exclusively American; and its use as a method of delicate quantitative research is very markedly so. Without entering upon

[1] Paper read at the Buffalo Meeting of the American Association for the Advancement of Science, August 20, 1886.

the historical data, which are of easy access to every investigator, I may mention that No. 77 of the *Astronomical Journal* contained nineteen photographic impressions of as many different phases of the solar eclipse of 1854, May 26—the moment of each impression being given to the nearest tenth of a second. These were taken at West Point, under the direction of Prof. Bartlett, of the U.S. Military Academy, and form a part of his memoir, in which he also gives the distances between the cusps, as measured by himself with the micrometer in the telescope. Ten years later, in 1864, Mr. Rutherfurd constructed the $11\frac{1}{4}$-inch photographic object-glass which has acquired so conspicuous a place in astronomical history; and with this, in addition to its other achievements, he obtained sharp photographic stellar images, with a definition previously unknown, taking for the first time distinct impressions of stars invisible to the naked eye, in fact to the $8\frac{3}{4}$ magnitude for white stars.

After constructing a micrometer of great delicacy for the measurement of these plates, he measured with this the relative distances and position-angles of the stars which they contained. And in the spring of 1866 he kindly placed in my hands the results thus derived from three plates of the Pleiades, each containing two impressions, taken on the evening of March 10. One of these plates contained forty stars. Bessel's memoir upon the Pleiades, published in 1844, gave the relative positions of fifty-four stars, measured with the Königsberg heliometer, during the years 1829 to 1841. Six of these fifty-four do not belong within the limits of the plate (which contains about one square degree), and ten of them are too faint for the photographic record, so that sixteen of Bessel's list are wanting; but, on the other hand, there are two additional ones, not observed by him.

From this fact alone it may be perceived that among the great benefits which astronomy may be justified in expecting from celestial photography, the accurate determination of magnitudes does not find place. The chemical action of the stellar light upon the film is so dependent upon the character of that light that, in the absence of a correct knowledge of its composition, we are very easily deceived regarding the amount. Thus one of Bessel's stars which was not recorded upon any of Mr. Rutherfurd's plates is estimated by Argelander as of the magnitude 8·0, and by Wolf as $7\frac{3}{4}$, while five are distinctly recorded which Argelander calls $8\frac{1}{2}$ or less, and eight which Wolf so estimates. The spectroscope would doubtless show a deficiency of the more refrangible rays in the light of the former, and a preponderance of the same in that of the latter.

This series of measurements by Mr. Rutherfurd, together with the computations to which the results were submitted, constitute, if I am not mistaken, the first application of the photographic method to exact astronomical determinations. And the investigation necessarily demanded especial care, both for guarding the numerical results against sources of unsuspected error and for fixing the limits within which known theoretical errors would remain unappreciable.

The importance of the successful application of a method so different from all previous ones, and so full of promise, and also the considerable time which would inevitably elapse before the memoir could be printed, led me at the same time to communicate to the *Astronomische Nachrichten*, at Altona, some of the resultant values. In a comparatively short note, written about the middle of August 1866, I gave for the ten most conspicuous stars of the Pleiades, after Alcyone, the corrections derived from one of the photographic plates of March 10, for the values, published by Bessel, for the position-angles and distances, from Alcyone in 1840, as likewise the average discordance found for a single measure.

In the next following year the Academy had not the means of printing its memoirs; and as in the meanwhile Mr. Rutherfurd had measured five more of the plates of the Pleiades previously taken, as well as six additional ones taken in the months of January and February 1867, these were also computed, and the results added to those from the first three plates in the memoir already written.

Various circumstances combined to delay the publication, chief among them being what seemed to me a manifest impropriety in printing the results derived from photographs and measurements made by Mr. Rutherfurd, and by his own methods, before some account of these methods should have been published by him. His communication on the subject had been made to the National Academy immediately previous to my own, but was not yet in such form as he desired for publication.

The result showed a very remarkable accordance with Bessel's determination for 1840, although the total amount of relative proper motion during the elapsed twenty-six years was comprised in the differences.

This memoir still remains in its original form, but unpublished; the results being deduced from twenty-four photographic impressions upon fourteen plates.

In the next year, 1868, I had the gratification of receiving from Mr. Rutherfurd the results of his measurements of thirty-two stars of the cluster Præsepe, derived from eleven impressions. These were computed in the same way that those of the Pleiades had been, and an analogous memoir upon this cluster was prepared for the National Academy.

Before leaving the country, early in 1870, I gave these two memoirs to Mr. Rutherfurd, with the request that he would send them to the printer, at the same time with his own paper, already mentioned, but not before then. The condition of his health prevented him from attending to the matter for some time, and in the interval he arrived at the unpleasant discovery that the screw of his micrometer had suffered from wear, and to an extent which led him to fear a want of that accuracy of which the method is susceptible, and which he hoped to see demonstrated by its very first applications.

Notwithstanding this possible blemish, it seems to me that the results ought to be now made public in their original form, after due mention of the circumstances; and it is among my hopes to be able soon to publish these two memoirs from the original manuscript of so many years ago.

The method was received with manifest distrust and disregard abroad; and, as was but natural for so essential a deviation from former methods, very many grounds of criticism and objection were brought up. One of the principal of these was the possible distortion of the collodion film, after receiving the impressions and before the measurements; but Mr. Rutherfurd speedily disposed of this point, at least so far as the albumenised plates are concerned; and, moreover, the combination of measurements of the same stars derived from various plates will at once make manifest the degree of confidence to which the several values and their wear are respectively entitled.

A far more serious obstacle to accuracy is presented by the difficulty of obtaining absolutely round images. Irregularity of form in the dots formed by the stellar impressions is almost incompatible with precision of measurement; and, as the time of exposure must often be long, the chief problem was, not so much to obtain the images as to insure uniformity of motion in the telescope during the period of exposure. Not that the photographic processes were not troublesome enough before the introduction of the dry-plate processes, for very great care and numerous precautions were often necessary to prevent the plates from drying too fast; but far the greatest difficulty consisted in obtaining sufficient precision in the clockwork and equatorial motion of the telescope.

It may easily be imagined how great was my desire, when leaving home for South America, to extend this new method of observation to the southern hemisphere. But the obstacles encountered in the endeavour cannot be easily imagined. Upon these I will not enlarge here further than by saying that in Cordova also the attainment of circular dots for the star images offered incomparably the greatest of all the difficulties of a practical character. The time of exposure was limited by the maximum size allowable for the large stars, and, previous to 1878, also by the drying of the plate, although exposures of twenty minutes were not unusual. Nevertheless, by dint of specially constructed governors and regulators, and by ceaseless attention, we did succeed in obtaining impressions which, to the unaided eye, appear absolutely round.

This necessity of long-continued and minute uniformity in the motion of the telescope is, of course, largely diminished by the employment of instruments of large aperture, inasmuch as the necessary time of exposure is diminished in the same ratio in which the amount of light is increased. It is yet further and most notably diminished by the manifold greater sensitiveness of the dry gelatine plates. But notwithstanding all this, the attainment of round images, while almost indispensable for giving to stellar photography that increased accuracy to which it may lay claim as a means of research in practical astronomy, still demands especial care and precaution.

The Argentine Government cordially afforded every assistance which I deemed it proper to ask for these investigations. And although the chief energies of the Cordova Observatory

were absorbed by those investigations for which the institution was established, I had the satisfaction of obtaining a sufficient number of stellar photographs to occupy not only my own lifetime, but many more, in their measurement and proper computation.

We photographed no northern stars there except the Pleiades and the Præsepe. Of the Pleiades I brought home sixteen plates, with two impressions of the whole group upon each, made in five different years, from 1872 to 1882, inclusive. Although the centre of the cluster never attains a greater altitude at Cordova than 34° 50′, some of the plates contain seventy stars. All but one of Bessel's stars are there, which belong within the limits of the field, the missing one being of the magnitude 9½, and there are yet other stars of the magnitudes 10, 10½, and 11. Of the Præsepe there are five plates, and with a correspondingly increased number of stars.

About seventy southern clusters have been repeatedly photographed at Cordova, comprising all those of the southern hemisphere which seemed important, also somewhat more than a hundred double stars, being a sufficient number to serve as a good test of the method. The total number of photographs now on hand is somewhat less than 1300, only few having been preserved in which the images were not circular.

Especial attention, however, was given for many years to taking frequent impressions, at the proper seasons, of four stars selected, on account of their large proper motion, as likely to manifest appreciable annual parallaxes. The refined and elaborate observations of Drs. Gill and Elkin, at Cape Town, have been made, computed, and published, while the Cordova photographs have lain untouched in their boxes. There is but one of my four stars, β Hydri, which is not included in their list. Still, it will be a matter of much interest to apply the photographic investigation to the same problem, even if for no other purpose than a comparison of the results of the two methods.

I am convinced that the Cordova plates contain a large number of stars as faint as the eleventh magnitude of Argelander's scale, and believe that these are much the earliest photographs of stars fainter than Mr. Rutherfurd's of 1865 and 1866. There are several plates, covering about a degree square, which cannot contain less than 550 stars, and I believe that some of them contain a greater number. Such are those of the cluster Lac. 4375 and that near X Carinæ.

The region in the vicinity of η Carinæ, and that magnificent tract in Sagittarius which is too densely sown with stars to be considered merely a portion of the Milky Way, and yet too large and undefined to be regarded simply as a cluster, were both of them taken several times, during the years 1875–82, in series of overlapping photographs, each containing about a square degree, and recorded upon a glass surface of 9 by 12 centimetres. In their present form they are of course of small value for scientific use, inasmuch as the stars are too crowded for their configurations to be easily perceived; and although these two series form, in fact, maps of considerable regions in the sky, still the record is of a very perishable nature, and of small avail for use by astronomers until it shall have been translated into an enduring and numerical form by micrometric measurement.

In this connection I may say that one of the greatest of my present anxieties regarding the Cordova photographs arises from a discovery of the ease with which the collodion or gelatine film may become detached from the glass. The Argentine Government has assigned a moderate sum for the prosecution of the measurements, and with this some progress has already been made. It is but right to add that the full amount was given for which I asked. Still, it is now quite inadequate, in consequence of the unfortunate depreciation of the national currency; and, in the present financial crisis there, I cannot reasonably expect more. Yet this matter of prompt measurement appears to me at present much more important than it did while I was unaware of the facility with which the film can blister and peel.

In 1883, after Mr. Common's brilliant success in photographing nebulas with his great 3-foot reflector, he proposed to me a joint arrangement for photographing the whole heavens. My work at Cordova was so near its close that it was out of the question to undertake anything new; but the immense labour requisite for the measurement of the plates would, under any circumstances, have tended to deter me. It is an undertaking demanding the joint energy, application, and material resources of a large number of persons, if the results are to be made available for astronomical use; indeed, I see no other astronomical value in the unmeasured photographs than the possibility of confirming at some future epoch the existence of relative motion previously detected or made probable by some other investigation.

Since then the process of photographic charting is said to have been systematically undertaken by the Brothers Henry at Paris. I have seen none of their plates; but their sharpness is highly spoken of, and the work appears to be prosecuted with much skill and very sensitive plates. There can of course be no question as to the value of any permanent record whatsoever, corresponding to a known date; yet I cannot feel that any essential advance is likely to be made in this way until the photographic record shall have been brought within the range of numerical expression.

The measurements of the Cordova photographs, thus far completed, are those of the double stars, the four stars with large proper motion, of the Pleiades, of the Præsepe, and of the clusters Lac. 4375 and κ Crucis. The corresponding computations have been made, as yet, only for a portion of the Pleiades impressions, but I am hopeful of completing all these at a comparatively early date. We shall then be able not only to compare the results with Bessel's of forty-five years ago, but to test the deduced values of the proper motions by means of the photographic determinations of 1865 and 1866. Meanwhile, the valuable memoir of Wolf has been published, giving closely approximate positions for 571 stars of the group, and Dr. Elkin has recently been executing at New Haven a heliometric triangulation of the principal stars. Our photographic results will have to be confronted with his delicate heliometric ones; and, should they bear this test with tolerable success, it will be all that can reasonably be desired.

B. A. GOULD

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

PROF. W. GRYLLS ADAMS, F.R.S., will deliver, at King's College, London, a course of lectures on Electricity and Magnetism and their applications to Electric Lighting, Transmission of Power, &c., during the academical year 1886–87. A course of practical work in Electrical Testing and Measurement with especial reference to Electrical Engineering will also be carried on under his direction in the Wheatstone Laboratory. In the Wheatstone Laboratory, which is open daily for research from 1 to 4, except on Saturdays, there are special courses of practical work for students preparing for the Science Examinations of the University of London.

IT is purposed to celebrate, in a befitting manner, from November 6 to 8 next, the 250th anniversary of the founding of Harvard University, Cambridge, Massachusetts. The University was established on November 7, 1636, by an Act of the Colonial Legislature, and was named after John Harvard, who was a liberal benefactor of the new institution. Harvard is the oldest University in North America. The southern portion of the Continent possesses one many years older—the University of St. Mark, at Lima, founded in 1551 by the Emperor Charles V.

SCIENTIFIC SERIALS

Bulletin de l'Académie Royale de Belgique, July.—Modern kinetics and the dynamism of the future, by G. A. Hirn. The author replies to the arguments urged by M. Clausius against his view of the kinetic theory in its application to the resistance of gases. He continues to treat the question in connection with his peculiar spiritualistic opinions, and endeavours to overthrow the theory now generally accepted by physicists, because of the disastrous consequences which he supposes it would have on the progress of mankind.—On a class of conjugated polynomes, by J. Deruyts. This memoir, which is a further development of the author's previous researches, deals more especially with the important functions presented by certain polynomes in the approximate calculation of definite integrals.—On the distribution of the regenerate nerves, by C. Vauclair. In this paper the author deals with the peripheric distribution of the regenerate nerves compared with that of the primitive nervous system.—Essay on the origin of the Fraunhofer rays in relation to the

constitution of the sun, by Ch. Fievez. Given the high temperature, chemical composition, and slight mean density of the sun, its chemical elements cannot exist in the solid or fluid state, or even to any large extent in the condition of highly compressed vapours. Assuming, further, with most physicists, that the sun consists of a gaseous mass whose temperature increases from the circumference towards the centre, it is argued that the solar spectrum must be formed by the superposition of all the radiations of the chemical elements present in the sun. The luminous part of the spectrum would thus be constituted by the radiations of like vibratory period, and Fraunhofer lines by the radiations of unlike vibratory periods. From this it would follow that a chemical element might exist in the sun without being revealed by a dark line in the solar spectrum.

Schriften der Physikalische-Ökonomischen Gesellschaft zu Königsberg i/Pr., 1885.—Herr Fritsch here gives the result of a study of certain gaps in the pith of Coniferæ, discovered by Prof. Caspary some years ago. In its middle course the pith of a year's growth consists of elongated parenchyma, but at the end of the growth it presents cubic or egg-shaped cells in loose union. Those gaps the author finds in species of the genera Abies, Picea, and Larix, and in *Cedrus Deodara*. The last-named differs from the others in not having a partition of cross-lying cells through the pith, above the gaps. All Coniferæ with persistent bud scales have this, and some Coniferæ (*Tsuga canadensis* and *Torreya nucifera*) have it, and are without the gaps. Finally, the Junipereæ, Podocarpeæ, Taxineæ, and Pinus have neither. The gaps seem to arise through stretching of the wood-cells, and their size depends on the age and moisture of the stem and branches.—Herr Franz writes on the magnetism observed at the end of long iron well-tubes (200 and 250 m.) at Königsberg, and of some railway lines. The attractive force was proportional to the distance (not its square), the magnetism being pretty equally distributed over a line several metres long. In one well, the horizontal component at 1 metre distance was as much as fifteen times that of the earth's magnetism.—Herr Klien describes experiments in plant cultivation by the water-method (specifying the substances given in solution and their amount), and points out its advantage in study of the action of poisons, such as the sulphocyanite of ammonium occurring in ammonia superphosphate from gas manufacture, and spoiling that product for manure purposes.—A paper by Herr Scharlop appears to throw light on the production of some prehistoric urns in Prussia, from a mode of manufacture which has lately died out.—Dr. Tischler discusses the representations of weapons and costumes on old bronzes of the Hallstadt-Italian period.

SOCIETIES AND ACADEMIES

London

Entomological Society, September 1.—Robert McLachlan, F.R.S., President, in the chair.—The following gentlemen were elected Fellows:—The Rev. Prof. Dickson, D.D., and Messrs. P. Cowell, A. O. Walker, and Lyddon Surrage.—The President remarked with regard to the gnats from the Kent Waterworks, exhibited at the last meeting, that Prof. Westwood had since informed Mr. Douglas that they were only *Culex pipiens*.—Mr. Slater exhibited certain parasites found on the body of a larva of *Smerinthus tiliæ*, which Mr. Waterhouse believed to be *Uropoda vegetans*, a species of *Acari*.—Mr. W. Warren exhibited *Eupithecia fraxinata*, *E. innotata*, a variety of *E. satyrata*, a *Gelechia* caught in Wicken Fen twenty years ago by Mr. Bond, and believed to be a new species, *G. fumatella*, *G. vilella*, *Lithocolletis scabiosella*, and *Catoptria parvulana*. He also exhibited larvæ of *Gelechia vilella*.—Mr. South exhibited specimens of *Dicrorampha distinctana*, and stated that he considered it to be merely a local form of *D. consortana*, from which, in the larval stage, it could not be separated.—Mr. Stevens exhibited a living specimen of *Clerus formicarius*, recently found under the bark of an ash-tree in Arundel Park.—Mr. Billups exhibited *Chrysis succincta*, Linn., taken by sweeping at Chobham on July 28 last; he also exhibited *Microphysa elegantula*, taken at Broadstairs in August last.—The Rev. W. W. Fowler exhibited, on behalf of Mr. Theodore Wood, a larva of *Langelandia anophthalma*, a species new t[illegible] Britain.—Mr. H. Goss exhibited specimens of *Oxygastra Cu*[illegible] recently taken near Christchurch, Hants. He stated tha[illegible]d met with the species in the same locality in 1878, but n[illegible]r seen it anywhere else in the United Kingdom, nor was he aware of any recent record of its capture. Mr. McLachlan observed that the species was taken many years ago in Dorsetshire by the late Mr. Dale, but that he knew of no recent captures except those recorded by Mr. Goss. He also made some remarks as to the distribution of the species on the continent of Europe.—Mr. McLachlan exhibited a specimen of *Dilar meridionalis* taken by him in July last in the Pyrenees, also about 150 examples of the genus *Chrysopa* from the same district. Amongst them were *C. vulgaris*, *perla*, *Walkeri*, *viridana*, *tenella*, *prasina*, *flava*, *septempunctata*, *flavifrons*, and others not yet fully identified. He also exhibited a few Coleoptera from the same district, and remarked on the extraordinary abundance of a pretty Lamellicorn, which was so common as to give the meadows the appearance of being studded with multitudes of brilliant blue flowers.—Mr. C. O. Waterhouse called attention to the numerous reports which had lately appeared in the newspapers of the supposed occurrence of the Hessian Fly (*Cecidomyia destructor*) in Britain, and inquired whether any communication on the subject had reached the Society. The Rev. W. W. Fowler stated that he had been in communication with Miss Ormerod on the subject, and that she had informed him that neither the imago nor larva of the species had been seen, and that the identity of the species rested on the supposed discovery of the pupa.—Mr. A. H. Swinton communicated a paper entitled "The Dances of the Golden Swift." In this paper the author expressed an opinion that the peculiar oscillating flight of the male of this and allied species had the effect of distributing certain odours for the purpose of attracting the females.

Paris

Academy of Sciences, September 27.—M. Émile Blanchard in the chair.—Researches on the sugars, by M. Berthelot. The results are given of recent studies of some new principles obtained from the association of sugars with themselves, not by a stable combination of the class of saccharose substances, but by a combination easily dissolved, analogous to that of the hydrates and alcoholates. The facts observed illustrate the difficulties so often met with in the preparation of the double salts. They supply a fresh proof of the special part played by the dissolvents in the extraction of immediate principles, for, according as water or alcohol is employed, melitose or raffinose may be obtained.—Conditions determining the rapidity of images in chronophotography, by M. Marey. By the process here described, which is based on M. Chevreul's method of obtaining a perfectly black ground, the author is enabled to reduce the time of pose for each image to the two-thousandth of a second, and hopes by further improved dispositions to reduce it still more. The new photographs show that this reduction of time greatly increases the delicacy of the images obtained by this process of chronophotography.—Kinematic analysis of the locomotion of a horse, by M. Marey. In this paper are described and illustrated the movements of the fore-leg in the step, trot, and gallop. The tendency to economy of labour displayed in various degrees in the movements of all "animal machines" appears to attain the greatest perfection in the action of the horse, being, however, less evident in the trot and the gallop than in the slow pace.—Note on the removal of the Imperial Observatory of Rio de Janeiro to a new site, by M. Cruls. The new site, to which the Observatory will soon be removed, occupies about 40 hectares (100 acres) of the Imperial Fazenda of Santa Cruz, the usufruct of which is granted by the Emperor for this purpose. The new Observatory will stand on the same parallel, and about 2 metres to the west of the present establishment, and will be able to undertake observations both on atmospheric electricity and terrestrial magnetism much more successfully than was possible in its old home.—On the transformation of algebraic surfaces in themselves, and on a fundamental number in the theory of surfaces, by M. E. Picard. Having recently shown that surfaces capable of transformation in themselves by a birational substitution, including two arbitrary parameters, are of the genus zero or one, the author now examines the case of a single parameter, which he finds leads to totally different conclusions.—On a new method of determining the coefficient of expansion for solids, by M. Robert Weber. If a solid body be suspended like a pendulum, its oscillations will depend upon its form, its mass, and the distance of its molecules from the axis of rotation. At two varying temperatures this distance varies, whence results a change in the oscillations. Hence for a given body there is a determined relation between its temperature, u, the coefficient

of expansion, *a*, its dimensions, *d*, and time of oscillation, *t*. The value of *a* with these data may be calculated by the process here described, and in a future communication the author promises some values of coefficients of expansion determined by this method.—On the microscopic flora of sulphurous waters, by M. Louis Olivier. While prosecuting his researches on the reduction of the sulphates by living beings, the author has been led to the discovery of low organisms in sulphurous cold and thermal waters. These organisms are found to be very active at very high temperatures, thriving and multiplying themselves in the hot springs of Des Œufs (Cauterets), and elsewhere, at temperatures of from 46° to 50° C. Carefully collected and transplanted to an extract of beef, they continued to propagate at 65°, and even nearly to 70° C.—Influence of the organism of the guinea pig on the virulence of tuberculosis and scrofula, by M. S. Arloing. It results from several experiments that the virus of scrofula is not intensified by its presence for two generations in the guinea-pig. But the effect is different with true tuberculosis, which in its attenuated forms acquires by inoculation sufficient virulence to affect the rabbit, an animal otherwise so difficult to infect with this poison. —On the vascular system of the Echinidæ, by M. Henri Prouho. In reply to a statement recently made by M. Kœhler, the author shows by numerous quotations that, except on two points, their views are not in accord on the vascular system of these organisms.—The earthquake of August 27, 1886, in Greece, by M. Léon Vidal. The paper contains a detailed account of the disturbances in various parts of the mainland and adjacent archipelagoes, from which it appears that the phenomenon was due to a general cause situated somewhere to the south-west of the Island of Alphios, beyond the Strophades.—Remarks on a chart representing the Granitic and Cretaceous formations of the Spanish Pyrenees, and their disposition in a series of oblique ridges, by M. F. Schrader. On this map, drawn to a scale of 1 : 200,000, the author gives the results of his own surveys in a deep colour, marking off the districts which he has not yet visited, and for which he has utilised the works of Dufrénoy and Élie de Beaumont.—Explanation of the solar spots and faculæ, by M. J. Delauney. To explain these phenomena it is assumed that the sun consists of a very hot nucleus of metals in the fluid state wrapped in an atmosphere at a very high temperature and pressure, and formed almost entirely of hydrogen; further, that the nucleus contains in solution a large quantity of gas derived from the atmosphere; that the atmospheric pressure is least at the poles and at the equator, with a maximum at low latitudes on either side of the equator; lastly, that this atmosphere is subject to variations of pressure. The spots would then be caused by any atmospheric depression in any region of the solar surface, while the faculæ would correspond to an inverse phenomenon, the atmospheric hydrogen being absorbed or dissolved by the nucleus under the influence of high pressures. The spots would be the result of a cyclone, the faculæ of an anti-cyclone, the former being accompanied by a diminution of heat employed to transport the hydrogen from the interior of the sun to and even beyond the atmosphere, while the latter represent a liberation of heat resulting from the precipitation of the hydrogen absorbed in the solar mass.

STOCKHOLM

Academy of Sciences, September 15.—A refutation of the remarks of Dr. Hoppe on the new theory of unipolar induction, by Prof. E. Edlund.—On the Salmonidæ of the Swedish State Museum with reference to a work recently published on them, by Prof. F. A. Smith.—On the new parts (15 17) of "Algæ aquæ dulcis exsiccatæ quas distribuerunt," V. Wittrock and O. Nordstedt, exhibited and commented upon by Prof. V. Wittrock. Researches on the general Jupiter-perturbations of the asteroid Thetis, by Herr C. V. L. Charlier.—Some new developments of the elliptic functions, by Prof. Hugo Gyldén.—On the habits of two Swedish species of the solitary wasps, by Prof. Chr. Aurivillius.—On a new nitro-naphthaline-sulphur-acid, by Prof. P. T. Cleve.—On glycolurite and acetylurea, by Prof. O. Widman.—On the products of oxidation of the ortho-nitrocumenolocryl-acid and its combinations, by the same.—New researches on the re-arrangements of the atoms in the propyl group, by the same.—On the curve of coincidence of the common algebraic differential equations of the first order, by Prof. C. F. E. Björling.—On the integration of the differential equations in the problem of the N-bodies, iii., by Prof. Dillner. —On the connection between the coefficients of expansion and the coefficients of elasticity at different degrees of temperature, by Prof. G. R. Dahlander.—On the determination of sulphur and haloods in organic combinations, by Dr. P. Klason.

BOOKS AND PAMPHLETS RECEIVED

"Lehrbuch der Vergleichenden Anatomie der Wirbelthiere," by Prof. Dr. R. Wiedersheim (Fischer, Jena).—"Edinburgh Astronomical Observations," vol. xv., by Prof. P. Smyth (Neill and Co.).—"Pictorial Arts of Japan," part 4, by Wm. Anderson (Low and Co.)—"A Treatise of Spherical Trigonometry," part 2, by W. J. McClelland and T. Preston (Macmillan and Co)—"Illustrated Hand-book of Victoria, Australia Ferres, Melbourne).—"Journal of Statistical Society," September (Stanford).—"Euclid Revised," Books i. and ii., by C. J. Nixon (Clarendon Press)—"Handbook of Zoology," 3rd edition, by Sir J. W. Dawson (Dawson, Montreal).—"Bulletin of the Amer. Mus. of Nat. Hist.," July 1886 (New York).—"British Fungi," vol. ii., by J. Stevenson (Blackwood).—"Challenger Reports, Zoology," vols. xv.-xvi. "Explosions in Coal-Mines," by W. N. and J. B. Atkinson (Longmans).—"Philosophische Studien," Dritter Band, 4 Heft, by W. Wundt (Engelmann, Leipzig).—"Proceedings of the Boston Society of Natural History," vol. xxiii. part 2 (Boston).—"Memoirs of the Boston Society of Natural History," vol. iii. No. 12, by W. K. Brooke; No. 13, by S. H. Scudder (Boston).—"Des Mesures absolues de la Chaleur rayonnante," by K. Angström (Upsal)—"Stonyhurst College Observatory: Results of Meteorological and Magnetical Observations, 1885," by Rev. S. J. Perry.—"Rules regarding Defects of Vision," by Sir J. Fayrer (Churchill).—"Ancient and Modern Methods of Arrow Release," by E. S. Morse (Essex Inst.).—"Results of Experiments at Rothamsted on the Growth of Barley," by Prof. J. H. Gilbert.—"Descriptive List of Native Plants of South Australia Recommended for Cultivation," by J. G. O. Tepper.

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THURSDAY, OCTOBER 14, 1886

SCIENTIFIC WORTHIES

XXIV.—JOHN COUCH ADAMS

PROFESSOR J. C. ADAMS, whose portrait we this day present to our readers, entered St. John's College, Cambridge, in 1839. He soon gave promise of those great mathematical powers that have brought such renown to his University. He came out as Senior Wrangler in 1843, and the excellence of his answering is still a tradition at Cambridge.

By what seems to have been an inspiration of genius, he was guided after taking his degree to concentrate his talents on the solution of an astronomical problem of excessive interest but of corresponding difficulty. The planet Uranus had shown irregularities in its motion. The orbit differed from the elliptic path which an undisturbed planet would pursue, and the deviations could not be fully accounted for by the influences of the other known planets. The only explanation of the discrepancy which astronomers could be expected to favour lay in the supposition that there was some other still more remote planet yet unknown.

It was the search for this unknown planet which attracted the distinguished Senior Wrangler. We can imagine the delight with which a well-equipped mathematician would throw himself into the solution of such a problem. On it he was to concentrate the powers that had been cultivated during his University career.

The planet was to be sought for by the measured deviations of Uranus from its calculated positions. Those who have ever had occasion to study the planetary theory are well aware of the difficulty and the laborious intricacy of the subject. To most of us it has seemed a thorny and difficult problem when the planet is given to find the perturbations. What are we to say of the difficulty of the converse problem, Given the perturbations and find the planet! This was the problem which Adams faced, and which, to his imperishable fame, he succeeded in solving. The story of this discovery is familiar to all, and the controversies that arose have long since died away. To each of the joint discoverers, Leverrier and Adams, the gold medal of the Royal Astronomical Society was presented on February 11, 1848. In his address on the occasion, Sir John Herschel, speaking of the two astronomers, says:—

"M. Leverrier and Mr. Adams—names which as genius and destiny have joined them, I shall by no means put asunder; nor will they ever be pronounced apart so long as language shall celebrate the triumphs of Science in her sublimest walks. On the great discovery of Neptune, which may be said to have surpassed, by intelligible and legitimate means, the wildest pretensions of clairvoyance, it would now be quite superfluous for me to dilate. That glorious event and the steps which led to it, and the various lights in which it has been placed, are already familiar to every one having the least tincture of science. . . . I will only add that as there is not nor henceforth ever can be the slightest rivalry on the subject between these two illustrious men—as they have met as brothers, and as such will, I trust, ever regard each other—we have made, we could make, no distinction between them on this occasion. May they both long adorn and augment our science, and add to their own fame, already so high and so pure, by fresh achievements."

The discovery of Neptune was a brilliant inauguration of the astronomical career of Adams. We cannot here enter into a detailed account of his numerous labours. There is an admirable account given of them up to the year 1866 in the address of Mr. De la Rue to the Royal Astronomical Society, when Adams was again the recipient of a gold medal. We find that he has worked at and written upon the theory of the motions of Biela's comet; he made important corrections to the theory of Saturn; he made an elaborate investigation of the mass of Uranus, to which he was naturally attracted from its importance in the theory of Neptune; he has improved the methods of computing the orbits of double stars: but next to the discovery of Neptune the fame of Adams mainly rests on his researches on the moon and on the theory of the November meteors. To each of these subjects we must devote some attention.

In the *Phil. Trans.*, vol. cxliii. part iii. p. 397, his paper was published "on the secular variation of the moon's mean motion." This memoir originated a long controversy, in which the ablest mathematicians have participated.

The "secular acceleration of the moon's mean motion" is the phrase which denotes a gradual but excessively slow diminution in the moon's periodic time. Although the amount of this diminution is very minute, yet the fact that it always tended in the same direction rendered the amount accumulative, so as to become very perceptible in the succession of ages. The explanation of the acceleration formed a problem on which the great mathematicians at the close of the last century exercised their powers, and at length the explanation was given by Laplace. He found that, when the analytical expression for the moon's mean motion was developed, it contained certain terms depending upon the eccentricity of the earth's orbit. This eccentricity varies in consequence of the planetary perturbations, and hence the changes of the moon's mean motion. Laplace calculated the amount, and deduced, or thought he had deduced, a correspondence between the observed value and the calculated value. The high authority of Laplace, and the brilliant success of his efforts to explain other perturbations in our system, led to an acquiescence in his results, and the great problem of the secular acceleration was believed to have been solved.

In Mr. Adams's paper he joined issue with Laplace. The question was fortunately one which did not involve any real element of uncertainty. It was a problem in mathematics, or rather dynamics—difficult no doubt, but not really open to any ambiguity. Prof. Adams showed that Laplace had only considered a part of the disturbing influence, and when the true amount was determined, it came out to be only about one-half that found by Laplace. So serious a charge received the careful consideration which it merited. Several leading mathematicians impugned the calculations of Adams, but he was able to vindicate his theory at every point, and finally the correctness of his calculations was verified in one manner by M. Delaunay, and in another by Prof. Cayley. The importance of this result is not to be estimated merely by its value as a correction to Laplace. The author of the "Mécanique Céleste," like other writers, makes errors in his work. Numerous errors have

been detected by Laplace's commentators, but the secular acceleration occupies quite a distinct position. It must be remembered that the calculations of Laplace appeared to render a physical explanation of a remarkable phenomenon. But when this calculation was shown to be seriously wrong, it followed that the cause of the secular acceleration conjectured by Laplace was inadequate to explain the observed facts. The labours of Adams thus reopened the breach between the observations and the theory. The variations in the eccentricity of the earth's orbit will account for part of the secular acceleration. The other part has to be accounted for in a different way. The theory of the tides seems to offer an explanation of the discrepancy. Owing to their incessant action the period of the diurnal rotation has been slightly elongated, and this effect, when duly taken account of, seems to remove the margin between the theoretical and observed values of the secular acceleration. This margin has indeed a singular interest in the recent theory of tidal evolution, inasmuch as it affords us the only measurable indication we have of the effect of tides on the earth's rotation.

The splendid shower of shooting-stars that occurred in November 1866 fixed the attention of astronomers on every part of the theory of these bodies. We were thus taught much concerning them, but for one of the most recondite parts of their theory we are indebted to the labours of Prof. Adams. It had been known that the great displays of the Leonids (for so these shooting-stars are called) take place every thirty-three years. From the year 902 down to the year 1866 many of the successive thirty-three-year periods witnessed the great shower, and records of a considerable number have been handed down to us.

These minute bodies must revolve around the sun, each pursuing its orbit in accordance with the laws of Kepler. It became of interest to find the size and shape of this orbit, as well as its position. Certain features of the orbit are readily determined. The recurrence of the shower on a particular day of the year gives one point in the path of the meteors. The direction of the radiant gives a tangent to that path, and therefore its plane. The sun, of course, lies at the focus, and only a single further element—the periodic time—is requisite to complete our knowledge of the orbit. We are indebted to Prof. H. Newton, of Yale, for his careful discussion of this subject. He had shown that the choice of possible orbits was limited to five. There was first the great oval orbit, in which we now know the meteors do revolve every $33\frac{1}{4}$ years. There was next a nearly circular orbit, with a periodic time a little more than a year; another similar orbit, in which the periodic time would be a few days short of a year; and there were also two other smaller orbits. Prof. Newton had also indicated a method by which it would be possible to discriminate the true orbit as one of these five. The mathematical difficulties of this method were no doubt great, but they did not baffle Prof. Adams.

In the *Monthly Notices* for April 1867, p. 247, will be found the paper in which he announced his solution of the problem. The orbit of the meteors is not fixed, but every time the great swarm comes round, the node is found to be 29′ further on in the direction of motion. The effect of this is shown in the gradual alteration of the date of recurrence of the shower. The only influence known to us which could account for this change of the plane, is the attraction of the other planets. The problem, then, may be placed in this shape. A certain specific amount of change of the node takes place. The theoretical change can be computed for all the five different orbits, and Prof. Adams undertook to find it. The difficulty principally arises from the high eccentricities of some of the orbits, which rendered the more familiar methods of calculation inapplicable. After many months of labour, Prof. Adams, aided by his assistants in the Cambridge Observatory, completed his work. He showed that if the meteors revolved in the large orbit with the periodic time of $33\frac{1}{4}$ years, the perturbations of Jupiter would account for a change to the extent of 20′. The attraction of Saturn would augment this by 7′, and Uranus would add 1′, the effect of the earth and the other planets being insensible. The joint effect is thus 28′, which may be regarded as practically coincident with the observed value determined by Prof. Newton. The great orbit was thus a possible path for the meteors, but to complete his discovery Adams had to show that neither of the other four orbits could experience the same perturbation. This, too, he succeeded in demonstrating: he showed that in no one of the other orbits could the change exceed 12′. Thus the orbit of the Leonids was discovered.

Those tremendous powers of calculation which have been exercised on the heavenly bodies with such signal results have also been occasionally applied in various other directions. The discoverer of Neptune has found relaxation from the labours of physical astronomy by little calculations on which we must gaze with astonishment. He has had the curiosity to compute the sums of the reciprocals of the first thousand numbers to 260 places of decimals. We have such confidence in the accuracy of Prof. Adams that we have not thought it necessary to repeat this calculation! He has also taken the trouble to calculate thirty-one of Bernouilli's numbers beyond the point that previous calculators had attained, and he has expressed each of them both as vulgar fractions and as decimals. The sixty-second Bernouilli, the last computed by Adams, runs to 111 places, where fortunately for astronomy the appearance of a recurring figure has terminated this inquiry.

Need it be added that on Prof. Adams every honour which science can bestow has been conferred. We have now the pleasure of enriching our list of Scientific Worthies by the addition of his portrait. R. S. B.

THE BRITISH MUSEUM BIRDS

Catalogue of the Birds in the British Museum. Vol. XI. Fringilliformes: Part II. Containing the Families *Cærebidæ*, *Tanagridæ*, and *Icteridæ*. By Philip Lutley Sclater. (London: Printed by order of the Trustees. 1886.)

DR. GÜNTHER and the authorities of the Natural History Department of the British Museum are to be congratulated, first, on having sought, and next on having secured, the services of Mr. Sclater for the execution of the eleventh volume of their "Catalogue of Birds." The principal group of which it treats is one that has been, for five-and-thirty years or more, the

favourite subject of his study, and indeed there are people who hardly hesitate to assert that the final cause of the existence of the family *Tanagridæ* must have been to find occupation for that gentleman's ingenuity in discriminating, describing, and disposing its members.

In consequence of this the present volume of this "Catalogue" appears as a finished piece of work. Whatever may have been the faults of the plan on which the whole was designed, here its defects are reduced to a minimum, so that few are made manifest. The relationship of the *Cærebidæ* to the *Tanagridæ* is outwardly pretty close, and there is as yet no indication that future research into their inward structure is likely to separate them more widely, though in the existing state of ornithotomical study it would not be safe to say more. Granting, then, as Mr. Sclater considers, that the former of these families is so nearly allied to the latter that "it is indeed somewhat difficult to separate them by external characters," the comprehension of these two groups in one and the same volume is a very natural proceeding. As regards the third family, *Icteridæ*, associated with them on the present occasion, opinions may reasonably differ. Mr. Sclater confesses himself open to doubt on this point; and, if he does so, a slight amount of agnosticism may be pardoned in others who know not one-half so much about them as he does. Possibly if he had referred to some remarks of Prof. Parker's (*Trans. Zool. Soc.*, x., pp. 266, 267) on the relations of this group, the matter might have seemed to him a little clearer. Mr. Sclater's belief is that, since these birds "present many points of alliance with" the *Sturnidæ*, *i.e.* the true Starlings of the Old World, it would therefore be better "to place them *after* the *Fringillidæ*." Now the multitude of "points of alliance" is perhaps rather apparent than real, and though Prof. Parker considers (*loc. cit.*) that the *Icteridæ* and *Sturnidæ* cannot be "considered to be unrelated," he also shows that the former have nearer allies in their own part of the world. But this by the way. It is more important to inquire in what sense the word "*after*" is used by Mr. Sclater in the passage just quoted. We may be sure that he does not entertain a notion of the possibility of deploying any part even of the animal kingdom in a straight line, as was of old time thought not only possible but expedient; for such a notion would be completely at variance with the doctrine of evolution, which he of course holds. If the word "after" is merely intended to refer to the purely arbitrary arrangement followed, or to be followed, in the "Catalogue," it signifies nothing, and we have no cause to complain. Again, if "after" is to be understood in the sense of "inferior to," then we should wholly agree with him. But if the procession of forms be contrariwise arranged—there being no evidence in this volume to show whether this is so or not—and by the word "after" a later and consequently higher or more specialised type be indicated, then we should, with all deference, beg leave to demur to the supposition. Whatever be the rank and proper place among the true *Passeres* of the *Fringillidæ*—and Dr. Stejneger has lately propounded the view that theirs is the highest—the fact should always be remembered that the so-called *Fringillidæ* of many systematists certainly contain at least two groups, which in their more advanced stages can, so Prof. Parker tells us, be always discriminated. These two groups are the Finches proper and the Buntings, which last several taxonomers have recognised as forming a family, *Emberizidæ*, equal in value to the restricted *Fringillidæ*. Now Prof. Parker has shown that it is to the *Emberizidæ* rather than to these *Fringillidæ* that the *Icteridæ* are allied, and it therefore becomes important to determine the limits of the *Emberizidæ*, which, owing to the want of anatomical or morphological research, is admittedly hitherto a matter of guesswork. In regard to those members of the former which belong to the Old World, or are common to it and to the New World, no difficulty has as yet presented itself. It is in the New World alone that the doubtful forms exist; but even of some genera peculiarly American—*Phrygilus*, for example, as proved by Prof. Parker—indication is not wanting; and if we might hazard a supposition on the subject, it would seem on several grounds more likely that a closer alliance should be shown to exist between the *Icteridæ* and the *Emberizidæ* than between the latter and the true *Fringillidæ*.

From what has been submitted in the foregoing sentences, those who can "read between the lines" may perceive that underneath the points just touched upon is a question of much greater significance than is ordinarily presented by matters of mere taxonomy—especially of the taxonomy of a group of birds so homogeneous as are the *Passeres*. It is undeniable that the American forms of this multitudinous and confessedly highest group of birds (with the exception of those which, being so closely related to the Old-World forms may be not unreasonably supposed to be their derivatives) show a great preponderance of the weaker and, morphologically speaking, lower types. It is in the New World, and especially in South America, that we find all the *Tracheophonæ* and a majority of the different groups of *Oligomyodi*.[1] We can hardly doubt that these are as nearly autochthonous as any groups which now exist; that is to say, they had their origin on land which is now represented by the American continent. Though analogy is often a deceitful guide, it does not seem irrational to urge the same of the *Oscines*. Among them it is certain that not one of the three families which different systematists have selected for the post of leader is strongly represented in the New World—two of them, the *Corvidæ* and *Fringillidæ* (if we exclude the presumed *Emberizidæ*), very poorly indeed; while the remaining family, *Turdidæ*, cannot number, even at a very high estimate, one-third of its members as American. The meaning of these considerations will become plainer if we substitute for the expression "weaker and lower types" its justifiably equivalent rendering, that of "older and more generalised types." Then we shall see the important signification of the alliance we have supposed to exist between the *Icteridæ* and the *Emberizidæ*; and, moreover, a reasonable means of accounting for the remoter relationship, recognised by Mr. Sclater (Introd., p. viii.), between the *Tanagridæ* and the *Fringillidæ* on the one hand, and the *Cærebidæ* and *Mniotiltidæ* on the other, is provided. This result, we trust, will serve to excuse these remarks, which might otherwise appear to be irrelevant; and, speculative as

[1] To attempt here to account for the distribution of the non-American families of this group—*Pittidæ*, *Philepittidæ*, *Eurylæmidæ*, and *Acanthisittidæ*—would lead us far beyond the limits of our present subject.

they are, it must not for a moment be imagined that we think the introduction of anything like them was needed or would have been expedient in such a volume as that now under notice. A reviewer may take liberties that are denied to an author.

But now to return to our proper business. In his treatment of the subject Mr. Sclater, as might be expected, shows himself its master; not but that there are a few points—and one of them we have indicated—on which he owns himself doubtful. This is no drawback, for such there are, and long will be, in every matter of this kind. One great merit, in our eyes at least, is that he steadily treads the old high-road which has conducted its passengers to so many great achievements, and refuses to follow the bewildering by-paths, of late so much vaunted by various writers, that are eventually found to mislead the unwary or inexpert traveller into bottomless bogs. A plain view of things is taken, and one that is suggested no less by eminent knowledge than by common-sense. There is no attempt to regenerate a fallen world of science within the narrow limits of a catalogue, or to make the catalogue of a single museum, however great its wealth, pass for a monograph. From the beginning of the volume to its end there does not appear the trace of a wish to indulge—may we be pardoned the word?—a "fad"; not even in the frivolous matter of nomenclature, a rock on which all novices are sure to strike, and often to split.[1] The various "keys" with which every group is provided seem always to fit and to turn easily in their locks—not, indeed, a surprising fact, since Mr. Sclater, if not the inventor, has long been one of the most skilful handlers of this convenient differentiating instrument, so useful when manufactured by an adept, and so useless when turned out by a tiro, who not seldom contrives, in the course of a few lines, so to complicate his conditions (of his own choosing, be it remembered) as to render them characterless, if not contradictory. To sum up, it may be said that, supposing the plan of the British Museum "Catalogue of Birds" to have been well laid, Mr. Sclater has shown how it may be well executed.

THE VITAL STATISTICS OF GLASGOW

The Vital Statistics of the City of Glasgow. Part II. The Districts of Glasgow. By James B. Russell, M.D., LL.D., Medical Officer of Health.

IN this Report Dr. Russell presents us with the vital statistics of the city of Glasgow and its districts for 1880, 1881, and 1882, and with some comments on, and inferences to be drawn from, the facts enumerated. Some years ago an Improvement Trust scheme for the sanitary reformation of the houses of the people was elaborated and put into operation. This scheme has achieved "a summary revolution in the worst parts of the city," not apparently before it was wanted, for Dr. Russell shows that some of the districts of Glasgow—notably that known as Bridgegate and Wynds—do not compare favourably even with the worst slums of London or Liverpool. Bridgegate and Wynds had a death-rate for 1880, 1881, and 1882, of 38·3 per thousand, a birth-rate of 37·1 per 1000, a death-rate under one year per 1000 born of 206, and a death-rate from consumption and acute diseases of the lungs of 16·75—this figure alone being higher even than the total death-rate of many English towns. Much of this district has been improved off the face of the earth—the population in 1881 was 7798, in 1871 14,294—still the houses that are left "are radically bad, and total demolition and destruction is the only remedy." It is such districts as these that have, as Dr. Russell remarks, "been the heartbreak of successive generations of Glasgow philanthropists." The death-rate of the city of Glasgow, as a whole, for 1880, 1881, and 1882, was 25·2 per 1000, with a birth-rate of 37·3 per 1000; although considerably less healthy than London, Glasgow compares favourably with Dublin, and stands on about the same level as Liverpool and Manchester. The death-rates of the different districts of the city in 1871–72—prior to the improvement schemes—are compared with those in 1880, 1881, and 1882, subsequent to the carrying out of many improvements in unhealthy areas by demolition and reconstruction. The comparison shows that in all the districts the general death-rate and the death-rate under five years (with one exception) were much lower in the latter period than in the former. "This result," Dr. Russell remarks, "is important, as proving that the displacement of the inhabitants of the central parts of the city has not deteriorated the health of the districts into which they have removed. It was proved by special investigation that the people whose wretched houses were demolished by the Improvement Trust distributed themselves over the city. It is often said that the habits of these people are such that, go where they please, they will not be the better of the change. It is evident, however, that they found physical conditions so much more conducive to health that, whether or not their habits have been improved, undoubtedly their health has been, in their new residences. The moral is to persevere in the destruction or improvement of the houses of the people. The certain result is to improve their health."

The influence of overcrowding on mortality, and the connection subsisting between overcrowding and an Irish population are well shown in the contrast between two of the districts. Blythswood is remarkable as having the lowest proportion of inmates per inhabited room, the largest proportion of large-sized houses, the lowest death-rate, the lowest birth-rate, the lowest mortality under five years, the lowest proportion of deaths under one year per 1000 born, and the lowest proportion of Irish-born of any district in Glasgow. Bridgegate and Wynds, on the other hand, has the largest proportion of inmates per inhabited room, the largest proportion, save one, of one-apartment houses, the highest death-rate over all, the highest death-rate under five years, the largest proportion of deaths under one year per 1000 born, and the highest percentage of Irish-born inhabitants. And in general, Dr. Russell states it to be a fact "that a district which has houses occupied *above* the standard number of persons per room (*i.e.* above the mean number of the whole

[1] Indeed, we think we should have ground for complaining that Mr. Sclater has not made one nomenclatural change. He is of course as well aware as any one that the idea of a generic type never occurred to Linnæus, though it was ever present with Brisson. Nevertheless, modern specialists are required to find a type for every Linnæan genus; and, despite the almost universal practice, the type of *Tanagra* (which is only Brisson's *Tangara* with a modified spelling) is clearly *T. tatao*. Moreover, the generic term *Calliste*, used by Mr. Sclater for that species and its congeners, is but questionably admissible from its prior application by Poli ("Test. utr. Siciliæ," i. p. 30)—in a different dialectical form (*Callista*), it is true; but the Goddess of Wisdom may be called to witness that her name, whether written Athen*e* or Athen*a*, is one and the same; while we ask her pardon and that of our readers for drawing attention to such a trifle.

city), has a general death-rate *above* the standard death-rate of the city, an infantile death-rate *above* the standard infantile death-rate of the city, and especially that the fatality of those diseases which are directly related to overcrowding or deficiency of breathing-space—viz. diseases of the lungs and infectious diseases—is in excess in those districts."

Dr. Russell also discusses such subjects as the percentage of uncertified deaths, and the insurance of lives in friendly societies, the relations of legitimacy and illegitimacy to certification and insurance, and their bearings on the social conditions of poor populations—subjects of great interest to philanthropists and sanitary reformers, as indicating the instincts and habits of so large a mass of our poor populations. The rest of Dr. Russell's Report is of more purely local interest; but enough has been said to show that Glasgow, if it has been in want of sanitary reform, has not been behindhand in what may be described as one of the greatest works of the age, and that philanthropy in this case has met with its due reward in the vast improvements effected in the social condition of the people.

THE FRESH-WATER FISHES OF EUROPE

The Fresh-Water Fishes of Europe. A History of their Genera, Species, Structure, Habits, and Distribution. By H. G. Seeley, F.R.S., &c. With 214 Illustrations. 8vo. Pp. vi. and 444. (London, Paris, New York, and Melbourne: Cassell and Co., 1886.)

A WORK containing an original, exhaustive, and critical account of the fresh-water fishes of Europe, such as might bear the title heading this notice, would be an undertaking which would require on the part of the author a thorough acquaintance with ichthyology, considerable experience with the method of ichthyological research, an autoptical examination of many of the types preserved in the various European museums, and, finally, the formation of a collection more complete than the combined series of European fresh-water fishes in the museums of London, Paris, Vienna, Berlin, and St. Petersburg; in fact, an undertaking that would occupy the greater portion of a life-time, and stand as a monument of which any naturalist might be proud.

We have too high an opinion of Prof. Seeley's abilities to doubt for a moment that he might have produced a standard work of this nature, if he had chosen to devote the requisite time and labour to it. But what he has really accomplished is merely a compilation from the standard works mentioned in his preface, without the addition of any new facts or observations, and without any attempt at such a critical treatment of the subject as might be expected from an author acquainted with the objects described. His book, in fact, might have been compiled in the author's own library or in that of the British Museum without his looking at a single fish. The illustrations are no less wanting in originality; with the exception of half-a-dozen anatomical figures familiar to every ichthyologist, the remaining 208 are simply borrowed from Heckel and Kner, "Süsswasserfische der östreichischen Monarchie"; and consequently no fish peculiar to any other part of Europe or absent from the Austrian fauna is represented in the book. It should be remembered, however, that at the date of their publication (1858) the two Austrian ichthyologists above named were enabled to include in their fish-fauna a number of the species of Northern Italy. We think that the source whence the illustrations were taken should have been stated in the preface.

As regards the usefulness of the book, there cannot be any doubt that a handy book on the fresh-water fishes of Europe was a great desideratum. A glance at the natural-history columns of the *Field*, *Land and Water*, and other weekly papers shows the great number of travellers on the Continent who seek for information about fresh-water fishes which are strange to them, and to whom the original works wherein they could find it are either unknown or unintelligible. For this large class of the non-scientific public Prof. Seeley has supplied a real want and a useful book of reference, the utility of which would have been much greater could he have induced his publishers to go to the expense of figuring other fishes besides those found in Austria; and we cordially join him in the hope "that the fabric of the work will give a new interest to the fishes of our own country, and may influence British peoples to a thrifty cultivation of the roving wealth which swims, little heeded, in our forms of fresh-water fish life."

OUR BOOK SHELF

Papers in Inorganic Chemistry. Part I. Non-Metals. Part II. Non-Metals and Metals. By George E. R. Ellis, F.C.S. (London: Rivingtons, 1886.)

THIS is a collection of examination questions arranged progressively, and is intended for the use of science teachers and students. The idea is a good one, and we have no hesitation in saying that the book will be appreciated by those for whose benefit it has been compiled. Although we are far from approving of the present mania for examinations, we agree with the author that the conscientious answering of well-selected questions is of great advantage to the student. It not only tests his knowledge gained from text-books and from lectures, but it renders it more accurate and permanent.

The solution of chemical problems is generally a weak point with beginners, and we are glad to see a fair proportion of such problems in Mr. Ellis's book. There are, however, a few arithmetical questions which appear a little out of place in papers in inorganic chemistry. On p. 6, for instance, there is one on the tonnage of the s.s. *Oregon*, and others may be found on pp. 10, 12, 30, &c.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Our Guns

YOUR article on the above (p. 517) induces me to repeat an appeal which I made ten years ago in a paper on "Explosive Compounds," contributed to Stanford's "British Manufacturing Industries." I there pointed out the enormous discrepancy between the results obtained in the testing of the pressure exerted by the explosion of gunpowder by the Government Committee on Explosives and those of Count Rumford made in 1793, and described in his essay on "The Force of

Fired Gunpowder" (published in London, 1802, and recently reprinted, with his other essays, in America).

Our Government official experiments give a pressure per square inch varying from 15·4 to 28·1 tons per square inch, the latter obtained in the 81-ton gun. Rumford's maximum was 277 tons, as shown by overcoming the tested tenacity of metal that the powder tore asunder; or 73 tons, as indicated by the lifting of a weight by the explosive energy.

As I explained in the paper above named, the tearing test is fallacious as a theoretical measure of the force exerted, because the tenacity of the metal was tested by Rumford, as it still is by others, in most cases, by a gradually-applied strain, which should not be compared with a vibratory shock. As a measure of the practical bursting possibilities of gunpowder upon metal of given thickness and tenacity, as usually measured, Rumford's figures are directly applicable, though allowance must be made for the relief afforded by the movement of the shot in a gun.

My appeal was for a repetition of Rumford's experiments by those who are responsible to the nation for these very serious matters, and for a reconsideration of the reliability of the method of testing by the "Rodman" and "Crusher" gauges, which have supplied such very different results from those of Rumford. My reasons for believing Rumford's experiments to be more reliable than those of the Committee were stated as follows, and I still maintain their cogency:—

"(1) The resistance to be overcome, and by which the force was measured, viz. the gravitation of a known weight, was by far more definite and measurable than the degree of indentation or compression of a cylinder of copper, which serves as the measure of force in the Rodman and Crusher gauges.

"(2) In Rumford's arrangement the force of the explosion was more directly applied to the resistance by which it was measured than in the official experiments, where the shock of the explosion was first communicated to a solid piston 1 inch in length, and by this transferred to the copper cylinder of the Crusher gauge or the knife of the Rodman gauge. By this arrangement much of the force is expended upon internal work in the intervening piston, producing mechanical vibration of its substance, and a returning wave of elastic compression, which would have no measurable effect on the gauge. Besides this, another portion of the force compressing the piston must be converted from mechanical motion into heat motion.

"If any reader supposes that I am hypercritical in making this objection, let him try the following experiment. Take a block of iron—a common 1 lb. weight, for example—place it on the hand, and the hand upon a table; then strike the weight smartly with a carpenter's hammer. It will be found that blows which would fearfully mutilate the hand if directly applied to it, may be struck upon the weight thus resting entirely upon the hand, and will scarcely be felt, provided the blows are dealt suddenly and smartly. The mountebank's exploit of breaking a great stone upon a man's bare breast, the common method of reducing the dimensions of geological specimens by holding them in the hand and cracking with a hammer, and the experiment of shooting a bullet through a swinging door without moving it on its hinges, are familiar illustrations of this principle, which appears to have been overlooked in these official researches.

"The complete absence of windage in Rumford's arrangement, by exploding in a perfectly closed chamber, is a third advantage. I therefore regard Rumford's experiments as the best that have yet been made on this interesting subject, although, as he himself admits, they are by no means free from error."

W. MATTIEU WILLIAMS

Photographs of Stellar Spectra

THE article upon this subject in NATURE, vol. xxxiv. p. 439, requires a correction which has been pointed out by Dr. Copeland. The spectrum of the star DM. + 37° 3821 was observed by him on September 22, 1884, and found to contain bright lines; the observation was published in the *Monthly Notices* for December 1884, but was overlooked at the time when the article above mentioned was prepared.

A similar correction, pointed out by Dr. Huggins, is required in the "Investigation in Stellar Photography" by the present writer, published in vol. xi. of the "Memoirs of the American Academy of Arts and Sciences." On p. 208 the method of observing stellar spectra by means of a prism placed before the object-glass of a telescope is ascribed to Secchi. In fact, it had previously been employed by Fraunhofer.

EDWARD C. PICKERING

The Late American Earthquake and its Limits

IN your very flattering *critique* of my "Alphabetical Catalogue of European Earthquakes" the reviewer says:—"The tendency to alignment in volcanoes has often been noticed; Prof. O'Reilly indicates a similar peculiarity in earthquakes, adding that the lines along which they range approximate to great circles. This inference or suspicion can be verified only by detailed charting." Judging from the facts published up to the present relative to recent earthquakes of America and Europe, I think some such verification has been furnished by them. At the Exhibition of Scientific Apparatus held at South Kensington in 1877, I exhibited a globe mounted so as to allow of great circles being easily traced through points on the surface. Several coast-line great circles were shown thereon, amongst them that of the southern boundary of the Tertiary formation in the United States. It was also marked on the sketch earthquake-map of Europe exhibited before the Section of Geology of the British Association at their Swansea meeting of 1882, and on other maps, such as the earthquake map of the British Islands; and yet no leading fact went to prove that any particular significance should be attached to this great circle. The earthquakes of August 27 and 28 in the United States have furnished, in my opinion, some proofs of this significance. The following are the places through which this great circle passes:—Victoria Fort, on coast of Gulf of Mexico; Cairo (Ill.); axis of Lake Erie; Lake Ontario; River St. Lawrence (parallel to); New Brunswick coast of River St. Lawrence; Labrador, south coast; York Point and Straits of Belle Isle; Ireland, Shannon mouth; Wales, south coast of; St. Bride's Bay; Mendip Hills; Southampton; Dieppe, north of; Chalons; Bâsle, north-east coast of Zurich Lake; Coire; Trent; Venice; Dalmatian coast; south-west coast of Isola Longa; Mount Olympus; Skyro Island; Syrian coast, head of Akaba Gulf; Arabia, Mount Seiban, Wady Maifa; Cape Guardafui; Pacific Ocean, Paumota Group; coast of Mexico, near Cape Corrientes; Zacatecas territory.

According to Major Powell's telegram, the origin of the earthquake was along a line of post-Quaternary dislocations on the eastern flank of the Appalachian Chain, especially where it crosses North Carolina. The great circle just described passes more inland than that mentioned by Major Powell, and was taken, as regards position, from the geological map of the United States, by C. H. Hitchcock and W. P. Blake, 1873, but it is parallel to the line limiting the Tertiary formation which crosses North Carolina, and which is probably also the seat of the post-Quaternary disturbance referred to. The great circle in question traverses the area of disturbance between Kennett (Ark.) and Buffalo (on Lake Erie). On the European side the following places lie near its direction. The Bristol coal-fields, where an explosion of fire-damp took place lately, about the time of the earthquake; the English Channel, lat. N. 50° 10′ and long. W. 1° 40′, where an earthquake shock is reported to have occurred by H. Mohn in your issue of September 23 (p. 496); the point lying about fifty-five miles to the south of the great circle, where it passes at Southampton. Switzerland: M. Forel reports in your journal of the 16th ult. (p. 469) a series of shocks in the western part of Switzerland having occurred in the first days of September, and which he considers as the *suite* of the earthquake of August 27. In Eastern Europe an earthquake occurred on this same date, which travelled eastward from Malta to the South of Italy and reached Smyrna, which lies somewhat to the north of the great circle. In Mexico an earthquake is reported as having occurred at Tequisextlan on the 3rd ult. I can find no such place, but if it be the same as Tepantitlan, about fifty miles east-north-east of Guadalajara, it would be somewhat south of the great circle in question. As all these places are not far removed from the direction of the great circle, and as there must be several parallel lines of fissuring in the Appalachian Chain, thus forming a zone, there is in this way, I think, evidence furnished that a zone of seismic action exists, having the general direction of the great circle represented by the continuation of the boundary-line of the Tertiary formation in the United States to the west of the Mississippi Valley, as marked on the geological map of Messrs. Hitchcock and Blake.

In the map forwarded herewith I have defined the surface of disturbance by lines joining the extreme points mentioned as having suffered shocks; but further information may, and probably will, modify this outline. The polygonal form thus obtained is, I think, more satisfactory than the curved forms

usually adopted, since there are some grounds for thinking that the limits of disturbance are generally lines of faulting or limits of formations. In the first report received, the extreme points mentioned to the west were Jacksonville and Chicago, both situated on a line which represents the axis of the promontory of Florida, as shown by the map. The great mass of the surface shaken lies to the east of this line, so that this surface would be bounded by coast-line directions, at least partially. When fuller information comes to hand, I trust to be able to show more perfect results in this respect. J. P. O'REILLY

Algebraic Notation of Kinship

MR. F. GALTON has described two systems of kinship notation: one in his work on "Hereditary Genius," pp. 50 53; the other in a letter to NATURE, vol. xxviii. p. 435. I propose to indicate here the outline of another system, which, though slightly more cumbrous in form than Mr. Galton's, seems to me to possess some advantages of its own.

Let us denote the male members of a family by a capital letter, say F, females by a small letter, *f*. Taking the person described as a starting-point, relatives in different generations may be denoted by *indices* attached to these letters. For relatives in the same generation, the index is 0; for those in the first, second, &c., generation before him, the indices are 1, 2, &c.; for those in generations following him they are −1, −2, &c. Thus, a brother would be denoted by the symbol F^0, a sister by f^0; the father by F^1 and mother by f^1; a son by F^{-1} and a daughter by f^{-1}. Again, the father's father would be denoted by $F^1 F^1$, or more shortly by F^2; the father's mother by $F^1 f^1$; a son's son by F^{-2}, a daughter's son by $f^{-1} F^{-1}$; the father's brother by $F^1 F^0$ (or perhaps by F^{10}, if it be remembered that here 10 stands for 1 + 0, and not for ten, which is too large a number to be often required); the mother's brother by $f^1 F^0$; the father's sister's son by $F^1 f^0 F^{-1}$, and her daughter by $F^1 f^0 f^{-1}$; and so on. The advantages of the system are: (1) that it is readily used and interpreted; (2) that the generation of any relation with respect to the person described may be found at once by adding the indices of his kinship symbol. For example, in the last two instances given, the sum of the indices is 0, showing that in both cases the generation is the same as that of the person described.

The simpler relationships are shown in the following table:—

F^2 Grandfather = $F^1 f^1$ Grandmother | $f^1 F^1$ Grandfather = f^2 Grandmother

$F^1 F^0$ Uncle | $F^1 f^0$ Aunt | F^1 Father = f^1 Mother | $f^1 F^0$ Uncle | $f^1 f^0$ Aunt

F^0 Brother | PERSON DESCRIBED | f^0 Sister

$F^0 F^{-1}$ Nephew | $F^0 f^{-1}$ Niece | F^{-1} Son | f^{-1} Daughter | $f^0 F^{-1}$ Nephew | $f^0 f^{-1}$ Niece

F^{-2} Grandson | $F^{-1} f^{-1}$ Granddaughter | $f^{-1} F^{-1}$ Grandson | f^{-2} Granddaughter

If desired, suffixes to the letters might be added to denote the position in his family of any person noted in the kinship symbol. The symbol $F_3^{-1} f_5^{-1}$ would, for example, denote a son's daughter, the son being the third member of his family, and his daughter the fifth. CHARLES DAVISON

Birmingham, September 30

Physiological Selection

AS Dr. Romanes has had his attention drawn to my letter (NATURE, vol. xxxi. p. 4), he may be interested in knowing just how far his theory of physiological selection was anticipated by what was in my mind.

My idea was that a spontaneous variation might occur in the sexual elements of the offspring of one parent or pair which would leave them fertile with each other, while rendering them almost or quite infertile with the rest of the species, so that the family would be physiologically insular.

Though I did not definitely so limit it, it did not seem to me likely that a corresponding change in both sexes, which was what I had in my mind, would occur, except in the offspring of the same parent or pair; and I rejected the idea of a gradually increasing infertility in favour of a total or nearly total infertility arising in the one generation, because I did not see any cause for the continuous increase from generation to generation of such infertility.

It did not occur to me that a partial infertility would, in a number of generations, produce the same result, as pointed out by Dr. Romanes (NATURE, August 5, p. 316). Nor do I now see clearly that it would ever lead to the total infertility which exists between species which have not otherwise diverged very much. At present, however, I only wish to point out that the idea of gradually increasing infertility was in no way anticipated by me. EDMUND CATCHPOOL

Friends' Institute, 13, Bishopsgate Street Without, London, E.C., October 9

American Vines

IN reading Prof. Carruthers' very interesting address to the Biological Section of the British Association, I observe that he says, when speaking of the vine discovered by Dr. Schweinfurth at Abd-el-Qurna: "The leaves which have been obtained entire exactly agree in form with those cultivated at the present day, but the under-surface is clothed with white hairs, a peculiarity which Dr. Schweinfurth has not observed in any Egyptian vines of our time." Will you allow me to remark that this is a character of several of our American vines? Both the Northern Fox and the Summer Grape (*V. Labrusca* and *V. æstivalis*) are conspicuously downy on the lower surface of the leaves—so much so that they appear white.

This fact adds another to the list of points in which the old flora of the eastern hemisphere resembles that now existing on the western side of the Atlantic. But the resemblance in this case is of much more recent date than those with which we are so familiar from the researches of Prof. Heer among the Oeningen beds of Switzerland.

In connection with the facts above mentioned, it would be of interest to ascertain if an opportunity should ever occur whether the other noteworthy differences between the American vines generally and the European vine, namely, the musky or foxy flavour and the soft and pulpy, not firm and fleshy, berry were accompaniments of the downy leaf. This may never be possible, but it would serve to show which of the two was the older stem from which the other has diverged. E. W. CLAYPOLE

Akron, O., U.S., September 30

"Scopelus mülleri"

A FEW weeks since I received a letter from Mr. Southwell, of Norwich, wherein he informed me that Capt. Gray had sent him a very interesting fish, which he kindly forwarded to Cheltenham. He captured it on August 1, 1886, in lat. 73° 12′ N., and long. 14° 28′ W. Capt. Gray remarked:—"It was at the surface; I noticed it while away in a boat as I leant over the bow and watched the water as it passed. It was covered with bright golden scales when I first found it, but they are nearly all rubbed off. It was alive when I picked it up, and the temperature of the water was 35° F., and that of the air 35° also."

The specimen is in comparatively a good state of preservation; the tail, however, has been broken, reducing its total length to 2 inches; from the snout to the base of the caudal fin, 1·8 inch.

D. 14, P. 11, V. 8, A. 16, C. 19, L.l. 36.

Its proportions are normal, agreeing with Collett's description, the origin of the dorsal fin commencing exactly midway between the end of the snout and the base of the caudal fin. As good examples appear to be very rare, I propose remarking on certain appearances which have been in dispute. The eye-like spots are thus disposed: along the edge of the abdomen, between the throat and base of the ventral fins, 5 pairs; between the ventral and anal, 3 pairs; along the base of the anal, 8 pairs; beyond the anal, 6 pairs. Krüger gave a similar number between the ventral and anal to what exists in this specimen, but Collett found 4 pairs there. Between the 6th and 7th pairs along the base of the anal fin, and between the 5th and 6th pairs behind that fin, there is a wider space than between the others. Along the side and below the posterior half of the lateral line are two more of these eye-like spots, while a row of three passes from

the anterior of these to the subopercle. Another spot exists at the shoulder above the gill-opening, and an indistinct one on the edge of the preopercle.

The figure in the magnificent work of the "Fishery Industries of the United States," plate 202, agrees with this specimen in the proportions of the body and length of the fin-rays; but, supposing the species to be the same, differs from it materially in some other points. The position of the dorsal fin is shown too far forward; the scaling is not identical, as the lateral line in Capt. Gray's specimen stands upon a narrow but vertically elongated row of smooth scales, having three rows of smaller ones above it, and four below it; also the eye-like spots are not similarly placed, and the pectoral fin is narrower, with its rays more elongated. FRANCIS DAY

Cheltenham, October 1

The Sense of Smell

WITH reference to Mr. Mitchell's inquiries (NATURE, September 30, p. 521), there is a peculiarity about musk which I have never found anybody to be previously aware of, namely, it is impossible to smell it *twice*, taking two good "sniffs" consecutively at a plant, *i.e.* after a single exspiration; on the second inspiration there is no odour of musk whatever.

GEORGE HENSLOW

Humming in the Air caused by Insects

YOUR correspondent who writes on the above subject in this week's number of NATURE (p. 547) remarks that "It is singular that no explanation has been offered by any one for such a common phenomenon." May I be allowed to refer him to my "Observations in Natural History" (published in 1846), p. 226, where I have given a statement of my own on the subject, adding a reference to Humboldt's "Personal Narrative," in which he makes some remarks on this humming, as heard in the tropical regions, where the phenomena is naturally so much more striking, and on a wider scale.

There can be no doubt the explanation of the phenomenon given by your correspondent is the correct one.

Bath, October 9 LEONARD BLOMEFIELD

THE HONG KONG OBSERVATORY

DR. W. DOBERCK, Government Astronomer at the above Observatory, has recently published an official report on the astronomical instruments under his charge, and on the time service of Hong Kong in 1885, the determination of local time being the chief purpose of the astronomical branch of the institution.

The Report states that the Observatory possesses a transit instrument, by Messrs. Troughton and Simms, of 3 inches aperture and 3 feet focal length. Setting in declination is effected by means of two small circles fixed on the telescope near the eye-end, and read by levels. The axis is perforated for side lamps. The pivots, which are made of chilled bell-metal, show no perceptible difference between their diameters, but minute irregularities appear to exist, though too small to allow their exact amount to be determined by means of the axis level This level is used in determining the inclination of the axis, and another similar level is provided for use with the zenith micrometer in the observation of differences of zenith distances on either side of the zenith.

The eyepiece was originally furnished with one movable and seven fixed vertical wires, but the latter after a little while began to get entangled with the fixed wires, and finally broke. Although it had been found very useful in the determination of the instrumental constants, it was not thought well to replace it for fear lest the permanent wires might become disturbed or broken by it. Transits were at first observed over all the seven wires, but in 1886 only the five middle wires have been used. There are also two horizontal wires about a minute of arc apart, and the object the transit of which is to be observed, is brought midway between them. The eyepiece and wire system can be revolved through a quarter of a revolution, so that the vertical wires become horizontal, and can be used for determining the differences of zenith distance for latitude; but as the instrument is in constant use as a transit, this arrangement has never been made use of, there being the less necessity for employing it, as Col. Palmer had accurately determined the latitude of the Observatory in 1882.

The telescope rests upon a cast-iron stand with reversing apparatus; no change in the inclination has ever been perceived to be caused by the reversion. The stand rests on a slab of Portland stone on the top of a brick pier sunk 5 feet in the ground, where it is surrounded by a brick cylinder to protect it from surface oscillations.

In 1884 505 transits were observed; in 1885, 313; the inclination of the axis was observed 150 times in the former and 117 in the latter year. A meridian mark, which is viewed through an object-glass of about 66 feet focal length is placed about 70 feet to the north of the transit instrument; another meridian mark is 11,354 feet to the south across the harbour.

The standard sidereal and mean time clocks were supplied by Messrs. E. Dent and Co. The former has a cast-iron back which is firmly screwed to iron bolts cemented in the pier placed in the clock-room. The pendulum has the zinc and steel compensations originally designed for the transit of Venus expeditions. The clock was also supplied with a galvanic contact apparatus omitting one second each minute, for working a sympathetic dial in the transit-room, but as the contact-apparatus was found to interfere with the going of the standard-clock, its use was discontinued early in 1885, and the observations have since been made with a chronometer which is subsequently compared with the standard-clock.

The mean daily rates during ten-day periods of the standard clock are given in a table, and from the rates between January 1 and June 9 the following formula for the rate at t degrees Fahrenheit is deduced:—

$$r = + 1s.\cdot247 - 0s.\cdot033 (t - 70^\circ).$$

The clock stopped twice during the year, viz. on June 12 and August 23, each time during a thunderstorm. A difficulty was experienced in the attempt to determine separately the barometric coefficient, since the mean height of the barometer in Hong Kong falls regularly as the mean temperature rises, but it appeared to be insensible.

It should be noted, however, that the mean rates which Dr. Doberck publishes here suggest that the formula given above is only correct within certain limits. The clock would appear to be compensated for 80° or 85°; at least there seems to be no regular variation in its rate about these temperatures, the decline in the rate which accompanies the rise of temperature up to 80° showing a check or even a slight reversal about that point. Probably, however, the mean daily rates cannot be trusted to the degree of refinement to which they are here given. The number of transits observed is decidedly small, and the errors of the transit-instrument do not seem to have been very frequently or regularly determined during 1885. No information is given as to the degree of accordance of the daily rates actually observed.

The mean time clock is similar to the sidereal standard clock, and appears to go as well. It is furnished with galvanic contact springs, which are pressed every hour at the exact second, and send a current through a reversing commutator worked by one Léclanché cell, by means of which the current that drops the time ball at 1 p.m. is closed.

Some trouble has been experienced with the time ball. First the galvanic coil in the mean-time clock-case for setting the clock right before the ball is dropped, is not strong enough, as it takes nearly an hour and a half to correct an error of a second. The lock sent out with the

time ball, also, was not fit to drop the ball, the blow of the hammer failing to discharge the bolt, so that the hammer had also to be struck at the instant the current arrived from the clock. The coil was fused by lightning on June 12, and from that date until November 20, the ball was dropped by hand. Since November 22 a new lock has been fitted, which gives satisfaction.

The time-ball tower is erected on Tsim-sha-tsui Point, directly facing the shipping. It stands in front of the new police-station beside the mast for hoisting meteorological signals, at the foot of which the typhoon gun, pointed towards the city opposite, is placed. In the police boat-basin, at a short distance north-west of the tower, the small observatory is built. The time-ball tower is about half a mile distant from the observatory, with which it is connected by wire. The base of the tower is about 40 feet above sea-level, and the top of the ball-mast about 84 feet. On the ground-floor is a massive granite pier, which supports the entire apparatus. Dr. Doberck describes at considerable length the arrangements for dropping the ball, for breaking its fall, and for ascertaining that the fall has taken place at the proper instant, but they do not call for special remark. The accuracy of the time-ball signal at 1 o'clock depends, he says, practically upon the error of the standard clock being accurately determined. If the weather has permitted transit observations to be made the previous evening, the error of course will be pretty closely known; otherwise the regularity of the rate of the standard clock must be trusted to. A table of the errors of the time-ball in 1885 is given, showing that the mean probable error of the signal for each month is about 0·2s.

The time service is at present confined to the dropping of this ball, but there would be, Dr. Doberck points out, no difficulty in dropping any number of time-balls along the coast or distributing hourly time-signals to the principal public buildings, &c.

The observatory possesses three chronometers, the rates of which are here given. Of the three, two keep mean-time, and one keeps sidereal time, but one of the former is useful only as a hack watch.

The equatorial of the observatory is the Lee equatorial, described by Admiral Smyth in the "Speculum Hartwellianum" and the "Celestial Cycle." The great length of the polar axis renders the instrument unsteady and sensitive to every motion of the observer. A Maclean star spectroscope has been fitted to it. The object-glass appears to be still in good condition.

The meteor shower (the Andromedes) of November 27 was observed, and it was estimated that about 2000 meteors an hour were visible, most being small; none being brighter than the first magnitude, and only a few so bright. The radiant was determined to be at R.A. 27°, Decl. 40° N., but it was at least 3° in diameter.

THE RAINFALL OF THE CAPE COLONY

OBSERVATIONS of rainfall were begun in the colony about forty-five years ago, but until 1876 no general system of registration was in force; and, except in the case of the Royal Observatory and a few other stations, no continuous records were available. In 1876, however, Mr. John G. Gamble, M.A., M.Inst.C.E., the Hydraulic Engineer to the Colony, induced the Government to grant a sum of 100*l*. for the purpose of establishing rain-gauges throughout the country. This grant has been continued yearly since then, with the result that, although the sum is quite inadequate for the speedy erection of the number required, there are now 250 gauges from which monthly returns are obtained. A gauge is placed at every seat of magistracy, and private persons are also supplied with gauges free of charge on condition that they observe continuously for five years, and forward monthly returns to the Meteorological Commission at Cape Town. All the services rendered by the observers are gratuitous. The monthly returns are tabulated and printed together with other meteorological observations in a report by the Meteorological Commission, which is presented annually to the Colonial Legislature.

At the end of 1883 there were 75 stations at which records had been kept for at least five years. An appendix showing the average rainfall for each month of the year at these stations was published by the Meteorological Commission in their 1883 report, and in their report of the following year some diagrams plotted from these averages were included, which show more strikingly than figures the fluctuation of the rainfall from month to month.

That the observations begun ten years ago are beginning to bear good fruit is evident from the series of rainfall maps exhibited in the Court devoted to the Cape of Good Hope at the Colonial and Indian Exhibition. There are sixteen maps altogether, fourteen of which have been compiled by Mr. Gamble and two by Mr. W. B. Tripp, F.R.Met.Soc. One of those by Mr. Gamble shows the position of the gauges and the districts into which the colony has been divided for the purpose of rainfall registration; the others represent, by means of different colours, the general distribution of rainfall for each month of the year and for the whole year. The contours on the maps for the various months show differences of 1 inch in the rainfall, starting from a contour indicating the area where the fall is less than 0·5 inch. The contours on the map for the year indicate differences of 12 inches, beginning at 6 inches, and going up to 54 inches. The number of inches of rainfall at the various places is marked in figures.

Mr. Tripp's maps are intended to show the relation between the physical configuration of the country and its rainfall—one map representing contour-levels every 1000 feet up to 4000 feet, and the other representing the mean annual rainfall.

A casual examination of the various maps is sufficient to show that the conditions which determine the rainfall are not the same for the whole of South Africa. Thus in the south-west district of the Western Province the chief portion of the rain falls in the winter months, while in the Eastern Province, and in Natal and the Orange Free State, the greater portion falls in the summer, from October to March. On the southern seaboard of the Cape Colony the rainfall is irregularly distributed throughout the year, the greatest monthly fall at any place varying from one-ninth to one-eighth of the total.

A glance at Mr. Gamble's map showing the distribution in the year, shows that the north-west part of the colony is almost rainless. With the exception of the tract occupied by the Namaqualand Mountains, the average yearly fall in this desert is less than 6 inches; at Pella, a village on the Orange River, the rainfall for the year is 2½ inches, one-fifth of which falls in May. Some of the months at this place are rainless. Throughout the greater part of the colony the yearly rainfall varies from 6 to 18 inches, the smaller falls being characteristic of the regions in the interior, generally known as "The Karroo," from 2000 to 4000 feet above sea-level, and of a plateau nature; while the greater falls are found nearer the sea, and in the mountainous parts. In the south-west district, excluding the Cape Peninsula, and on the narrow strip of country on the south coast, between Swellendam and Port Elizabeth, the yearly rainfall ranges from 18 to 30 inches, except in the forests of George and Knysna, where it exceeds 40 inches. In the Cape Peninsula the rainfall varies from 25 inches at the Royal Observatory to 54 inches on the south-east side of Table Mountain. In the eastern districts of the colony, and in the neighbouring territories, where the main portion of the rain comes in summer, the fall averages from 18 to 30 inches,

with somewhat higher records from the Amatola forest region. There are doubtless many places in this great tract, notably in the Drackensberg, where the rainfall exceeds that shown on the maps, but there are no records to admit of the area being mapped.

With regard to the causes which determine the variation in the rainfall, Mr. Gamble has pointed out that this is due chiefly to the sea-currents and the prevailing winds. "Natal and the Eastern Province (of the Cape Colony) get their rains chiefly in summer, when the south-east trade wind blows," while "the western portion of the Western Province gets its rain mainly in winter with the north-west wind" (*Trans.* Philos. Society of South Africa). From whatever direction the rain may come, it seldom penetrates beyond the mountain-range which runs parallel to the coast. This barrier, as shown by Mr. Tripp's contour map, separates the elevated central plateau from the tract of lower country on the coast. During some of the summer months it would appear as if the south-east clouds were carried over parts of this barrier, but generally all the rainfall in the midland districts of the colony comes from thunder-showers of a very local character, heavy rain falling on one farm, while it is dry all round. It might be asked why, if the western portion of the Western Province gets its rain with a north-west wind, should the midlands not get their supply from the same source. To this Mr. Gamble answers: "We may note that the anti-trade of the Western Province comes apparently from a portion of the equatorial regions that is occupied by sea, while the north-west winds that blow in the Eastern Province, if they really keep their theoretical curvilinear path and are not interfered with by the height and temperature of the ground, come from a portion of the equator where there is land and consequently small evaporation."

As might be expected, the areas occupied by given rainfalls alter in position and size from month to month, but these fluctuations cannot well be described without the aid of maps. It may be noted, however, that in May, which marks the beginning of the wet season at Cape Town, the area over which the rainfall varies from 0·5 to 1·5 inch occupies nearly the whole of the colony. The maps for October to March are extremely interesting, as marking the gradual advance of the rains which come from the south-east. Concerning droughts and floods, Mr. Gamble has made the pregnant remark:—"It is frequently said that in such and such a year there was a drought in the colony; in another year, heavy floods in the colony. This way of speaking is incorrect, for, in consequence of the very distinct climates of the east and and west respectively, it is very rare that a drought occurs all over South Africa at the same time."

As the future prosperity of South Africa depends on irrigation, it is almost needless to point out the importance of the work done by Mr. Gamble. Considering the small outlay which has been incurred, the results are remarkable. Whether the work will in future be carried on in the same scientific spirit as it has been hitherto is somewhat doubtful in view of the backward tendency at present in course of development in the colony.

THOMAS STEWART

FERDINAND STOLICZKA

IN an interesting memoir, published by order of the Government of India, Mr. V. Ball gives a sketch of the life and work of Dr. F. Stoliczka, for many years Palæontologist to the Geological Survey of India. This memoir appears in connection with the publication of the scientific results of the second Yarkand mission, of which Stoliczka was Naturalist, and during the return journey of which he met his untimely end.

Born at Hochwald, in Moravia, in May 1838, Stoliczka obtained his early education at Prague, from whence he proceeded to Vienna, where he took the degree of a Doctor of Philosophy. To Prof. Suess he was indebted for his first regular training in geology, and he received the kindliest help in palæontology from Dr. Hörnes, who was for some years Director of the Austrian Imperial Mineralogical Cabinet, and was well known by his researches on the Mollusca of the Vienna Tertiary. He died in the prime of life, but not before he had seen the firstfruits of Stoliczka's labours on the Cretaceous fossils of India. Stoliczka's first contribution to science was made (1859) to the Vienna Academy of Science as a memoir on some fresh-water Mollusca from the Cretaceous formation of the North-Eastern Alps, and in 1861 he became one of the staff of the Austrian Geological Survey, of which Dr. Haidinger was then the chief. Here he had the fullest opportunities of working at his favourite pursuit, and well does he seem to have availed himself of them. There was a conscientious accuracy as well as an extensive knowledge of his subject displayed in Stoliczka's writings of this period that early marked him out for a brilliant career.

In the year 1862 he received the appointment of Assistant to the Geological Survey of India, and was present with Dr. Oldham, the Superintendent of the Survey, at the meeting of the British Association at Cambridge over which Prof. Huxley presided. There are many who may still remember his slight figure, and dark hair brushed back: in after days he became rather stout. At that time he knew but a few words of English, but very shortly afterwards we find him not only speaking and understanding English well, but actually writing notes in his journal in English.

On his arrival at Calcutta he at once commenced to work on the Cretaceous fossils of Southern India, and the splendid series of memoirs on these forms, of which Part I appeared in 1863, was not completed until ten years afterwards. These memoirs, in which as to the Belemnites and Nautilus he was assisted by Mr. Blanford, form a work of over 1400 pages, illustrated by 176 plates, a record in itself of a laborious life. The work of arranging and describing the fossils collected by others was, however, only a small portion of the work performed by Stoliczka. He threw himself with ardour into everything that pertained to the natural history of his adopted country, and there was scarcely a division of the animal kingdom that he had not a tolerable acquaintance with, and to the published records of which he did not add something—Mammalia, birds, reptiles, mollusks, Polyzoa, arachnids, Crustacea.

From time to time his work took him from the Museum workshops, and he visited now the North-Western Himalayas, and again the Andaman Islands, and portions of Burmah. In all and every place he visited he found something new and interesting, and by the numerous papers which he published as the result of his travels, one might almost follow him in his journeyings.

In 1873 it had been arranged that Stoliczka should go to Europe to take charge in part of the splendid collection of minerals and fossils sent to the Great Exhibition of Vienna from the Geological Survey of India, but he was tempted to go instead as one of the mission from the Government of India to the King of Yarkand and Kashgar. On May 17 he left Calcutta on a journey from which he never returned. Yarkand was reached on November 8. Early in October, and shortly before crossing the Sanju Pass (16,500 feet high), Stoliczka had been seriously ill from apparently a slight attack of spinal meningitis, from which, however, he rallied, and he seems to have enjoyed the three weeks' sojourn at Yarkand. On December 4 Kashgar was reached, but the formal presentation to the King of Her Britannic Majesty's letters did not take place till January 10, 1874. In February an excursion was made to Artish and Kalti Ailak, and on March 17 leave was

taken of the King, and the return journey to India commenced. Returning by Yarkand, the Kara-korum Pass was ascended on June 16, and Stoliczka seems to have suffered from the great height. On the 17th the last record appears in his journal. On the 18th the first symptoms of a new attack of spinal meningitis showed themselves, and, despite all the care of his devoted friends, he breathed his last on the afternoon of the 19th, some eleven marches from Leh, where he was buried beneath a willow-tree. The Government of India placed a suitable inscription over his grave, and other evidences of the esteem and regard in which his memory is held will be found in the Museums of Calcutta and Vienna. And now another, and this not the least, will be found in this too brief, but sympathetic, record of his life and labours, written by one who knew him well, and who was able to appreciate not only the scientific labours of his friend, but his honesty and loyalty. A detailed list of all the scientific papers and published letters of Stoliczka between 1859 and 1874 is appended to this memoir.

THE IRON AND STEEL INSTITUTE

THE summer meeting of this Institute was held on the 6th to the 8th inst., in London, under the presidency of Dr. John Percy, F.R.S. In his introductory remarks, the President made special reference to some of the papers about to be read. He was very pleased to see that the employment of chromium in the manufacture of steel was receiving attention. As far back as 1821 Bertheir, in the *Annales des Mines*, had shown that iron with 1 to 1·5 per cent. of chromium forged well, whilst it took a keen edge when ground, and had a very high tenacity.—Dr. Percy exhibited a portion of a broken ploughshare of American manufacture, which was formed of three metals, and seemed to be produced by casting steel on both sides of malleable iron. He drew attention to mitis metal, but refrained from offering any opinion on the subject, referring simply to the statements put forward that by the use of aluminium in its composition the melting-point was lowered, whilst, as the product was more liquid, it ran better, and sound castings were more easily produced. In speaking of Indian metallurgy, reference was made to the iron column at Delhi, the largest piece of forged iron in the world. The President next drew attention to the development of iron and steel-making in the United States, showing its rapid progress, and how enormously the capacity for production, both in that country and here, was in excess of the demand, as regarded blast-furnaces, Bessemer converters, and open-hearth furnaces. The address concluded with some remarks on diminished cost of production; to what a degree this has been carried, and the influence it has had on the labour market may be inferred from the circumstance that nowadays a single lace-making machine does the work formerly done by 2000 women, that wood-planing, which used to cost 12*s.* per square foot, is now done for 2*d.* or 3*d.*, that the manufacture of gold chains has been reduced from 30*s.* to 3*s.* 6*d.*, and that a gross of steel pens may now be procured for 4*d.* which used to cost 7*l.* Sir Henry Bessemer proposed a vote of thanks to Dr. Percy for his address, which was seconded by Mr. Adamson, the President-Elect.

The first paper read was that of Sir Frederick Abel, F.R.S., and Colonel Maitland, Superintendent of the Royal Gun Factories, Woolwich, on the erosion of gun-barrels by powder-products. This, in the author's opinion is due to a softening, if not fusing, effect exerted upon the surfaces of the metal by the high heat of the explosion, an increase of this softening or fusing effect by the chemical action of the sulphur at the high temperature produced, and the mechanical action of the rush of gases, vapours, and liquid products upon the softened or fused surfaces. There are two kinds of scoring or erosion: muzzle-loading scoring is due to the rush of powder-products over the top of the projectile through the clearance or windage, which has to be allowed for facility of ramming home the shot along the bore in a muzzle-loader; breach-loading scoring is produced by the rush of the powder products behind a shot, acting as a gas-tight plug, during and immediately after its passage through the gun. Evidently erosion will increase with the amount of the powder products, with the pressure in the bore, and with the duration of the time of action, and it is important to ascertain what material best resists erosion by powder products, or what treatment of the material is best calculated to increase its power of resistance to erosion. With this object in view experiments were made on thirteen rifled barrels, of different steels, of $2\frac{1}{4}$ inch bore, firing 100 rounds each with $10\frac{1}{4}$ lb. charges of pebble powder and 6 lb shot, fitted with service driving rings; these barrels were screwed into the mouth of the chamber of a 22 cwt. breech-loader. Gutta-percha impressions were taken after each batch of twenty-five rounds. During the preparation of the barrels specimens were cut in prolongation of the bores and tested mechanically, and the proportions of carbon, silicon, and manganese were determined in samples of the metal. The average pressure of the gas was 13 tons to the square inch. The results of the experiments are given in a table, but neither the chemical analysis of the metals nor the testing machine gave any assistance in accounting for the position of the barrels in the mean order of merit in which they were placed by five skilled and independent observers. Thus the worst and the worst but one were respectively the highest and nearly the lowest in carbon, the first, fifth, and tenth were very closely allied both in analysis and as tested by the machine, and it became evident that some agency, hitherto unsought for, dominated the results. Separate and independent investigations were made by the writers of the paper, the one instituting a chemical and the other a mechanical examination of the metals. A chromic solution capable of exerting a very slow solvent action upon the metals brought their structure into relief, and the extent of erosion was found to be more or less referable to the less or greater amount of mechanical treatment the metal had received, and to the consequent extent to which uniform fibrous structure had been developed. Experiments made on the metal as cast, and forged to twice to four times, and to eight times its length proved that the more steel was forged or worked the less it suffered from the eroding effect of powder gas. This was found to be the case both as regarded hard and soft metal. Several members took part in the discussion, notably Mr. Adamson, Sir Frederick Bramwell, Sir Henry Bessemer, and Mr. Frederick Siemens.

The next paper, which was taken as read, was an elaborate report of 137 pages in length by Messrs. P. C. Gilchrist and E. Riley, "On the Iron-making Resources of the British Colonies and India, as illustrated at the Colonial and Indian Exhibition." It would appear the reporters are of opinion that, so far as the exhibits are concerned, the iron and coal producing power of the Empire is rather undershown, as with a proper application of the materials at the disposal of our colonies and India, they should at all events be able to supply their own requirements.

The next papers read were: "On some Early Forms of Bessemer Converters," by Sir Henry Bessemer, F.R.S., and "On Modifications of Bessemer Converters for Small Charges," by John Hardisty. The first of these contains descriptions of the different forms of converters selected by the author as typical of the whole, and which embrace the main features of ten several forms of apparatus which he has from time to time designed for the conversion of crude iron into steel. It was written with the double object of letting those who are seeking to improve the

process know what has already been done; while the general public ought not to remain ignorant of what legitimately belongs to them, and which, after the ample reward he has received for his inventions, the author desires they should enjoy without any restrictions. The author of the second paper holds that the making of steel in small quantities is a step in the wrong direction, because the steel cannot be made so cheaply; but, as he points out, it is to the interest of owners of small blast plant to possess the means of converting their product into steel, and of ironworkers who cannot find work for their puddling furnaces to make steel enough to keep their machinery at work rather than be dependent on larger firms for a supply of ingots. An American steel-maker in the discussion drew attention to the circumstance that in the United States, when the rail trade was brisk, it was impossible for the smaller works to obtain Bessemer ingots at all, and that they had to introduce small plant for self-preservation. From the statement of opinion it was evident that there was necessity for the original Bessemer converters and the smaller modified forms.

Mr. Frederick Siemens's paper on combustion with special reference to practical requirements draws attention to the means necessary for adoption to insure perfect combustion. The gases must be supplied in the exact chemical proportion in which they are required for combustion; they must be brought together in such a manner that the different molecules which have to enter into combination may readily do so, whilst every thing must be avoided which interferes with the motion of the gases while combustion is proceeding.

The author enters in detail on the way in which gases should be brought together, he explains that the Bunsen burner, though theoretically perfect, cannot be advantageously carried out in furnaces, as the flame of a Bunsen burner being almost non-luminous owing to free carbon not being liberated during combustion, has but little radiating power, and must in consequence transmit its heat by direct contact only. As the gases cannot generally be mixed before combustion, it is a matter of great importance how they are brought together when combustion commences, a mean being necessary between a too intimate mixture, producing a short flame having great heating but little radiating power, and an imperfect mixture, which does not allow of combination properly taking place. The third means necessary is the one to which the author has frequently drawn special attention, because neither the employment of gases in proper proportion, nor their proper mixture is sufficient to insure perfect combustion *if the disturbing influences of surfaces is allowed to interfere to prevent combustion, or to dissociate particles of gas already combined.* In the author's view the dissociation caused by hot surfaces is of various kinds, and takes place at different temperatures. At a comparatively low temperature, dissociation of hydrocarbons takes place, the carbon being liberated in the solid form as soot. At a moderately high temperature carbonic oxide is dissociated into solid carbon and carbonic acid gas; at a higher temperature the products of combustion begin to dissociate, steam splitting up into hydrogen and oxygen, and lastly, at a still higher temperature, depending upon the kind of surface with which the products of combustion come into contact, carbonic acid splits up into solid carbon and oxygen. From this it will be seen that dissociation has the effect of setting carbon free, and to its influence the formation of smoke is largely due.

The author then proceeded to show that smoke within a furnace chamber is caused by flame in the first instance touching surfaces which then become enveloped in a dense cloud of dissociated carbon, which prevents the heat rays from reaching them. The author illustrated his remarks by means of a gas-burner proposed to be used instead of the English fire-place, by the use of which it is stated that heat is much more uniformly distributed throughout a room. The flame was intensely bright and hot, due as explained to its being fed with hot air, and working with free development of flame that is entirely out of contact with any surfaces. The gas stove afforded considerable interest to the members, and the author by special request explained its mode of action.

The papers read on the last day of the meeting were two by Mr. F. Gautier, of Paris, on the casting of chains in solid steel, and on silicon in foundry iron. Hitherto chains have been made of wrought iron, the difficulty in the various processes of manufacture being the difficulty of securing a good weld; this, according to the author, is now overcome by a process of Messrs. Joubert and Leger, of Lyons, which combines chilled casting and instantaneous removal from the moulds. In the second paper the writer refers to the advantage of silicon in producing homogeneous steel and pig iron castings and improving foundry pig; he also drew attention to the introduction of ferrosilicon in French foundry practice. The author's views were in general supported by the members in discussion. Mr. F. W. Harbord's paper, "On the Elimination of Silicon, Phosphorus, &c., in the Basic Open-hearth Process," gave evidence that soft steel of the very finest quality could be produced from inferior material by this process, whilst the conditions of working in the Siemens furnace are peculiarly favourable to its production. Surgeon-Major Hendley's paper, "On the Process employed in Casting Brass Chains at Jeypore, Rajputana," was contributed by Mr. C. Purdon Clarke, and illustrated by samples. The papers on "Chrome Steel," by Mr. Brustlem, and on "American Blast Furnace Practice," by Mr. F. W. Gordon, Philadelphia, were put off to the next meeting.

NOTES

We regret to learn that Baron von Müller retires from the directorship of the Melbourne Botanic Gardens in June next.

The death is announced of M. Dubosc, a Paris optician, who assisted M. Léon Foucault in all his constructions, and especially in the organisation of his automatic electric lamp.

The Laboratoire d'Électricité created with the surplus of the Electrical Exhibition of 1881, held in the Palais de l'Industrie, will be erected on the site of the old Collège Rollin, on ground granted by the City of Paris. It will be open to electricians of every nation, and governed by the International Society of Electricians.

An ordinary General Meeting of the Institution of Mechanical Engineers will be held in the Yorkshire College, Leeds, on Monday, October 18, by invitation of the College authorities, in celebration of the opening of the Engineering Department of the College. The following papers will be read and discussed, as far as time permits:—"On Triple-Expansion Marine Engines," by the late Mr. Robert Wyllie, of Hartlepool; "Notes on the Pumping Engines at the Lincoln Water-Works," by Mr. Henry Teague, of Lincoln; "Description of a Portable Hydraulic Drilling Machine," by M. Marc Berrier-Fontaine, of Toulon.

The Commission of the French Budget having adopted without reduction all the proposals of the Government for the Algerian provinces, the construction of the large instruments for Bouzareah Observatory will be continued, and inspection of the heavens will be conducted on a large scale at Algiers. An observer connected with the Trocadero Observatory has been appointed to assist M. Trépied, and left Paris last week for his destination.

The photographic method has been established at the Algiers Observatory for the sun. Nine times out of ten the operation has been successful.

THE Duc d'Aumale has bequeathed to the Institute of France in perpetuity the mansion and domain of Chantilly, with its museum and all its other contents, to be preserved and maintained for the benefit of the French nation. This munificent gift is fettered by no conditions beyond those necessary for carrying out the main purpose of the testator. The value of the gift is estimated at thirty millions of francs, or nearly a million and a quarter sterling, and the income for maintaining it at 20,000*l.* per annum.

SOME time since, Herr Paul von Ritter, of Basle, gave the sum of 300,000 marks for the furtherance of scientific inquiry on the basis of the Darwinian theory. It has now been decided to employ half the interest in the maintenance of a "Ritter Professorship of Philogeny." The chair is to be filled by Dr. Arnold Lang, formerly scholar and assistant of Prof. Haeckel at Jena. Dr. Lang has for several years taken part in the work of the Zoological Station at Naples. The other half of the interest will be expended in grants for scientific travel, and in furnishing improved means of instruction in zoology at this University. Herr von Ritter has been made Doctor of Philosophy *honoris causâ.*

SINCE the end of 1883, regular observations of atmospheric electricity have been made at the Odessa Meteorological Observatory, and M. Klossofsky shows by the graphic method (*La Nature*) how there is an intimate relation between the variations of atmospheric pressure, and those of electric potential. Cyclonic movements of the atmosphere find a faithful echo in the indications of the electrometer, though sometimes mist, smoke, dust, and atmospheric precipitates may for a time mask the correspondence.

IN the course of a review in the *Chinese Recorder* of a new Chinese geometry by Dr. Mateer, Dr. Martin, the head of the Foreign Language College at Pekin, states that the first translation of Euclid into Chinese was made by the illustrious Father Ricci, "the apostle at once of religion and science." In time the paramount influence of Euclid grew into something like a bondage in the east as well as in the west. In the west a wholesome revolt took place long ago, but in China Euclid "has reigned with undisputed sway for three centuries, and nothing has been done even in the way of simplification until the work of Dr. Mateer. It is a strange fact that Ricci's Euclid was left standing through all these ages in the condition of a truncated pyramid. Only six books were translated by the great Jesuit, and the remaining nine were supplied about thirty years ago by Mr. A. Wylie, aided by Prof. Li Shenlon." Mr. Wylie subsequently translated Loomis's "Analytical Geometry and Differential Calculus"; but, says Dr. Martin, he would have done better to have commenced his mathematical textbooks by a version of Loomis's "Geometry," which, following the footsteps of Legendre, presents the whole subject in a compact and easily intelligible form. The translation of the Chinese titte of the older book is "First Book in the Science of Quantity"; that of Dr. Mateer's is "The Science of Form." A Chinese mathematician's view of the new work is given by Dr. Martin in the following words:—"This book presents the principles of geometry in a more concise form than Euclid, and omits nothing of importance that is found in Euclid. Besides the chapter on the three round bodies, there are throughout many excellent theories that were unknown to Euclid, especially those relating to spherical triangles, so essential to the study of astronomy." The price of the two volumes, it may be added, is about 2*s.* 3*d.*

THE number of the *Folk-Lore Journal* (vol. iv. part 4) just published, contains the first instalment of a paper by Capt. A. C. Temple, which promises to be the most important work yet published on the folk-lore of North-Western India. The writer has made it a practice to collect all the popular works published in the Punjaub relating to history, folk-lore, and religion. He has now about 350 of these in Arabic, Persian, Urdu, Hindi, Punjaubi, Pushtoo, and Sanscrit. It faithfully represents the current popular literature of the day in the Punjaub. Capt. Temple has had abstracts of the books prepared, and twenty-eight of these are published in the present paper. In the somewhat distant future when the whole 350 are published, few regions will have had their folk-lore so thoroughly investigated as the Punjaub.

ACCORDING to *Die Natur* a remarkable collection of minerals exists in the cellars of the Academia San Fernando at Madrid. It is contained in a number of boxes which have filled the cellars for about 200 years, and which may remain there as long again unless some better fortune befalls them than that which has attended them in the past. They come down from the golden age of Spanish domination in South America and in Mexico, when the mines of these regions made them the El Dorado of the globe. No one knows exactly the contents of the boxes, but they are believed to contain the rarest objects, although the scientific importance of collections was but little appreciated in the days when this one was made. It appears also that collections made by Humboldt during his travels in America, and handed over by him as a kind of scientific tribute to the Spanish Government, are in the same Academy "locked up since 1804 in a press, untouched." With respect to the famous skeleton of the *Megatherium americanum*, Cuv., found by the Marquis de Loreto on the banks of the Rio Luxon near Buenos Ayres in 1778, which is in the Museum of the Academy, its present state is described by the brothers Fraas of Stuttgart in their letters from the south of France and Spain, just published under the title of "Aus dem Süden," as being one of the utmost confusion. The bones are bored for mounting, but they are "completed and restored" to the verge of the impossible. The bones are placed in absurd positions, and parts which were inconvenient to the mounter are put aside altogether. The writers ask what the state of instruction in natural history must be in an Academy where such things are possible.

WE have never heard an adequate explanation of the extraordinary delay which takes place in the issue of the Annual Reports of the Japanese Ministry of Education. They are as a rule three years behind time. That for 1882 has only just been published. They contain a considerable number of statistics, but this alone would not account for the delay, for the publications of, say, the Japanese Meteorological Observatory contain far more and more complicated tables, yet the latter appear in reasonable time. It is very difficult to feel any interest in an annual report of the year 1882 at this date; conditions have altered, the circumstances are different, the inspector's report for 1885 may show energy and success where those of 1882 had to reveal apathy and ignorance. The Tokio University, for example, has been wholly remodelled, and one reads with very languid interest now that in 1882 there were such and such departments, so many graduates, and the like. The details in the report, especially those relating to students abroad, to the position of libraries and museums, their utility to and appreciation by the public, would be of interest and well worth quotation, or at least a short summary, if they were those of last year instead of those of four years ago. These reports may perhaps be of use to any one who undertakes to write a history of education in Japan; for any practical present use they are they might as well remain unpublished in the Archives of the Education Department in Tokio. We can perceive nothing in the nature of things, or in the design or details of the reports to prevent them being produced regularly in the first half of the year succeeding that to which they refer.

In a very interesting paper in the new number of the *Asiatic Quarterly Review*, Miss E. M. Clerke, writing on "Arabic Analogies in Western Speech," says that whole classes of astronomical, astrological, and generally scientific terms are a standing memorial to the debt of culture Europe owes to the East. Logarithm is a corruption of *el-jowarzeem*, and algebra of *el-jabr wa'lmukábala*, literally, the integration and comparison. Alembic is *el-anbik*, a retort, whence the Italian *lambiccare*, to distil; and nearly all the terms used in alchemy denote its Oriental origin. Star-names come from the same source: Algol is *el-ghol*, the ghoul; and Vega, a fragment of *nasser-el-waga*, the falling eagle. Most precious stones and minerals, as sapphire, emerald, bezoar, jasper, amber, antimony, are transparent disguises of Arabic originals; jewel itself is from *jouhour*. Similarly the names of poisons and remedies, as well as maladies, come from Arabia: thus arsenic is *ez-zernikh;* massage, the fashionable cure by friction, is from *mass*, to handle, and leprosy is an obvious corruption of *el-abras*. Again, such words as spinach, endive, chicory, saffron, arrowroot, cotton, hemp, carraway, cummin, and aloe are obvious derivatives from Arabic. The names of many flowers come from Arabia, by adoption from Persian, also fruits, as lemon and orange. Carat is an Arabic weight. Monsoon comes from *mousem*, a fixed time, and sirocco and simoom. These and a hundred others given by Miss Clerke "go to prove that the world is one vast commonwealth of ideas, most widely shared by the classes least conscious of their indebtedness to foreign influence."

Father Dechevrens, of the Siccawei Observatory near Shanghai, writing on a violent typhoon which visited that district on August 14, doing considerable damage, says that it was remarkable by the long persistency of the low pressures that continued from 3 a.m. on the 14th to 5 a.m. on the 18th, that is, ninety-three hours, during the whole of which time the wind blew hard, and during the two last days the rainfall rose from 6 inches to 12. He says this is a singular and very rare phenomenon, and to explain it he follows the typhoon all along its course. Like the typhoon of the same date in 1881, it came from the open sea. On the 11th the barometer at Manilla caused suspicion of a storm to the north-east of Luzon and east of Formosa. The Loochoo Islands must have been passed on the afternoon of the 13th, and at noon on the 14th it reached the coast of China about Wenchow. Having got to the mainland, the storm proceeded for some time to the west through the province of Kiangsi, and then was divided. One part recurved to the south-west towards Kwangsi and Tonquin, and is easily followed by observations made at Amoy and Hong Kong; the other part of the depression turned round to the north and got nearer to the Yang-tse River. On the morning of the 18th an entire change took place in the atmospheric conditions. The second depression was in its turn divided, and while the portion higher in latitude formed itself into a distinct storm and got away to the north, the other part approached Shanghai and put back to sea through the mouth of the Yang-tse. When Shanghai was placed between the two depressions, the air, not knowing, as it were, towards which of them to flow, got rapidly calm. While the two centres were thus getting away in opposite directions, the Siccawei barometer rose without any strong wind blowing. This, concludes Father Dechevrens, is a new phase of this singular typhoon, the centre of which passed very close to Shanghai without giving birth to any gale, except the one that had preceded the division and the departure. It is to be hoped that the learned writer may be able to give this typhoon, with the peculiarities here noted, the same detailed and thorough study that he gave that of 1881. In the latter instance he gave what may be called the life-history of a storm from its birth in the China Seas, almost to its dissipation far in the interior of he continent of Asia.

The luminosity of insects has been lately studied in a very careful manner by Dr. Dubois, one of M. Bert's students. The animal selected was the American *cucujo*, or *Pyrophorus noctilucus*. It has three luminous organs—two prothoracic and one ventral. Dr. Dubois opposes the view that the light results from direct oxidation of the substance of the luminous organs, by oxygen of the air coming through the tracheæ. In pure oxygen the luminosity is the same as in air, and it is the same in pressures under one atmosphere. Nor does compressed oxygen affect it, and this gas cannot restore the light when extinct in organs which yet respond to mechanical agents or electricity, even when the pressure is raised to four atmospheres. The prothoracic plates give a good illumination in front, laterally, and above, and serve when the insect walks in the dark; when it flies or swims, its fine abdominal lantern is unmasked (and the abdomen raised) throwing downwards an intense light with much greater range. The insect seems to be guided by its own light. If the prothoracic apparatus is quenched on one side with a little black wax, the cucujo walks in a curve, turning towards the side of the light. If both sides are quenched, the insect walks hesitatingly and irregularly, feeling the ground with its antennæ, and soon stops. The light of the cucujo gives a pretty long spectrum from the red to the first blue rays; when the light diminishes, this shortens somewhat on the side of the blue, but more on the other side. The maximum is about wave-length 528 μ 56 (as in the solar spectrum). The light is more green than that of *Lampyris noctiluca*. It is capable of photography, but does not develop chlorophyll. The prothoracic organs of six insects did not set a radiometer in motion, but they affected a Melloni pile slightly. No distinct electric action could be traced in the organs. Separated from the body, the organs are still brilliant. If the insect is deprived of water, it ceases to produce light; and it recovers the power when plunged in water. The eggs may be dried to the extreme limit, at ordinary temperature, without losing their light-yielding power; put in water after eight days even, they become luminous again. Further, if the luminous organs are dried *in vacuo*, and pulverised in a mortar, a little water (even if freed from gas by boiling) makes the mass luminous throughout. Dr. Dubois finds the photogenic substance to be an albuminoid, soluble in water and coagulable by heat; it enters into conflict with another substance, of the diastase group, and part of the energy thus liberated appears as light.

Some experiments lately brought before the Paris Academy by M. Luvini, combine with those of other observers (he considers) in warranting the conclusion that "gases and vapours, under any pressure, and at all temperatures, are perfect insulators, and cannot be electrified through friction, either with one another, or with solid or liquid substances."

The *Giornale d'Agricoltura e Commercio* for August reports the discovery in West Africa of a new variety of coffee-plant, whose berry appears greatly to resemble that of Arabia in appearance and flavour. It grows, however, not on a shrub but on a tree nearly 7 feet high, which develops rapidly and yields an abundant crop. Arrangements are already being made for introducing its cultivation in favourable localities.

The additions to the Zoological Society's Gardens during the past week include a Brown Capuchin (*Cebus fatuellus* ♀) from Guiana, presented by Messrs. Kühner, Hendschel, and Co.; a Macaque Monkey (*Macacus cynomolgus* ♀) from India, presented by the Countess de Geloes; a Yellow-footed Rock Kangaroo (*Petrogale xanthopus* ♀) from South Australia, presented by Mr. G. Langborne, Chief Officer s.s. *Rome;* a Common Squirrel (*Sciurus vulgaris*), British, presented by Miss Gertrude Hudson; two Lanner Falcons (*Falco lanarius*) from Eastern Europe, presented by the Baron D'Epremesnil; a Blue and Yellow Macaw (*Ara ararauna*) from South America, presented

by Mrs. George Quish; a Gannet (*Sula bassana*), British, presented by Mr. J. H. Gurney, F.Z.S.; two Common Chameleons (*Chamæleon vulgaris*) from North Africa, presented respectively by Mr. Charles T. Port, F.Z.S., and Mr. T. H. Carlton Levick; a Common Viper (*Vipera berus*), British, presented by Mr. W. H. B. Pain; a Porto Rico Pigeon (*Columba corensis*), a Triangular-spotted Pigeon (*Columba guinea*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

M. THOLLON'S MAP OF THE SOLAR SPECTRUM.—M. Thollon, in the *Bulletin Astronomique* for July, gives some interesting details concerning the great map of the solar spectrum which he presented to the Paris Academy of Sciences about a year ago. He had completed an earlier design in 1879, but the positions of the lines in it had not been determined with the precision he desired. He therefore resolved to go over the work again, and to make a chart which should represent the positions, breadths, and relative intensities of the lines as faithfully as possible. The work has required four years of continuous toil to carry it from A to *b*, at which point M. Thollon now leaves it. M. Trépied proposes to carry it on to the violet. M. Thollon's map shows the spectrum under four different aspects: as seen when the sun is 10° high, and the air contains but little water-vapour; then as with the sun at 30° of altitude, first with the air saturated with water-vapour, and next when the air is very dry; and, lastly, the solar spectrum as it would be seen outside our atmosphere. It is therefore easy to see which lines are truly solar, which due to water-vapour in our atmosphere, and which to dry air. M. Thollon finds the dry-air lines limited to the great groups A, B, and α, which M. Egoroff ascribes to oxygen. Besides the water lines, which are arranged in seven groups, M. Thollon on a single occasion observed a vast number of telluric lines between α and D, the special origin of which he was not able to determine.

The measures were made with a very fine glass pointer, which allowed a bisection of a line to be made with great exactness, the probable error of an observation being less than 1/700 of the interval between the D lines. The breadth of a line was determined by observing at what distance from its extreme point, the glass pointer was equal to it in breadth. The intensity of the lines were estimated by eye. The map, which will be published in the *Annales de l'Observatoire de Nice*, is more than 33 feet in length, and embraces more than a third of the visible spectrum. From the scale on which it is drawn, the number of lines—about 3200—which it contains, the precision of the measures, and the fullness of the information given concerning the telluric lines, it will be, that which its author has striven to make it, the fullest and most perfect chart of the spectrum yet published. One of the chief purposes which it will serve will be to afford information as to the occurrence of changes in the spectrum, and M. Thollon shows by a diagram of the spectrum between B and C that we have strong reason to suspect that several lines have greatly altered in intensity since the date of Ångström's famous chart.

COMET FINLAY.—Mr. Finlay, of the Cape of Good Hope Observatory, discovered a comet on September 26. It appears to be probably identical with Comet 1844 I., its elements being given by Dr. Holetschek as follows:—

T = 1886 Nov. 22·6821 Berlin Mean Time.

$$\left.\begin{aligned} \pi - \Omega &= 299^\circ\ 14'\ 21'' \\ \Omega &= 48\ \ 35\ \ 55 \\ i &= 3\ \ 23\ \ \ 0 \end{aligned}\right\}\ \text{Mean Eq. } 1886{\cdot}0.$$

$$\log q = 0{\cdot}08793$$

Ephemeris for Berlin Midnight

1886	R.A. h. m. s.	Decl. ° ′	Log Δ	Log *r*	Brightness
Oct. 19	18 8 10	26 40·2 S.	0·1442	0·1249	1·37
23	18 21 52	26 33·9	0·1410	0·1173	1·44
27	18 36 11	26 22·2	0·1378	0·1105	1·50
31	18 51 6	26 4·7 S.	0·1347	0·1044	1·57

The brightness on September 26 is taken as unity.

NEW MINOR PLANET.—A new minor planet, No. 260, was discovered by Herr Palisa at Vienna on October 3.

NEW COMET.—A new comet was discovered by Mr. E. E. Barnard on October 4. It was independently observed by Dr. Hartwig on the following night. October 5, 16h. 2m. G.M.T., R.A. 10h. 37m. 24s.; Decl. 1° 3′ N. It is described as bright and round. Daily motion + 1·5s. in R.A., and + 3′ in Decl.

THE PULKOWA OBSERVATORY.—M. Struve has issued his Annual Report for the year ending May 25, 1886. During the year the fundamental determinations of star places for 1885·0 were regularly persevered in with the great transit instrument and the vertical circle. With the former Herr Wagner and his assistants, Wittram and Harzer, observed 4785 transits. With the exception of 110 observations of the sun these refer exclusively to the 383 Pulkowa fundamental stars. With the vertical circle Herr Nyrén obtained 739 complete observations, including 105 observations of the sun. The fundamental declination determinations for 1885 would be almost completed, had not Herr Nyrén wished to repeat the observations with a reversion-prism eye-piece attached to the instrument in order to investigate certain systematic discordances. Herr Romberg, observing with the meridian-circle, obtained during the year 4359 observations, chiefly of stars with large proper motion, comet stars, &c. The great 30-inch refractor has been intrusted to Hermann Struve, and has been employed in observing the fainter double stars of Burnham's catalogues, the satellites of Mars, Saturn, and Neptune, the Maja nebula (discovered photographically at Paris), and *Nova* Andromedæ, which was easily visible on January 27. M. Struve speaks in terms of the highest approval of the instrument, both as regards its optical power and as regards the mounting, the movement of the dome, &c. The 15-inch refractor has been used by H. Struve for obtaining micrometer measures of the brighter satellites of Saturn. He has obtained 42 comparisons of Japetus with Titan, 40 of Titan with Rhea, and 23 of Rhea with Dione. Herr Backlund has continued in charge of the 4-inch heliometer, and has measured with it the relative positions of Jupiter's satellites, for a determination of the mass of Jupiter, and of the orbits of the satellites. He has also undertaken a series of measures to determine the parallax of Bradley 3077, which has a large proper motion. In the physical department of the Observatory Herr Hasselberg, using a Steinheil objective of 50 mm. aperture and 1·5 m. focal length, in combination with two bisulphide of carbon prisms, has succeeded in obtaining excellent photographic images of the solar spectrum. Between wave-lengths 4000 to 4227 on Ångström's scale, he was able to count some 650 lines, whereas Vogel's map gives but 450 in the same space. During the course of the year 140 sun pictures were taken on 110 days.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *OCTOBER* 17–23

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on October 17

Sun rises, 6h. 29m.; souths, 11h. 45m. 24·2s.; sets, 17h. 2m.; decl. on meridian, 9° 20′ S.: Sidereal Time at Sunset, 18h. 46m.

Moon (at Last Quarter on October 20) rises, 19h. 15m.*; souths, 2h. 55m.; sets, 10h. 42m.; decl. on meridian, 16° 55′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury ...	7 46 ...	12 34 ...	17 22 ...	14 33 S.
Venus	5 18 ...	11 4 ...	16 50 ...	3 32 S.
Mars	10 45 ...	14 43 ...	18 41 ...	22 45 S.
Jupiter... ...	5 52 ...	11 25 ...	16 58 ...	6 3 S.
Saturn	21 51* ...	5 53 ...	13 55 ...	21 19 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich)

Oct.	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
17 ...	111 Tauri ...	5½ ...	19 24 ...	20 14 ...	69 240
17 ...	117 Tauri ...	6 ...	21 9 ...	21 17 ...	339 322
23 ...	B.A.C. 3538 ...	6½ ...	0 53 ...	1 23 ...	348 274
23 ...	44 Leonis... ...	6 ...	2 6 ...	2 38 ...	344 271
23 ...	B.A.C. 3562 ...	6½ ...	2 14 ...	2 49 ...	347 268

Oct.	h.	
20 ...	7 ...	Saturn in conjunction with and 3° 16′ north of the Moon.
21 ...	4 ...	Mercury at greatest distance from the Sun.
22 ...	21 ..	Venus in conjunction with and 0° 18′ north of Jupiter.

Variable Stars

Star	R.A. h. m.	Decl. ° ′			h. m.	
U Cephei	0 52·2	81 16 N.	Oct.	19,	5 11	*m*
R Sculptoris	1 21·7	33 8 S.	,,	22,		*M*
Algol	3 0·8	40 31 N.	,,	19,	5 25	*m*
			,,	22,	2 14	*m*
λ Tauri	3 54·4	12 10 N.	,,	18,	0 46	*m*
			,,	21,	23 38	*m*
R Canis Minoris	7 2·4	10 12 S.	,,	21,		*M*
S Cancri	8 37·4	19 27 N.	,,	21,	4 38	*m*
U Ophiuchi	17 10·8	1 20 N.	,,	18,	4 25	*m*
		and at intervals of			20 8	
β Lyræ	18 45·9	33 14 N.	Oct.	18,	0 0	*m*
			,,	21,	5 0	*M*
η Aquilæ	19 46·7	0 43 N.	,,	19,	2 0	*M*
			,,	23,	21 30	*m*
δ Cephei	22 24·9	57 50 N.	,,	18,	2 0	*m*

M signifies maximum; *m* minimum.

Meteor Notes

There are a large number of active radiants visible during the present week, the chief shower being the *Orionids*, R.A. 90°, Decl. 15° N. Other radiants are that of the *Arietids*, R.A. 31°, Decl. 9° N.; near ψ Aurigæ, R.A. 78°, Decl. 32° N.; that of the *Gemellids*, R.A. 108°, Decl. 24° N.; near μ Leonis, R.A. 143°, Decl. 28° N.; and near Vega, R.A. 283°, Decl. 43° N. October 18 is a fireball date.

THE NEW ELEMENT, GERMANIUM[1]

SOME months ago Dr. Clemens Winkler announced the discovery of a new element which he named germanium, a preliminary account of which has already appeared in these columns. Dr. Winkler has since been able to make a more systematic examination of the subject, and he now describes in detail the preparation and properties of the new element and also of a number of its compounds. The view he first held, that germanium occupied a position in the periodic system intermediate between antimony and bismuth, he now shows to be untenable, there being now no doubt that it is the *ekasilicium* of Mendelejeff prophesied fifteen years ago. This latter view was, in fact, expressed by Richter, Mendelejeff, and Lothar Meyer shortly after the discovery of germanium.

The new element occurs, as previously stated, in the recently-discovered mineral, argyrodite. Its isolation is, however, difficult, especially from the presence of arsenic and antimony in minerals which accompany argyrodite. The formula assigned to the latter mineral is $3Ag_2S,GeS_2$.

The following is the best method for separating the germanium. The finely-powdered mineral is intimately mixed with an equal weight of soda and sulphur, and the whole submitted to the action of a moderate red heat in a Hessian crucible. The product is powdered whilst still warm, and repeatedly boiled with water; the aqueous extract is slightly acidulated with sulphuric acid, and the precipitated sulphides of arsenic and antimony allowed to settle. On then adding a considerable excess of hydrochloric acid, the germanium sulphide is thrown down as a white voluminous precipitate; this is gently roasted, then heated with concentrated nitric acid, and finally ignited. The germanium oxide obtained may be reduced by ignition in hydrogen.

Germanium has a melting-point apparently somewhat lower than that of silver—that is, about 900°—and at a temperature a little higher than this it appears to volatilise. It crystallises in octahedra, is extremely brittle, has a perfect metallic lustre, and a grayish-white colour; its specific gravity is 5·469 at 20°·4. It is insoluble in hydrochloric acid, is readily dissolved by aqua regia, is converted into a white oxide by nitric acid, and into a soluble sulphate by concentrated sulphuric acid.

Determinations of the atomic weight of germanium were made by estimating the percentage of chlorine in the chloride, $GeCl_4$, and the number 72·32 was obtained as the mean of four experiments, this number agreeing closely with the atomic weight of Mendelejeff's ekasilicium.

The specific heat of the new element has been determined by Nilson and Pettersson, at temperatures between 100° and 440°, with the following results:—

[1] Clemens Winkler, *Journal f. prakt. Chemie*, 1886, pp. 177-229.

	1	2	3	4
Specific heat	0·0737	0·0772	0·0768	0·0757
Atomic heat	5·33	5·58	5·55	5·47

Compounds of Germanium.—Oxides: There are two oxides of germanium, namely, GeO and GeO_2. The former is obtained in the hydrated condition by heating the corresponding chloride ($GeCl_2$) with sodium carbonate solution; on heating the precipitate in a current of carbonic anhydride, the water is expelled and the grayish-black oxide, GeO, remains. The higher oxide is obtained by burning germanium in oxygen, or by decomposing the chloride, $GeCl_4$, by water; it forms a dense white powder slightly soluble in water, possesses both basic and acid properties, the latter being, however, the more pronounced. *Sulphides:* Two of these are likewise known, corresponding to the oxides. The lower sulphide, GeS, is obtained from the disulphide either by heating it with an excess of germanium in a current of carbonic anhydride, or by gently igniting it in a current of hydrogen; it forms beautiful thin plates of almost metallic lustre and having a gray-black colour. Germanium disulphide, GeS_2, is obtained by precipitating a solution of the dioxide by sulphuretted hydrogen with the addition of a considerable excess of a mineral acid; it is then thrown down as a bulky white precipitate which is very appreciably soluble in water. *Chlorides:* The dichloride, $GeCl_2$, is formed when hydrochloric acid gas is passed over heated pulverulent germanium or its sulphide; it is a thin colourless liquid, which fumes strongly on exposure to the air. The tetrachloride, $GeCl_4$, is produced by burning germanium in chlorine, or by distilling a mixture of germanium with mercuric chloride; it is a thin colourless liquid boiling at 86° C. and fuming in the air; its specific gravity at 18° is 1·887. *Iodide:* A tetriodide, GeI_4, only is known, and is best obtained by heating germanium in iodine vapour; it forms a yellow powder, melts at 144°, and boils between 350° and 400°.

AUSTRALASIA

THE following have been quite recently received from Australasia:—

Transactions and Proceedings of the New Zealand Institute for 1885, vol. xviii. (first of new series) (Wellington, May 1886). This volume commences a new series of these well-known *Transactions*, in which, "for convenience and economy" the size of the page has been reduced from the handsome royal octavo to a demy octavo size. The volume contains over 450 pages, and some 17 plates. Among the more important contributions which are printed in the *Transactions* may be noted the following:—*Miscellaneous:* E. Tregear, the Maori in Asia.—Dr. J. Haast, stone weapons of the Moriori and the Maori.—Rev. S. W. Baker, new volcano in the Friendly Islands.—*Zoology:* T. Jeffery Parker, skeleton of Notornis.—T. White, feathers of two species of Moa.—A. Reischek, numerous papers on New Zealand birds.—W. Colenso, on the bones of a new species of Sphenodon.—W. W. Smith, on the habits of *Ocydromus australis*.—J. W. Kirk, on a new species of Argonauta.—Geo. M. Thomson and Chas. Chilton, critical list of New Zealand Crustacea Malacostraca.—E. Meyrick, New Zealand Micro-Lepidoptera (Tineina, Pars.).—A. T. Urquhart, on the spiders of New Zealand (many new species described and figured).—J. W. Kirk, on some species of Vorticella from Wellington describes thirteen species, of which two are given as new, and figured.—*Botany:* W. Colenso, on some newly-discovered cryptogamic plants of New Zealand, describes some fifty-nine species; two ferns, fourteen mosses, and forty-three Hepaticæ, and hints that it may be the last lot of novelties that, owing to age, he may himself collect and describe.—On some new or rare native plants, chiefly phanerogams; on *Clianthus puniceus*, Sol.—D. Petrie, on new species of native plants.—R. M. Lang, on classification of Algæ, and on the Fucoids of Banks Peninsula.—T. F. Cheeseman, three new species of Coprosma.—T. Kirk, additions to the flora of Nelson.—*Geology:* Capt. F. W. Hutton, the geology of Scinde Island; new Tertiary shells; the Wanganui system, with a catalogue of the Mollusca.—A. McKay, on the age of the Napier limestone.—*Astronomy:* Notes on the total eclipse of the sun of September 9, 1885, being a digest of many communications.—*Chemistry:* W. Skey, on a new mineral (awaruite) from Barn Bay.—W. S. Hamilton, on platinum crystals in the ironsands of Orepuki Goldfield.

Proceedings of the Linnean Society of New South Wales, vol. x. part 4, with 18 plates (Sydney, April 1886).—Dr. R. von

Lendenfeld, studies on Sponges: (1) the vestibule of *Dendrilla cavernosa*, sp.n.; (2) on *Raphyrus luxonii*, a new gigantic species from Port Jackson; (3) *Halme tingens*; (4) two cases of mimicry in Sponges (plates 39-43).—On recent changes in the forest flora of the interior of New South Wales; notes how the Pine Scrub (Callitris) rapidly supersedes the angiospermatous trees. The larva of a beetle (*Diadoscus erythrurus*) in part keeps the pine in check; drought seems favourable to the development of the beetle, or at least, by affecting the vegetation of the pine, enable its ravages to be more felt.—On the Australian fresh-water Rhizopoda.—On an Alga forming a pseudomorph of a siliceous Sponge.—On the dorsal papillæ of Onchidium.—Fourth addendum to the Australian Hydromedusæ.—E. P. Ramsay and J. Douglas-Ogilby, descriptions of many new or rare fishes.—George Masters, catalogue of the described Coleoptera of Australia, part 2.—N. de Miklouho-Maclay and Wm. Macleay, the Plagiostomata of the Pacific, part 3 (plates 45, 46).—A. Sidney Olliff: Trogositidæ of Australia.—On a new species of Chrysophanus.—On Australian Ptinidæ.—W. A. Haswell, on some Australian Polychæta, part 1 (plates 50-55).—E. Meyrick, Australian Micro-Lepidoptera.—J. Brazier, a new Ochidium.—New land and fresh-water Mollusca from New Guinea.

Second series, vol. i. part 1, with 6 plates (May 25, 1886).—E. P. Ramsay and J. Douglas-Ogilby, descriptions of new Australian fishes; new species of fish from New Guinea; a new Coris from the New Hebrides.—E. P. Ramsay, on a new genus and species of fresh-water tortoise from the Fly River, New Guinea (plates 3-6).—George Masters, catalogue of Australian Coleoptera, part 3.—F. Ratte, *Crioceras australe*, Moore (?), a Lower Cretaceous fossil from Queensland (plates 1, 2).—Wm. Macleay, the insects of the Fly River, New Guinea.—C. W. de Vis, on some Geckos in the Queensland Museum.—A. S. Olliff, on a new Aphanipterous insect from New South Wales.—Wm. A. Haswell, on the myology of *Petaurista taguanoides*.—Capt. F. W. Hutton, the Mollusca of the Pareora and Oamaru systems of New Zealand.

Proceedings of the Royal Society of Queensland, vol. ii. parts 1 and 2, June 1886, contain, among others:—W. E. Armit, notes on the philology of the islands adjacent to the south-eastern extremity of New Guinea (pp. 2-12), and on the Papuans (pp. 78-116).—C. W. De Vis, on the bones and teeth of a large extinct lizard (pp. 25-31, plates 1-3).—On an extinct Ornithorhynchus (pp. 35-40, plate 4).—On some new species of Salarias, and on a new species and genus of lizard (pp. 56-61).—On a fossil Saurian (pp. 181-192, plates 10-15).—Henry Tryon, on Queensland harvesting-ants.—W. A. Tully, short account of the measurement of the base-line in connection with the trigonometrical survey of Queensland.—Baron von Müller, on a new tiliaceous tree (*Elæocarpus Bancroftii*) from North Eastern Australia.

Proceedings of the Royal Society of Tasmania for 1885 (Tasmania, 1886).—From the records of the *Proceedings* it is interesting to learn that, though the Society has lost the exclusive control over the Museum and Gardens, which now are managed by trustees, some of whom are elected by the Society, yet the work of the Society continues to develop, and its library to increase. This volume is accompanied by a sketch-map, coloured, giving the general geological features of Tasmania, by C. P. Sprent and R. M. Johnston; and a geological chart, by Mr. Johnston, showing the proposed provisional classification of the stratified rocks of Tasmania and their equivalents elsewhere.—Among the more important papers we note the following:—R. M. Johnston, various memoirs on the geology and palæontology of Tasmania.—R. A. Bastow, on the mosses and Jungermania of Tasmania.—W. F. Pettard, new Tasmanian marine shells.—Baron F. von Muller, notes on J. J. H. de Labillardière (with a portrait).—Capt. Shortt, earthquake-phenomena in Tasmania.—T. Stevens, on boring for coal in Tasmania.

ON THE OCCURRENCE OF CELLULOSE IN TUBERCULOSIS

CELLULOSE, the principal constituent of the vegetable cell-wall, has been found to occur also in some animals; the mantle of *Phallusia mamillaris* and of Cynthia, and the external coat of Salpa consist mainly of tunicin, or animal cellulose. Now a further very valuable contribution to our knowledge of the occurrence of this body has been made in Vienna by Herr Ernst Freund, working at Prof. E. Ludwig's laboratory. Freund has succeeded in preparing from some of the organs and blood of tuberculous persons a substance exactly resembling cellulose, and showing all the reactions which have hitherto been described as peculiar to the latter. The reactions employed were the following:—(1) Conversion of cellulose when dissolved in concentrated sulphuric acid into dextrose on boiling with dilute sulphuric acid; (2) resistance if treated with Schultze's reagent, a mixture of nitric acid and chlorate of potassium; (3) yielding of a collodion-like mass by the action of nitric acid and ether; (4) assuming a blue colour by the action of iodine in presence of concentrated sulphuric acid or chloride of zinc solution; (5) assuming a violet colour by the action of a naphthol when dissolved in concentrated sulphuric acid (Molisch's reaction); (6) insolubility in common (indifferent) solvents (dilute alkalies); (7) solubility in a solution of cupric hydroxide in ammonia. The substance obtained from miliary tubercles and from the blood of tuberculous persons was subjected to ultimate analysis in three cases, and yielded between 45·12 and 44·70 per cent. C, and between 6·41 and 6·19 per cent. H; while 44·74 per cent. C and 6·17 per cent. H corresponds to $C_6H_{10}O_5$. A quantitative determination of the cellulose of the tubercles has not been made. The researches were carried out on material from twenty-five tuberculous and thirty non-tuberculous cases. The tuberculous material (lungs, spleen, peritoneum with miliary tubercles, blood) embraced cases of conglomerated as well as of infiltrated tuberculosis in the different stages of the disease. The non-tuberculous material examined was taken partly from healthy organs, partly from organs affected by various diseases—as, *e.g.*, from pneumonia, emphysema, pulmonary gangrene—and failed to show any of the reactions described above. Carcinomatous, sarcomatous, lupoid, syphilitic, and other non-tuberculous granulations were also examined with negative results. From his researches Herr Freund makes the suggestion that in tuberculous growths and in the blood of tuberculous persons cellulose forms an intrinsic constituent. We need not refer to the importance and suggestiveness of Freund's discovery for pathological science, making further researches on this subject very desirable.

DISINFECTION BY HEAT

THE Annual Report for 1884 of the Medical Officer of the Local Government Board contained a memoir, by Dr. H. F. Parsons, on the subject of disinfection by heat. Of this memoir the leading points are here given.

In considering the applicability of heat as a means of disinfection, several distinct questions present themselves for solution. It has first of all to be determined what degree of heat and duration of exposure are necessary under different conditions, as of moisture and dryness, in order to destroy with certainty the activity of the contagia of infectious diseases.

We have next to ascertain how the required degree of heat may be made to penetrate through bulky and badly conducting articles, *e.g.* of clothing and bedding, for the disinfection of which the application of heat is especially employed.

We have also to learn whether such articles can be submitted to the required degree of heat without injury, for if not, disinfection presents little advantage over destruction.

After giving a *résumé* of the results of previous experiments to ascertain the degree of heat necessary to destroy the contagia of infectious diseases, from those of Dr. Henry published in the *Philosophical Magazine* for 1831, to those of Koch and his coadjutors (*Mittheilungen aus dem kaiserlichen Gesundheitsamte*, Berlin, 1881), the author states the results of a series of experiments made by him in conjunction with Dr. Klein, who prepared the infective materials, and, after these had been exposed to disinfecting processes, tested the results by inoculation on animals; control inoculations with unheated portions of the same materials being also in all cases made.

The following were the infective materials employed:—

(1) Blood of guinea-pig dead of anthrax, containing bacillus anthracis without spores.

(2) Pure cultivation of bacillus anthracis in rabbit broth, without spores.

(3) Cultivation of bacillus anthracis in gelatine, with spores.

(4) Cultivation of bacillus of swine fever (infectious pneumo-enteritis of the pig) in pork broth.

(5) Tuberculous pus, from an abscess in a guinea-pig which had been inoculated with tubercle.

Infectious pneumo-enteritis of the pig (swine fever) has been shown by Dr. Klein to be caused by the introduction into the body of the affected animal of a specific bacillus. This disease among pigs is highly infectious, the contagium being transmissible from pig to pig through the air, and persisting in infected buildings in a similar manner to the observed behaviour of small-pox and scarlet fever among human beings; and, though not transmissible to mankind, it can be inoculated upon rodents, although in the latter animals it is not contracted by infection received through the air.

The experiments on the disinfecting power of dry heat were mostly made in a copper hot-air bath, or in one improvised of flower-pots, and furnished with a Bunsen's regulator; those with steam were made in a felt-covered tin cylinder, through which passed a stream of steam from a kettle beneath.

The mode of procedure in exposing the materials to heat was as follows:—Strips of clean flannel were steeped in the respective infective fluids, dried in the air, wrapped separately and loosely in a single layer of thin blotting-paper, and suspended in the centre of the apparatus in company with a thermometer, so placed that its bulb was close to the packets of infected material.

The following were the results of the experiments with dry heat:—

Anthrax bacilli without spores were sterilised by exposure for five minutes only to a dry heat varying between 212° and 218° F.

Spore-bearing cultivations of the bacillus anthracis, on the other hand, did not lose their vitality by a two hours' exposure to 220° F., but were sterilised by exposure for four hours to 220° F., or one hour to 245° F.

A rabbit inoculated with swine fever virus which had been exposed to dry heat varying between 212° and 218° F. for an hour remained well; but one inoculated with virus exposed to a similar heat for only five minutes, died of swine fever after nineteen days, the usual time of death after inoculation being between five and eight days.

Guinea-pigs inoculated with tuberculous pus exposed for five minutes to 220° F. remained well.

The foregoing results, as far as regards anthrax, are far more favourable to the efficacy of dry heat as a disinfecting agent than those of Koch. It appears that the spores of the bacillus anthracis lost their vitality, or at any rate their pathogenic quality, after exposure for four hours to a temperature a little over the boiling-point of water, or for one hour to a temperature of 245° F. Non-spore-bearing bacilli of anthrax and of swine fever were rendered inert by exposure for an hour to a temperature of 212°–218°, and even five minutes' exposure to this temperature sufficed to destroy the vitality of the former, and impair that of the latter.

As none of the infectious diseases for the extirpation of which measures of disinfection are in practice commonly required are known to depend upon the presence of bacilli in a spore-bearing condition, it is concluded that, as far as our present knowledge goes, their contagia are not likely to retain their activity after being heated for an hour to 220° F.

In the experiments with steam the results were conclusive as to the destructive power of steam at 212° F. upon all the contagia submitted to its action. In one instance only was there room for suspicion that the disinfection had not been complete: this was in the case of the highly-resisting anthrax spores, exposed to steam for five minutes only: the animal had six days afterwards a swelling at the seat of inoculation, but remained well. On the other hand the animals inoculated with unheated portions of the same materials all died.

These results are in accordance with those of Koch, Gaffky, and Löffler, and it may be considered established that the complete penetration of an object by steam heat for more than five minutes is sufficient for its thorough disinfection.

In view of the above satisfactory results it was not deemed necessary to make any experiments as to the disinfecting power of steam at higher temperatures or under pressure, its efficacy being taken for granted.

Dr. Klein found that boiling in water for only one minute was sufficient to render inert the spores of the bacillus anthracis, although it is known that some of the spore-bearing non-pathogenic bacilli are only destroyed by prolonged boiling, or by a moist temperature above the boiling-point.

Some observations were made on the destruction of lice by heat. It was found that the eggs of lice could be conveniently hatched by tying up tightly in muslin a small piece of the garment on which they were deposited, and carrying it about for a week or two in a warm pocket. Tested in this way no development was found to take place in eggs of lice which had been exposed for one hour to 300° F. dry heat, for one hour to 230 F. dry heat, or for ten minutes to steam at 212° F., or which had been boiled for five minutes in water. The maximum heat which lice or their eggs will bear with impunity was not ascertained.

In order to secure the thorough and certain disinfection by heat of porous articles likely to retain infection, such as clothing and bedding, it is necessary that the heat should be made to permeate the articles in every part to such a degree and for such a length of time as to destroy all infectious matter which they may contain.

It has been remarked that such articles as bedding and blankets are the highest outcomes of the ingenuity of man to check the passage of heat from one side of the object to the other. It is no wonder, therefore, that they should be found difficult of penetration by heat. Even thin layers, however, of badly conducting substances interpose a considerable barrier to the passage of dry heat. The following experiment was made to ascertain how far the inclosing of infective objects in blotting-paper or test-tubes plugged with cotton wool (as in Dr. Koch's experiments) hindered the full access of heat to them.

Two similar registering thermometers were taken: the bulb of one was tied up in a single layer of thin white blotting-paper, that of the other was placed in a test-tube ⅞ inch wide in such a manner as not to touch the sides, and a plug of white cotton wool 1 inch deep was pushed into the tube around the stem of the thermometer, but not as far as the bulb. Both the paper and cotton wool were previously dried. The two thermometers, together with another with bare bulb, were then hung up in a hot-air bath. Heat being applied, the thermometers were read half-hourly as follows:—

Time from lighting	Readings of thermometer with bulb: Bare ° F.	In paper ° F.	In tube ° F.
½ hour ...	162	147	151
1 hour ...	212	193	196
1½ hour ...	234	213	219
2 hours ...	242	236	238
2½ hours ...	244	244	244

The following experiment was made with a thermometer having the bulb covered with a single layer of blanket and placed in the hot-air bath already heated:—

Time from placing in hot-air bath	Thermometer with bulb bare ° F.	Thermometer with bulb in blanket ° F.
½ hour ...	246	231
1 hour ...	260	250
1½ hour ...	266	254
2 hours ...	268	263
2½ hours ...	268	264

Experiments made with larger articles and apparatus showed how difficult it was to secure the penetration of a dry heat sufficient for disinfection into the interior of such an object as a pillow. It was only effected by employing a high degree of heat, or by continuing the exposure during many hours, length of exposure compensating for a lower degree of heat. On the other hand heat in the form of steam penetrates much more rapidly than dry heat. Thus a thermometer in a roll of dry flannel placed in a hot-air bath at 212° F., at the end of an hour registered only 130° F. In the same roll, placed in the steam cylinder for ten minutes, the thermometer marked 212° F. Experiments on the large scale were equally conclusive. The causes of the superior penetrative power of heat in the form of steam over hot air appear to be:—

(1) The large amount of latent heat in steam, set free on its condensation. In hot dry air, on the other hand, the evaporation of hygroscopic moisture takes up heat and delays the attainment of the required temperature.

(2) Steam, on condensation into water, occupies but a very small fraction of its former volume and thus makes room for more. Hot air in cooling diminishes in volume in much less proportion.

(3) The heat evolved in the moistening of a dry porous substance. In the centre of a highly-dried roll of flannel placed in the cylinder in a current of steam at 212° F., a thermometer, after five minutes' exposure, registered 239° F.

(4) The higher specific heat of steam than of air.
(5) The greater diffusive power of steam than of air.
(6) The effects of pressure. By applying steam under pressure, relaxed and reapplied from time to time, so as to displace the cold air remaining in the interstices of the material, we have a means of considerably increasing the penetrative power of the steam.

In view of the superior efficacy of steam, both in the destruction of infective matters and in the penetration of badly-conducting materials, some experiments were made with moist air in the hope that it might be found possible to obtain the advantages of the use of steam without its drawbacks.

In these experiments either an evaporating vessel containing water was placed at the bottom of the hot-air chamber, or steam evolved in a separate boiler was led into the chamber by a pipe.

An attempt was made to measure the degree of humidity of the air by suspending in the chamber two maximum-registering thermometers arranged side by side, one of them having its bulb covered with gauze kept moist by dipping in a phial of water, as in the wet-and-dry-bulb arrangement employed by meteorologists. It appears, however, that there are no tables or formulæ in existence by which the degree of humidity of the air corresponding to a given difference between the wet and dry bulb thermometers at these high temperatures can be ascertained. The conditions in a heated chamber are so different from those met with in meteorological practice, that it is doubtful whether the relative humidity of the air could be obtained in this way with any great degree of accuracy; but a comparison of the readings of the wet and dry bulb thermometers was found in practice to be useful as a rough indication of the dryness or dampness of the air, although the readings could not be reduced to a common measure.

The experiments seem to show conclusively that moistening the air of the heated chamber diminishes the time necessary for the penetration of heat into a badly-conducting object. As examples the following observations may be quoted. They were made in an iron chamber heated by a furnace underneath, and furnished with a pipe by which steam could be admitted.

		No steam admitted	A small jet of steam admitted	Large jet of steam admitted
Maximum readings of thermometers hung up in chamber	Dry bulb	299° F.	299° F.	249° F.
	Wet bulb	146°	165°	190°
Temperature attained in centre of similar pillows exposed for one hour in heated chamber		136°	188°	209°

The moistening of the air of the heated chamber by either method was further found to have the advantage of rendering more equable the distribution of temperature in different parts of the chamber, thus tending to prevent scorching of the articles placed therein.

On the other hand it was not found that the presence of moisture in proportions such as these, or even greater, increased the disinfecting effect at the temperature employed; spores of the bacillus anthracis retained their vitality equally well in heated air whether it were moist or dry; thus they caused the death of a guinea-pig after exposure for an hour to a temperature of {dry bulb 220° F., wet bulb 190° F.} whereas five minutes' exposure to a current of steam at 212° F. was sufficient to render them inert.

To avoid risk of injury to articles subjected to disinfection by heat is an important practical question, not only on account of the value of the articles themselves, but also because, if the exposing of such articles to heat be attended with risk of injury, there is danger lest, to avoid this risk, they may not be sufficiently heated to insure disinfection. The following are the principal modes in which injury may occur; they are somewhat different in the case of steam from that of dry heat:—

1. Scorching or partial decomposition of organic substances by heat. In its incipient stages this manifests itself by changes of colour, changes of texture, and weakening of strength.
2. Overdrying, rendering materials brittle (by dry heat).
3. Fixing of stains, so that they will not wash out.
4. Melting of fusible substances, as wax and varnish, and ignition of matches accidentally left in pockets.
5. Alterations in colour, gloss, &c., of dyed and finished goods.
6. Shrinkage and felting together of woollen materials.
7. Wetting (by steam).

Scorching begins to occur at different temperatures with different materials, white wool being soonest affected. It is especially apt to occur where the heat is in the radiant form. To avoid risk of scorching the heat should not be allowed much to exceed 250° F., and even this temperature is too high for white woollen articles.

By a heat of 212° and upwards, whether dry or moist, many kinds of stains are fixed in fabrics so that they will not wash out. This is a serious obstacle in the way of the employment of heat for the disinfection previous to washing of linen, &c., soiled by the discharges of the sick.

Steam disinfection is inapplicable in the case of leather, or of articles that will not bear wetting. It causes a certain amount of shrinkage in textile materials, about as much as an ordinary washing. The wetting effect of the steam may be diminished by surrounding the chamber with a jacket containing steam at a higher pressure, so as to superheat the steam in the chamber.

For articles that will stand it, washing in boiling water (with due precautions against re-infection) may be relied on as an efficient means of disinfection It is necessary, however, that before boiling the grosser dirt should be removed by a preliminary soaking in cold water. This should be done before the linen leaves the infected place.

The objects for which disinfection by dry heat or steam is especially applicable are such as will not bear boiling in water, *e.g.*, bedding, blankets, carpets, and cloth clothes generally.

Apparatus for disinfection by heat may be classified as follows:—

(a) *By hot air*—
1. Apparatus in which the heat is applied to the outside o the chamber, and the products of combustion do not enter the interior.
2. Apparatus in which the heated products of combustion enter the interior.
3. Apparatus heated by steam or hot water circulating in closed pipes.
4. Apparatus in which air previously heated is blown into the chamber.

(b) *By steam*—
5. By a current of free steam.
6. By steam confined in a chamber at pressures above that of the atmosphere.

The most important requisites of a good apparatus for disinfection by heat are (*a*) that the temperature in the interior shall be uniformly distributed; (*b*) that it shall be capable of being maintained constant for the time during which the operation extends; and (*c*) that there shall be some trustworthy indication of the actual temperature of the interior at any given moment. Unless these conditions be fulfilled, there is risk, on the one hand, that articles exposed to heat may be scorched, or on the other hand, that through anxiety to avoid such an accident the opposite error may be incurred, and that the articles may not be sufficiently heated to insure their disinfection.

In dry-heat chambers the requirement (*a*) is often very far from being fulfilled, the temperature in different parts of the chamber varying sometimes by as much as 100°. This is especially the case in apparatus heated by the direct application of heat to the floor or sides of the chamber. The distribution of temperature is more uniform in proportion as the source of heat is removed from the chamber, so that the latter is heated by currents of hot air rather than by radiation.

There is a marked difference between the distribution of temperature in a chamber heated primarily by radiant heat and in one heated by the admission of hot air or steam. Radiant heat is most intense close to its source, diminishing rapidly as we recede therefrom. Also it does not turn corners, and thus objects lying behind others are screened from it, except so far as it may be reflected upon them from other surfaces. The rays strike the walls of the chamber and objects therein, so that these are more highly heated than the air, which becomes heated only secondarily by contact with them.

On the other hand, if air already heated, or steam, be admitted into a chamber, the temperature tends to equalise itself in the different parts, and the walls and solid contents of the chamber do not become hotter than the air.

In chambers heated by gas, when once the required temperature has been attained, but little attention is necessary to maintain it uniform, and in the best-made apparatus this is automatically

performed by a thermo-regulator. On the other hand, in apparatus heated by coal or coke the temperature continually tends to vary, and can only be maintained uniform by constant attention on the part of the stoker.

In very few hot-air chambers did the thermometer with which the apparatus was provided afford a trustworthy indication of the temperature of the interior; in some instances there was an error of as much as 100° F. This is due to the thermometer, for reasons of safety and accessibility, being placed in the coolest part of the chamber, and to the bulb being inclosed for protection in a metal tube which screens it from the full access of heat. The difficulty may be overcome by using, instead of a thermometer, a pyrometer actuated by a metal rod extending across the interior of the chamber.

In steam apparatus the three requirements above mentioned are all satisfactorily met, and for this reason, as well as on account of the greater rapidity and certainty of action of steam, both in penetrating badly conductive materials and in destroying contagia, steam chambers are, in Dr. Parsons's opinion, greatly preferable to those in which dry heat is employed.

It is important that the arrangements of the apparatus, the method of working, and the mode of conveyance to and fro, should be such as to obviate risk of articles which have been submitted to disinfection coming into contact with others which are infected.

The latter part of the Report is taken up with descriptions of the various forms of apparatus in use for disinfection by heat, and accounts of experiments made with a view to test their practical efficiency.

ON THE FRACTIONATION OF YTTRIA[1]

HAVING already explained the methods of chemical fractionation, it may be useful now to describe some of the results yielded by an extended perseverance in these operations.

I must, in the first place, explain that my work has been confined to a limited and very rare group of bodies—the earthy bases contained in such minerals as samarskite, gadolinite, &c. These have been repeatedly put through the fractionation mill by other chemists, but the results have been most unsatisfactory and contradictory, no sufficiently good test being known whereby the singleness of any earth got out by fractionation could be decided, except the somewhat untrustworthy one of the atomic weight. I say *untrustworthy*, because it is now known that fractionation, unless it is pushed far beyond the point to which some Continental chemists have even carried it, is quite as liable to give *mixtures* which refuse to split up under further treatment of the same kind, as it is to yield a chemically simple body. This I have fully gone into in my paper "On the Methods of Chemical Fractionation." The unsatisfactory nature of fractionation work may be seen from expressions used, in private letters to me, by some of the eminent chemists who have almost made this method their own. One writes—"It is very tiresome working with the rare earths, as we never can be sure when we have got a definite result. There will never be an end to their history. I am very tired of it, and am much inclined to give it up." Another writes—"Unfortunately I commenced my researches on the rare earths with too little material, and I have not had the courage, at my age, to recommence the work on more abundant material. The further I advance in my work the more I am convinced that no known method permits of the complete separation of these different earths one from the other." A third writes—"One loses so much material in the separations that it appears to me scarcely possible, with the material available, to arrive at a successful solution of the question." I could multiply similar quotations, all breathing the same almost despairing spirit.

It would certainly not have been prudent on my part to invite a time-honoured comparison, and "rush in" where so many eminent men "fear to tread," were it not that good fortune had placed in my hands a physical test for these obscure molecular groupings which is of the most exquisite sensitiveness. I refer to what I have for shortness called the Radiant-Matter test.

It is well known that a limited group of these rare earths, when phosphoresced *in vacuo*, yield discontinuous spectra. The method adopted to bring out the spectra is to treat the substance under examination with strong sulphuric acid, drive off excess of acid by heat, and finally to raise the temperature to dull redness.

[1] A Paper read before Section B of the British Association at the Birmingham meeting, by William Crookes, F.R.S., V.P.C.S

It is then put into a radiant-matter tube of the form shown in Fig. 1, and the induction spark is passed through it after the exhaustion has been pushed to the required degree. The phosphorescence occurs beneath the negative pole. As each gaseous molecule, carrying its charge of negative electricity with it, strikes the earthy sulphate, it has a tendency to part with its charge, provided it finds a body ready to take up the electricity; otherwise it retains its charge. Bodies like yttrium sulphate, &c., easily take the electric charge, and under the stimulus phosphoresce, emitting light whose waves tend to collect round definite centres of length. The phosphorescent light which the discharge evokes is best seen in a spectroscope of low dispersion, and with not too narrow a slit. In appearance the bands are more analogous to the absorption-bands seen in solutions of didymium than to the lines given by spark spectra. Examined with a high magnifying power, all appearance of sharpness generally disappears: the scale measurements must therefore be looked upon as approximate only; the centre of each band may be taken as accurately determined within the unavoidable errors of experiment, but it is impossible to define their edges with much precision. The bands are seen much sharper when the current first passes than after the current has been passing for some time and the earth has become hot. On cooling, the sharpness of the bands re-appears.

As a general rule, the purer the earth the sharper the band, and when impurities are removed to the utmost extent, the sharpness is such as to deserve the name of a line. This may be illustrated by mixing together yttria and lime. Lime phosphoresces with a continuous and yttria with a discontinuous spectrum. Mixed together, the phosphorescing energy of the lime does not spend itself over the whole spectrum, but concen-

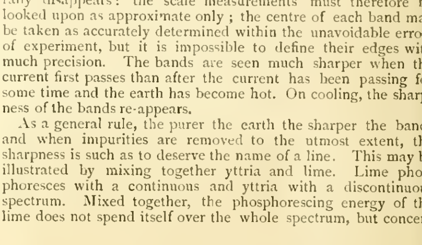


FIG. 1.

trates itself in greatly reinforcing the yttria bands. A molecule of yttria vibrating with a definite wave-length gives a nearly sharp line, but the molecule of lime with which it is weighted has no special tendency to vibrate to one wave-length more than another. The yttria induces the right vibration in the adjacent molecule of lime; but this lime, once set in vibration, cannot confine itself to the exact wave-length required, and overflows a little on each side, and the result is a widening and blurring of the bands, becoming greater in amount as the extraneous earth increases in quantity.

To this rule one exception occurs. The body which I have named Sδ, or 609, is remarkable for the great sharpness of its phosphorescent line, and I have noticed scarcely any variation in its sharpness, however large the bulk of extraneous earth associated with it. This line, however, is sharper and brighter when the current is first turned on than it is after the earth has been phosphorescing for a minute or so.

In the Bakerian lecture on yttrium delivered before the Royal Society (*Phil. Trans.* Part 3, 1883), I described the phosphorescent spectrum given by this element, and in the address which I have had the honour of delivering before this Section I gave a drawing of the spectrum of yttrium, together with a sketch of the train of reasoning by which I had been led to the opinion that excessive and systematic fractionation had split up this stable molecular group into its components, distributing its atoms into several groups, with different phosphorescent spectra.

No longer than twelve months ago the name yttria conveyed a perfectly definite meaning to all chemists. It meant the oxide of the elementary body yttrium. I have in my possession specimens of yttria from M. de Marignac (considered by him to be purer than any chemist had hitherto obtained), from M. Clève (called by him "purissimum"), from M. de Boisbaudran (a sample of which is described by this eminent chemist as "scarcely

soiled by traces of other earths"), and also many specimens prepared by myself at different times and purified up to the highest degree known at the time of preparation. Practically these earths are all the same thing, and up to a year ago every living chemist would have described them as identical, *i.e.* as the oxide of the element yttrium. They are almost indistinguishable one from the other both physically and chemically, and they give the phosphorescent spectra *in vacuo* with extraordinary brilliancy. This is what I formerly called yttria, and have more recently called *old* yttria Now these constituents of old yttrium are not *impurities* in yttrium any more than praseodymium and neodymium (assuming them really to be elementary) would be impurities in didymium. They constitute a veritable splitting up of the yttrium molecule into its constituents.

The plan adopted in the fractionation of yttria does not differ in principle from the methods described in my former paper "On the Methods of Chemical Fractionation." Dilute ammonia is added to a very dilute solution of the earth in only sufficient quantity to precipitate one half. After standing for several hours the precipitate is filtered. After each fractioning the filtrate is passed to the left and the precipitate to the right, and the operations are continued many thousand times.

The diagram (Fig. 2) shows the scheme clearly, with the direction the precipitates and solutions travel. Limited space, even on a large diagram, prevents me from giving more than a few operations, but they will be sufficient to satisfy you that enormous patience, a large amount of material, and a not insignificant number of bottles, are requisites for successful fractionation. Such proceedings are tedious enough even in their narration, but no mere words can enable any one to realise the wearisome character of these operations when repeated day by day, month after month, on long rows of Winchester quart bottles.

After a certain time, on examining the series of earths in the lowest line of bottles, their phosphorescent spectra are found to alter in the relative intensities of some of the lines, and ultimately different portions of the fractionated earths show spectra such as I have endeavoured to illustrate at the foot of the diagram (Fig. 2), where I give the spectra of five components of yttrium.

The final result to which I have come is that there are certainly five, and probably eight, constituents into which yttrium may be split. Taking the constituents in order of approximate basicity (the chemical analogue of refrangibility), the lowest earthy constituent gives a deep blue band, Gα (λ 482); then there is a strong citron band, Gδ (λ 574), which has increased in sharpness

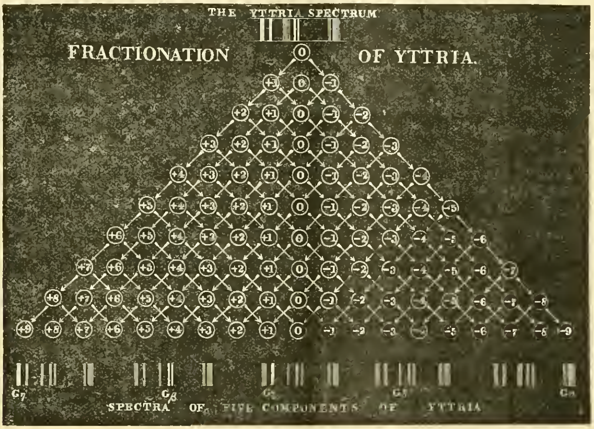


FIG. 2.

till it deserves to be called a line; then come a close pair of greenish-blue lines, Gβ (λ 549 and λ 541, mean 545); then a red band, Gζ (λ 619), then a deep red band, Gη (λ 647), next a yellow band, Gε (λ 597), then another green band, Gγ (λ 564); this (in samarskite and cerite yttria) is followed by the orange line Sδ (λ 609). The samarium bands remain at the highest part of the series. These, I am satisfied, are also separable, although for the present I have scarcely touched them, having my hands fully occupied with the more easily resolvable earths. The yellow band, Gε, and green band, Gγ, may in fact be due to a splitting up of samarium.

Until we know more about these bodies I refrain from naming them, but will designate them provisionally by the mean wavelength of the dominant band. If, however, for the sake of easier discussion among chemists a definite name is thought to be more convenient, I will follow the plan frequently adopted in such cases, and provisionally name these bodies as shown in the table given below.

The initial letters S and G recall the origin of the earths respectively from Samarskite and Gadolinite.

Not only has yttrium been split up by subjection to fractionation, but samarium, as I have hinted above, is likely to prove equally unable to resist this operation. In the phosphorescent spectrum of samarium sulphate the line Sδ (609) is one of the constituents. When yttria is added to samaria this line is developed in greater intensity, as yttria has the power of deadening the other bands of samarium, while it does not seem to affect the

Position of lines in the spectrum	Scale of spectroscope	Mean wave-length of line or band	$\frac{1}{\lambda^2}$	Provisional name	Probability
Bright lines in—					
Deep blue ...	8·931	482	4304	Gα	New
Greenish-blue (mean of a close pair...)	9·650	545	3367	Gβ	New, or the Zβ of M. de Boisbaudran
Green........ ..	9·812	564	3144	Gγ	New
Citron	9·890	574	3035	Gδ	New, or the Zα of M. de Boisbaudran
Yellow.........	10·050	597	2806	Gε	New
Orange..	10·129	609	2693	Sδ	New
Red	10·185	619	2611	Gζ	New
Deep red......	10·338	647	2389	Gη	New

line Sδ. Several circumstances, however, tend to show that although line Sδ accompanies samarium with the utmost per-

tinacity, it is not so integral a part of its spectrum as the other red, green, and orange lines. For instance, the chemical as well as physical behaviour of these line-forming bodies is different. On closely comparing the spectra of specimens of samaria from different sources, line Sδ varies much in intensity, in some cases being strong and in others almost absent; the addition of yttria is found greatly to deaden the red, orange, and green lines of samarium, while yttria has little or no effect on the line Sδ; again, a little lime entirely suppresses line Sδ, while it brings out the samarium lines with increased vigour. Finally, attempts to separate line Sδ from samarium and those portions of the samarskite earths in which it chiefly concentrates has resulted in sufficient success to show me that, given time enough, and an almost inexhaustible supply of material, a separation would not be difficult. These facts, together with the peculiar behaviour of the lines Gε and Gγ, strengthen my suspicion as to the resolvability of samarium.

Samaria giving the line Sδ had been prepared from cerite and samarskite. Many observations had led me to think that the proportion of band-forming constituents varied slightly in the same earth from different minerals. Amongst others, gadolinite showed indications of such a differentiation, and therefore I continued the work on this mineral. Very few fractionations were necessary to show that the body giving line Sδ was not present in the gadolinite earths; no admixture of yttria and samaria from this source giving a trace of it. It follows, therefore, that the body whose phosphorescent spectrum gives line Sδ occurs in samarskite and cerite, but not in gadolinite.

It now became an interesting inquiry whether all these constituents of yttrium were united together in exactly the same proportion in every case. A glance at the diagram will show that yttrias from different sources, although they may be alike as far as our coarser chemical tests are concerned, are not built up exactly in the same manner. Thus, when the samarskite yttrium was forming, all the constituent molecules—which I have provisionally named Gα, Gβ, Gγ, Gδ, Gε, Gζ, Gη, and Sδ—condensed together in fair proportion. In gadolinite yttrium the constituents Gβ and Gδ are plentiful, Gζ is very deficient, Sδ is absent, and the others occur in moderate quantities. In the yttrium from xenotime Gδ is most plentiful, Gβ occurs in smaller proportion, Gζ is all but absent, and Sδ is quite absent. Yttrium from monazite contains Gβ and Gδ, with a fair proportion of the other constituents, Gβ is plentiful and the red is good. Yttrium from fluocerite is very similar to that from monazite, but Gα is weaker. Yttrium from hielmite is very rich in Gδ, has a fair quantity of Gα and Gβ, less of Gγ, no Sδ, and only a very faint trace of Gη. Yttrium from euxenite is almost identical with that from hielmite. Yttrium from cerite contains most Gζ and Gδ, less Gα and Gβ, only a trace of Gη, and a fair proportion of Sδ.

I have already mentioned how the key to these explanations was gained by an examination of the phosphorescent spectrum of M. de Marignac's Yα (now called by him gadolinium).

Referring to the diagram, it is seen that Yα is composed of the following band-forming bodies:—Gβ, Sδ, Gζ, together with a little samarium. Calling the samarium an impurity, it is thus seen that gadolinium is composed of at least three simpler bodies.

It is by a method of his own, differing from mine, that M. de Boisbaudran has obtained phosphorescent spectra of some of the rare earths. He takes the induction-spark between the surface of a strong and acid solution of the metallic chloride and a clean platinum wire a few millims. above it. The platinum wire is kept negative and the solution positive; it is then observed that in many cases a thin layer of fluorescent light is seen at the surface of the liquid. This layer gives a spectrum of nebulous bands. For the sake of brevity I will adopt M. de Boisbaudran's term, and call this process the *method of reversion* (the direction of the spark being reversed). As this method is entirely different to the one I adopt, it is not surprising that the results are also different. Experimenting in this way M. de Boisbaudran has obtained, among others, two bands (λ 573 and λ 543·2), which he considers are caused by two elements, named respectively Zα and Zβ, and which he considers new, at all events if we except terbium and possibly the elements of what was formerly called holmium. His method fails to show any spectrum in solutions of yttria which by my method give the yttria bands with the greatest brilliancy; while conversely his method shows a fluorescent spectrum in solutions of earths separated as widely as possible from yttria, chemically as well as spectroscopically. My experiments on both these methods tend to the conclusion that our bands are not due to the same cause, although M. de Boisbaudran's experiments have led him to the opposite conclusion. The band of Zβ (543) falls between the double green band Gβ, and the band of Zα (573) would come very near the citron line Gδ.

In the hands of a practised experimentalist like M. de Boisbaudran this method may give trustworthy indications, but I must confess that in my opinion the test is one beyond the range of practical analysis, owing to the enormous difficulty of getting the phenomena described by the discoverer. Unless the strength of spark, the concentration and acidity of solution, and the dispersion and magnifying power of the spectroscope bear a certain ratio one to the other, the observer is likely to fail in seeing a spectrum even in solutions of earths which contain considerable quantities of Zα and Zβ. In my own case I not only have had the advantage of personal instruction in Paris from M. de Boisbaudran himself in the best method of getting these reversion spectra, but on returning to London I brought with me some of the identical earths which give these spectra at their best. In spite of these advantages I have sometimes experimented off and on for weeks without being able to see more than a feeble glimmer of the bands described by M. de Boisbaudran.

Again, when everything is most favourable and the reversion bands are at their strongest, they are but a faint and hazy shadow of the brilliant lines given by the bombardment process. M. de Boisbaudran, speaking of the relative sensitiveness of our two methods, says that the bombardment process *in vacuo* is incomparably more delicate than his reversion test, and I estimate the relative sensitiveness of the two methods to be in the proportion of about 1 to 100.

You have probably anticipated in your minds a question which is likely to occur at this point of the inquiry. If such results have been obtained by submitting yttrium to this novel method of analysis, what will be the result of fractionating some other reputed element?

Yttrium, as I have explained, is an exceedingly stable molecular group, capable of acting as an element, just as calcium, for instance, acts as an element: to split up yttrium requires not only enormous time and material, but the existence of a test by means of which the constituents of yttrium are capable of recognition. Had we tests as delicate for the constituent molecular groups of calcium, this also might be resolved into simpler groupings. It is one thing, however, to find out means of separating bodies which we know to be distinct and have colour or spectrum reactions to guide us at every step; it is quite another thing to separate colourless bodies which are almost identical both in chemical reaction and atomic weight, especially if we have no suspicion that the body we are dealing with is a mixture.

(I mention calcium because it is one of several other elements which I have put through the fractionation mill. Many hundred operations have given me just sufficient encouragement to make me wish I had time to push this work to the end.)

One of the chief difficulties in the successful carrying out of an investigation in radiant-matter spectroscopy is the extraordinary delicacy of the test. This extreme sensitiveness is a drawback rather than a help. To the inexperienced eye 1 part of yttrium in 10,000 gives as good an indication as 1 part in 10, and by far the greater part of the chemical work undertaken in my hunt for spectrum-forming elements was performed upon material which later knowledge shows did not contain sufficient to respond to any known chemical test. It is as if the element sodium were to occur in ponderable quantity only in a few rare minerals seldom seen out of the collector's cabinet. With only the yellow line to guide, and seeing the brilliancy with which an imponderable trace of sodium in a mineral declares its presence in the spectrum, I venture to think that a chemist would have about as stiff a hunt before he caught his yellow line as I have had to bring my orange and citron bands to earth.

Chemistry, except in few instances, as water-analysis and the detection of poisons, where necessity has stimulated minute research, takes little account of "traces," and when an analysis adds up to 99·999, the odd 0·001 per cent. is conveniently put down to "impurities," "loss," or "errors of analysis." When, however, the 99·999 per cent. constitutes the impurity, and this exiguous 0·001 is the precious material to be extracted, and when, moreover, its chemistry is absolutely unknown, the difficulties of the problem become enormously enhanced. Insolubility as ordinarily understood is a fiction, and separation by preci

pitants is nearly impossible. A new chemistry has to be slowly built up, taking for data uncertain and deceptive indications, marred by the interfering power of mass in withdrawing soluble salts from a solution, and the solubility of nearly all precipitates when present in traces in water or in ammoniacal salts. What is here meant by "traces" will be better under tood if I give an instance. After fifteen months' work I obtained the earth yttria in a state which most chemists would call absolutely pure, for it contained not more than 1 part of impurity (samaria) in 250,000 parts of yttria. But this one part in a quarter of a million profoundly altered the character of yttria from a radiant-matter-spectroscopic point of view, and the persistence of this very minute quantity of interfering impurity entailed another ten months' extra labour to eliminate these final "traces," and to ascertain the real reaction of yttria pure and simple.

The radiant-matter test applied to these phosphorescing bodies proves itself to be every day more and more valuable, and one of the most far-searching and trustworthy tools ever placed in the hands of the experimental chemist. It is an exquisitely delicate test, capable of being applied to bodies which have been approximately separated, but not yet completely isolated, by chemical means; its delicacy is unsurpassed even in the region of spectrum analysis; its economy is great, inasmuch as the test involves no destruction of material; and its convenience is such that any given specimen is always available for future reference. Likewise, the quantity of material is limited solely by the power of the human eye to see the body under examination. Beyond all these excellences is its trustworthiness. I should perhaps exceed the legitimate inference from experience were I to claim that this test is infallible; but this I may say—during the five years in which the test has been in daily use in my laboratory, I never once have been led to view its indications with suspicion. Anomalies and apparent contradictions have cropped up in plenty; but a little more experiment has always shown that the anomalies were but finger-posts pointing to fresh paths of discovery, and the contradictions were due to my own erroneous interpretation of the facts before me.

SCIENTIFIC SERIALS

Rendiconti del Reale Istituto Lombardo, July.—On some new substituted derivatives of benzine, by E. G. Körner. In order to complete the still defective aromatic series, the author has prepared a number of these derivatives, studying them in connection with the relative isomerous compounds. The list includes a hydrochlorate, $(B)HCl, H_2O$; a sulphate, $(B)2H_2SO_4$; orthoiodacetanilide. $C_6H_4I . NHC_2H_3O$; and nitro-orthobiiodobenzine, probably $C_6H_2 . I . I . H . NO_2$.—On the effects of the sulphate of copper against the parasites of the grape-vine, by Prof. Gaetano Cantani. It is shown that this remedy, which has already been successfully tried in France, should also be introduced in Italy, if not to supersede, at least jointly with, the milk of lime.—Chemical and experimental researches on human milk, by Prof. G. Sormani and T. Gigli. It appears from the authors' experiments that a mixed or normal diet yields far better results than an exclusively animal or vegetarian *régime*.—Meteorological observations made at the Brera Observatory, Milan, during the month of July.

Botanische Jahrbücher, von A. Engler, Siebenter Band, Heft iv.—Contributions to the morphology and classification of the Cyperaceæ, by Dr. F. Pax. The author regards the Cyperaceæ as reduced types of a series which is more advanced phylogenetically than the Juncaceæ. As regards their relations to the Gramineæ, he concludes that the affinity is not so direct that the one family could be derived from the other.—On the flower and inflorescence of the Centrolepidaceæ, by Prof. Dr. G. Hieronymus.—Contributions to the flora of the Cameroons, by A. Engler. A list of plants collected by Dr. Buchholz in the Cameroons in 1874, with descriptions of the new species.—On the origin of the weeds on arable land and waste places in Germany, part 1, by F. Hellwig.—Abstracts of important papers.

Heft v. opens with part 2 of the above paper by Dr. Hellwig. The first part is chiefly occupied with the general consideration of the subject, and lists of the plants in question; while the second contains a detailed account of the origin of the plants named in the foregoing lists.—The orchids collected by Dr. Naumann on the expedition of H.M.S. *Gazelle*, by F. Kränzlin. The volume closes with a valuable list of works published during 1885, on classificatory botany, &c. This, together with the frequent analyses of the more important of those papers which are published in languages not usually familiar to ordinary readers, greatly enhances the value of Dr. Engler's excellent serial.

Bericht über die Thätigkeit der botanischen Section der Schlesischen Gesellschaft, 1885, compiled by Prof. Dr. F. Cohn.—The Botanical Section of the Society held nine meetings during the year 1885, at which the following original papers were read:—Dr. Engler, on the vegetation of the German possessions in South Africa.—Dr. Pax, on the genus Acer.—Herr Limpricht, on the formation of pores in the cortex of the Sphagna.—Dr. Eidam, on an Entomophthoraceous fungus found on frogs' dung.—Dr. Schröter, on the mycological results of a journey to Norway.—Dr. Pax, on the morphology and classification of the Cyperaceæ.—Dr. Engler, on the family of the Typhaceæ.—The report closes with a statement of the results of the investigation of the Phanerogamic flora of the district in 1885, arranged by R. von Uechtritz.

Beiträge zur Biologie der Pflanzen, von Dr. F. Cohn, Vierter Band, Zweites Heft.—Investigations on the tendrils of the *Cucurbitaceæ*, by Dr. Otto Müller, of Breslau (3 plates). The author concludes, chiefly on anatomical grounds, that the irritable part of the tendril of the Cucurbitaceæ is of foliar nature.—Investigations of the *Flagellatæ*, by Dr. Arthur Seligo (1 plate).—*Basidiobolus*, a new genus of the *Entomophthoraceæ*, by Dr. Ed. Eidam (4 plates). The author regards the resting spores of this genus as true zygospores, though the gametes are of unequal size, and expresses the opinion that the *Entomophthoreæ* find their natural place in the *Zygomycetes*, as directly related to the *Mucorini*.

SOCIETIES AND ACADEMIES

SYDNEY

Royal Society of New South Wales, August 4.—Ch. Rolleston, President, in the chair.—The Society's Medal and Prize of 25*l*. was presented to Mr. S. Herbert Cox, F.C.S., F.G.S., for his prize essay on "The Tin Deposits of New South Wales." The principal deposits occur in New England as impregnations, segregation veins, and lodes in granite, also as gash veins in Silurian slates, and as a network of veins or stockwork in haplite. The granitic eruption occurred not later than Carboniferous times, and no sedimentary strata appear to have been deposited until the Tertiary period, when the leads of alluvial tin were formed, together with their associated gravels. Denudation on an enormous scale has gone on, and the Silurian slates which rest on the granites have only been preserved as outlying patches included in folds in the granite. Dykes of feldspar and quartz porphyry traverse both the granite and slates, but the date of this eruption is probably Tertiary, although evidence appears to point out that this acidic only preceded the ensuing basaltic eruption by a short time. The more fluid basalt flowed for considerable distances, frequently burying the gravels of the river-beds with the tin they contained, and preserving these "deep leads" from subsequent denudation. True lodes appear to be rare, but some remarkable impregnated areas exist in greisen; "segregation" veins of small size are found in the granite, and in the slate "gash" veins up to 4 inches in width occur, but these are certainly not true lodes. Fortunately, wolfram occurs in separate veins from the tin; copper and iron pyrites, fluor-spar, tourmaline, white mica, and topaz are common; beryl forms a rock with quartz, through which tinstone is impregnated. In the alluvial deposits, tinstone is found associated with diamonds, sapphires, zircons, &c. The greater quantity of the tinstone hitherto raised has been from the alluvial, and the "deep leads" which are still being worked, and will probably be greatly developed in the future, closely correspond in their course with the shallow ones. They are worked to depths of 140 to 180 feet, and are frequently found below solid floes of basalt. Very good crushing and smelting plants have been erected, and although the conditions of the district vary greatly in different parts, it may be taken as certain that a yield of 5 per cent. tin in lodes, and from $\frac{1}{2}$ to 1 cwt. per cubic yard in deep alluvial deposits, pays for extraction. The total output of tin between 1872 and 1883 is 64,794 tons of ingots and 13·268 tons of black tin.—A paper by the late Rev. P.

MacPherson, M.A , was also read, on the aboriginal names of rivers in Australia philologically examined.

PARIS

Academy of Sciences, October 27.—M. Jurien de la Gravière, President, in the chair.—On Dr. Spœrer's views regarding the solar spots and protuberances, by M. Faye. In a paper recently contributed to the *Proceedings* of the German Astronomical Society, M. Spœrer adopts the view that the faculæ and spots are due to the currents of hydrogen which forms the solar chromosphere. But to the ascending currents, the cause of which is unexplained, is attributed a descending current which, by penetrating amid the faculæ to the body of the sun, gives rise to a spot. The hydrogen thus drawn in reascends round about the funnel of the spot, and, by mingling with the ascending currents, effects a complete circulation. The author points out that these ideas are completely analogous to his own, and would be identical, had M. Spœrer studied the mechanical cause of this remarkable circulation, which is here attributed to the irregular velocities of the horizontal currents producing on the solar surface gyratory movements with a vertical descending axis like those of the terrestrial streams and atmosphere.—A comparative study of the actions of walking and running, together with the mechanism of the transition between these two movements, by MM. Marey and Demeny. In this paper, which complements the author's previous communications on animal kinematics, numerous differences are shown to exist between slow and rapid pace, the latter being characterised by moments of complete detachment from the ground and by other equally important features scarcely visible to the naked eye, but which are now clearly revealed by the chronophotographic and dynamographic processes. The paper is furnished with six diagrams illustrating the contrasts between both motions and the transitions from one to the other.—Considerations on the nervous system of the gastropods, by M. H. de Lacaze-Duthiers. In supplement to previous papers on several aberrant types of gasteropods, the author here continues his analysis of the facts connected with the central nervous system of these organisms. Special care is taken to distinguish between the groups of ganglia of primary importance from others which, notwithstanding their size and numbers, really play only a secondary part in the nervous system of the gasteropods.—Wheat culture at Wardrecques, Pas-de-Calais, and at Blasinghem, Department du Nord, in 1886, by MM. Porion and Dehérain. In continuation of previous reports of the results of experiments carried on for many years in the north-west of France, the authors here announced that the most profitable varieties of wheat are those which, besides yielding the largest returns, are best able to support strong manures without lodging. Preference above all is given to the square-eared variety (*blé à épi carré*), which they hope may be brought into general use in order to meet the growing competition of foreign growers.—Observations of Finlay's comet made at the Lyons Observatory (Brunner equatorial 0·16m.), by M. Gonnessiat.—Observations of the same comet made at the Observatory of Nice (Gautier equatorial), by M. Perrotin.—Note on the errors of division in Gambey's mural circle, by M. Périgaud. These errors being once clearly determined, the author considers that the Gambey circle with the new mercury bath allowing a continuous observation of the Nadir, may be advantageously used in astronomic researches where great precision is required.—On a question concerning the single points of plane algebraic curves, by M. E. B. Guccia.—On the glycerinate of soda, by M. de Forcrand. In this paper the author completes the study of the glycerinate of soda, begun by E. Letts in 1872, and subsequently prosecuted by M. Berthelot.—On the preparation of the sulphur of calcium with violet phosphorescences, by M. A. Verneuil. By the application of the principles laid down by M. E. Becquerel in his researches on phosphorescence, the author has succeeded in effecting the synthesis of this substance, which has been long known in commerce, but the preparation of which had hitherto remained a secret.—On the comparative volatility of the methylic compounds in the various families of the negative elements, by M. Louis Henry. In this paper the author restricts his inquiries to the monocarbonic derivatives, and more especially to the methylic derivatives. He finds that, at equal atomic weight, the diminution of volatility determined in methane by the substitution of a negative element for hydrogen, is all the greater the more this element is removed from hydrogen.—Law determining the position of the embryo in insects, by M. Paul Hallez. From his studies of *Hydrophilus piceus* and *Locusta viridissima*, the author arrives at a general law applicable to insects and probably also to other classes, which he thus formulates:—The cellule ovum is disposed in the same direction as the maternal organism, with a cephalic and a caudal pole, a right and a left side, a dorsal and ventral face coinciding with the corresponding faces of the embryo.—Contributions to the natural history of the Orthonectidæ, by M. R. Kœhler. During his researches on *Amphiura squamata* at the Zoological Laboratory at Cette, the author has found on these animals both male and female of the curious parasite, Rhopalura, already studied by Giard and Julin.—On the exhalations of carbonic acid in infectious diseases determined by aërial and non-aërial microbes, by M. S. Arloing.—Geological constitution of the district of Croix-Rousse (Lyons), by M. Fontannes. The tunnel 2400 metres long now in progress under the terrace between the Rhone and the Saone at Lyons has afforded an opportunity of studying the geological features of the district, which appears to consist mainly of Pliocene sands overlying gneiss with remains of *Mastodon arvernensis*, above which follow Pliocene alluvia with *Elephas meridionalis*, Quaternary alluvia, and Glacial deposits (moraines, loam, &c.).

BOOKS AND PAMPHLETS RECEIVED

"Food-Grains of India," by A. H. Church (Chapman and Hall).—"Electricity in the Service of Man," by Wormell and Perry (Cassells).—"Handbook of Acoustics," by T. F. Harris (Curwen).—"Beobachtungen der Russischen Polarstation an der Lenamündung," ii. Theil. Meteorologische Beobachtungen, by A. Eigner.—"Geometrical Drawing for Army Candidates," by H. T. Lilley (Cassells).—"The Gas Engine," by D. Clerk (Longmans).—"General Biology," by W. T. Sedgwick and E. B. Wilson (Holt and Co., New York).—"The Encyclopædic Dictionary," vol. v. part ii. (Cassells).—"Loisette's Art of Never Forgetting Compared with Mnemonics," by F. Appleby.—"Modern Petrography," by G. H. Williams (Heath and Co., New York).

CONTENTS

THURSDAY, OCTOBER 21, 1886

OUR GUNS

WHENEVER attempts are made to manage scientific matters by means of committees failure is sure to result. Some of our Continental friends, the French especially, are fond of working in pairs, and excellent results have been arrived at by such means, but the method does not appear suitable to the English intellect, hence all our great achievements in science have been attained by single individuals. As soon as a committee gets to work, darkness seems to fall over the intellects of its members, and not only are the most absurd blunders perpetrated, and errors of judgment committed, but they are persevered in long after outsiders have detected and exposed them. We need only mention the Longridge wire-gun and the Moncreiff carriage as instances of the defective judgment which kept back the introduction of these useful and original inventions for some thirty years. But the blindness of the Ordnance Committee, or of the Unknown Being who is responsible for our guns, is still more curious and distressing with reference to the strains which guns must be constructed to withstand. We look in vain for any information on this head from the recent reports of committees or from the lectures of their inspired representatives, while such information as we have reveals the fact that, at the time when our new breech-loaders were designed, the knowledge of pressures which the Unknown Designers had was absolutely erroneous, and that the errors were of so elementary a character that it requires no special knowledge of the subject to detect them. If the indicator-diagrams of a steam-engine, and a statement of the work performed by it, were laid before, at any rate, the two civil members of the Ordnance Committee, with a request that the pressures in the cylinder should be investigated with a view to ascertaining whether they were correct, these gentlemen would, at once, compare the indicated power with the work done, and if the former were less than the latter they would, without hesitation, declare that the pressures said to have prevailed in the cylinder were too low. Now, attached to the official drawing of the first 10-inch breech-loading gun was a pressure-curve purporting to represent one-fourth the bursting pressure when firing a projectile of 500 lbs. weight, and imparting to it a muzzle velocity of 2100 feet per second. The mean pressure, measured from this curve, is 8·8 tons per square inch, the travel of the shot in the bore is 22 feet, hence the work done by the powder would be 15,205 foot-tons. The muzzle energy of the shot is a little more, namely, 15,284 foot-tons. But besides the energy communicated to the shot, the 300 lbs. weight of powder gases have to be set in motion, the friction of the gas checks has to be overcome, rotation has to be imparted to the shot, the atmosphere has to be displaced, and the aggregate of this work can be shown to amount to at least one-third of that required to drive out the shot, a fact which the pressures recorded in the experiments made with the *Thunderer* gun clearly proved. Colonel Maitland, in his lecture on our new guns at the United Service Institution in June 1884, rightly pointed out that the area bounded by the pressure-curve represented the work done in the bore; how is it, then, that it never occurred to any one on the Ordnance Committee to compare the work done with the effect produced? Such a comparison would have shown 20,400 foot-tons of work done by an indicator-diagram measuring only 15,205 foot-tons! The check pointed out should have been applied as a matter of common prudence, because it is notorious that then, as now, our direct knowledge of the pressure of powder gases in the bores of guns was very limited.

We are aware that it has been explained that the pressure-curve we have been discussing does not represent powder-pressures, but pressures one-fourth of those which would burst the gun; but such an explanation does not mend matters, for it reduces the factor of safety of the gun—already assumed at the dangerously low limit of four—to three, which no one, surely, would contend to be sufficient!

We are obliged to revert to the question, Why has not an actual pressure-curve been made public, and why did not the *Collingwood* Committee commence their report by showing that the 12-inch 43-ton gun was designed of sufficient strength? The answer, we are afraid, is that our gun-builders do not know what pressures they have to contend against, that our guns are being made by rule of thumb; in fact, their proportions are slowly arrived at by the costly method of trial and error.

Again, Colonel Maitland, in his lecture, gives a diagram showing, graphically, the pressure resulting from firing quick-burning, medium, and slow-burning powders. We must assume, the curves being given for the purpose of comparison, that they represent the effects of the same weights of powder burned under exactly similar conditions; therefore the areas of the figures bounded by the curves, as Colonel Maitland tells us, represent the work done in the gun, and measuring the areas of each curve up to only 14 calibres' length of bore we find that the slow-burning powder does more than twice the work of the medium-burning, and two and a half times that of the quick-burning powder! Surely Colonel Maitland would not wish us to draw such conclusions; and yet they are necessarily deduced from a diagram which must have represented the views of the Ordnance Committee only two years ago, and after Capt. Noble had shown, in his admirable lecture on "The Heat-Action of Explosives," delivered at the Institution of Civil Engineers, that the potential energy of all powders was very nearly the same; a view recently indorsed by Sir W. Armstrong, who stated that rather more of the slowest-burning powder was required to produce a given ballistic effect. We venture to say that, had the questions in review been under the control of a single, competent, fully-responsible man, the anomalies which we have pointed out could never have arisen, the aid of practical mathematicians would have been invoked, and the warnings of Sir W. Armstrong and others would not have been disregarded.

The public, no doubt, is weary of the constant discussion of past blunders, and the repeated allusions to them would be unjustifiable were it not for the vital bearing which these have on the future, and a most valuable result will be obtained if our efforts, and those of other writers, should be the means of destroying our

absurd system of administration altogether, or at any rate of dispelling the fatal complacency in which the Ordnance Department wraps itself, and which found a voice when Colonel Maitland declared two years ago "that our ballistic knowledge has long been fuller and more complete than that of any of the Continental authorities"!

But it is proverbially easy to find fault, especially with Government departments, though by no means so easy to propose practical remedies for defects of administration which force themselves so irresistibly upon the nation as do the shortcomings of our naval and military management. Fortunately, however, no great originality is required on the part of the would-be reformer. He has only to observe how great private firms manage their business, and carry on operations quite as extensive and complicated as any of the branches of Her Majesty's service. What private firm could exist, secure the confidence of its customers, or the soundness of its work, if its head were appointed for five years, and selected, not because he had received special training in the business which he is about to manage, but had done good service in some other situation for which he had been trained; or, worse still, if the selection were made because the man selected was a good fellow and on the best of terms with all the members of the firm! Has any great firm ever started as a company? Has not every one of them owed its origin to the personal qualities of some one man, and those that have survived the death of their founder, have been carried on by men of distinguished ability selected usually by him. We do not speak of enterprises requiring so little originality as railway, gas, or water companies, but even in them the chairman rises by a species of natural selection to a position of prominence, the natural homage due to a master mind. Would any sane man propose, for a moment, that the chairmen of such companies should be changed every five years, and farther, that in order that they might suffer no loss they should exchange offices, that is, the railway chairman, at the expiration of his term, should become chairman of the gas or water company, and so on?

Yet this is precisely the way in which our dockyards and arsenals are managed. Officers, most of them most estimable and excellent gentlemen, who happen to be in favour with the superior powers, receive these desirable appointments, and often travel from one to another till they are obliged to retire from the service altogether. They would be more than human if they refused the offers made to them, and it is unreasonable for the public to cover them with abuse on account of shortcomings which are due to the system, and not to the individuals. We cannot, for a moment, admit that any of the committees are corrupt in the slightest degree, directly or indirectly, or that they have not done their best to carry out the work intrusted to them; neither are we disappointed with the results of our absurd organisation, any more than we should be with the misfits which would result if we insisted on our tailor becoming our bootmaker also.

Take the case of the Ordnance Committee. It is composed of officers who, including the President, are being continually changed, and it also numbers two apparently permanent civil members. Not one of these gentlemen has been brought up to the business of steel-making or gun manufacture, not one of them has made himself a name in the branch of manufacture and metallurgy which the Committee directs, not one of them would be competent to go abroad and start a steel-works and gun factory unaided by the contractors and subordinate experts about them—how can they be competent to deal either with the complicated theories or the practical details of gun manufacture? and when failure occurs, who is to blame?

Judging by Colonel Maitland's lecture and Sir F. Bramwell's expositions, the Committee, as a body, and as individuals, are not only satisfied but even complacent, for they find that other nations are in almost as bad a muddle as we are ourselves. It is perfectly obvious that there must be individual responsibility. The head of each department must be a permanent officer directly responsible for the design and execution of such things as the military or naval forces require, and these heads should be selected from the most able men to be found within or outside the services, men who have already achieved a reputation in the special departments for which they are required.

And there are precedents for this. The Post Office;—can anything exceed the admirable manner in which that branch of administration works, and keeps in the forefront of progress? It is needless to say that it is not worked by shifting committees. Or, take the steam department of the Admiralty. We never hear of serious complaints of the main engines or the countless subordinate machines of our war-ships; our Navy is ahead of all others in respect of adapting every useful invention, every scientific appliance; but then Mr. Wright is a permanent chief, and he is not hampered by a committee composed, shall we say, of carpenters appointed for short terms of office. And yet the changes wrought in Mr. Wright's department during his long term of office are more extensive in kind and much more varied in detail than anything the Ordnance Department have had to contend with. The steam-engine has been completely altered, surface condensation has been introduced, the steam-pressure has been increased eight-fold, compound and triple expansion has been introduced, the whole system of torpedo warfare has come into being with all its complicated appliances, the electric light has been adopted, and in addition the design and supervision of the Dockyard machinery has fallen to his share. The public never hears of Mr. Wright: had we not mentioned his name, most of our readers would have been ignorant as to who was the meritorious officer to whom we were referring; his very virtues have been the cause of his obscurity; his is not an heroic part, and he has never drawn public attention to himself by making a mess of anything.

And next, let us look abroad. We have no hesitation in stating that the most successful gun factory in the world are the Abouchoff Works near St. Petersburg. That establishment, since 1866, has been under the direct personal control of Admiral Kolokolzoff, and during that period the guns turned out have not varied either in design or material; they have been increased in length and constructed to produce higher muzzle energy; but it

is an absolute fact that the projectiles which fit the guns of 1866 can be fired from the guns of to-day, and *vice versâ*. None of the Abouchoff guns have ever burst or injured a single man! The Committee on the *Collingwood* accident ascribe the disaster in part to the unequal composition of the material. Admiral Kolokolzoff provides against the possibility of this by using nothing but crucible steel. His casting-house contains about 2000 crucibles; each holds a small charge of steel, the composition of which is determined with the utmost care and exactness. The consequence is that his material is absolutely uniform, and, in addition, he is the only man that has adopted Whitworth's method of fluid compression. He does not use crucible steel because he has no other means of casting: he has Bessemer converters and Siemens-Martin furnaces; but for the highest-class work he prefers the crucible metal, because of its necessary uniformity when prepared with proper care. Had we had a man of the Admiral's capacity permanently at the head of our Gun Factory, had we subsidised any important steel-works as liberally as the Elswick firm has been assisted, we could also, twenty years ago, have had ingots of 40 tons weight of crucible steel of any quality desired. The Abouchoff works began to make 12-inch guns about the same time as we did, but their gun of the same proportions as those of the *Collingwood* weighs 50½ tons against the 43 tons of our discredited weapon. Our amended guns will weigh the same as the Russian. How is this to be accounted for, if we be, as Colonel Maitland asserts, far ahead of our neighbours in the science of gunnery?

Let us now contrast the Russian record with the history of our own guns. Sir William Armstrong introduced what he, in his address to his shareholders, calls, with some pride, his own gun—our first breech-loader. It was a built-up gun, upon the principles advocated by Mr. Mallet in his work on artillery in 1856, and the breech mechanism was a close imitation of that of the guns on board the Chinese junk which was moored off Essex Street during the Exhibition of 1851. At that time the Broadwell ring, or, rather, gas-check, such as we know it applied to muzzle-loaders, had been used at Woolwich, but had probably been forgotten; at least it was not applied to the Armstrong breech mechanism, which failed from its avowed danger when applied to the larger calibres of guns. We then gave up breech-loading and reverted to muzzle-loading, and finally we have come back to breech-loading, and adopted steel some twenty-five years after the Russians had completely solved whatever difficulties there may have been in the process of using it. In muzzle-loaders we revert to the gas-check, and so we have at least three classes of projectiles in use instead of one only. How is it that we have got into all this confusion? The only possible answer is that it is caused by our absurd system of having no permanent responsible scientifically educated officer at the head of each department of the Arsenal. The newly appointed chief knows nothing of what his predecessor did or what his experiences had been, for experience cannot be readily communicated from one man to another; he is, in fact, not a chief, but, for more than half his time, the slave of his permanent subordinates.

HAINAN AND ITS PEOPLE

Ling-Nam, or Interior Views of Southern China, including Explorations in the hitherto untraversed Island of Hainan. By B. C. Henry, A.M. (London: S. W. Partridge and Co., 1886.)

PORTIONS of this book have already appeared from time to time in the two magazines in the English language published in China, the *China Review* and the *Chinese Recorder*, but they well deserved the more permanent book form, for the author, like many other missionaries, has travelled widely in parts of China which are rarely visited by Europeans. Mr. Henry, too, writes from a full mind; he has made the most of his great opportunities, and accordingly he has contributed here a very real and solid addition to our knowledge of the Middle Kingdom. In reading it we are constantly reminded of a work written a good many years ago by another missionary, which has now almost attained the dignity of a classic, viz. Dr. Williamson's "Journeys in North China"; both are of the same useful, substantial kind, and for a long time to come both will have to be referred to for information in regard to the respective districts with which they deal. Mr. Henry refers solely to Southern China, as the name *Ling-Nam* ("South of the Ridge") implies, and to the Kwangtung or Canton province. He describes various journeys through the central and northern parts of this large and populous province, along the principal streams. As we read o town after town with populations of 100,000 and over, we begin to understand how populous China is. But then, with the exception of the valley of the Yangtsze, the two great southern provinces of Kwangtung and Kwangsi are the most thickly peopled of the whole empire. Even those who have travelled in parts of the Canton province will be surprised to learn from Mr. Henry of the magnificent scenery of the north and north-west. The idea of the passing traveller in and around Canton and the neighbouring cities is that the whole province is a vast plain in a high state of cultivation; but in the upper courses of the tributaries of the West River Mr. Henry found scenes worthy of the wildest mountain regions. Here also, on the borders of Hunan, he came in contact with one of those tribes which are found like scattered fragments over the whole of China south of the Yangtsze—amongst, but not of, the Chinese, with their own communities living generally in fastnesses amongst the mountains, preserving in a great measure their ancient habits, and but slightly contaminated by the proximity of their Chinese conquerors. Their name is legion, and they are sure to furnish abundance of work for ethnologists in the future. In the present instance the people are called the Iu, and are described by Mr. Henry as lower in stature than the Chinese, with a similar complexion, although some are almost copper-coloured. They do not shave the head, but wear the hair coiled up behind, men and women having long hair. They wear immense silver earrings and necklets, while the hair is decorated with ornaments made of the pith of the wood-oil tree and cocks' feathers. Their territory is forbidden ground to the European, the Chinese taking care that the restriction is rigorously enforced. The meagre Chinese accounts of this people

add little to our knowledge of them; but it appears that they have no written language, although a few understand Chinese. Their language is distinct from any Chinese dialect. Beyond these few details nothing is known of the Iu, and they and their country appear destined to remain a mystery for some years to come.

But beyond question the most interesting and valuable chapters in the book are those dealing with the island of Hainan. This has been hitherto in great measure a *terra incognita*. The late Mr. Swinhoe succeeded in going a few days' journey from the coast, and vessels occasionally touched at one of the ports. But it was not until a few years ago that its position was properly settled; before that time it was twelve miles out on all the charts. Mr. Henry, with a Danish gentleman who had already made a circuit of Hainan on foot, travelled into the heart of the island, and making a long detour returned to the port of Hoihow on the north. He thus travelled through a considerable part of the mountainous region in the centre, which is the abode of the Lis, or aboriginal population, and had ample opportunity for studying their habits. The information given in this book about the island and its people is, as far as we know, the first detailed and definite account published in any European language. The whole northern half of the island he describes as a plain, level to a great extent, but mostly undulating, and broken in a few places by isolated hills and low ridges. The central and southern portions are mountainous, the highest elevation being reached in ranges called the Five-Finger and Li-Mother ranges, from which all the larger streams take their rise. The flora of the island, though but slightly investigated, is known to be of great variety and interest. Mr. Henry noticed about 100 species of plants which he recognised as well known, while he brought back 200 species which are now in process of determination. From what is now known, the flora seems more nearly allied to that of the islands of the south than to that of the adjacent mainland. The number and variety of Hainan birds is suprising. Mr. Swinhoe noted 172 species, 19 of which proved new to science, and were first described by him; but as his journey was only of a few weeks' duration, and chiefly along the coast, it is probable that many new discoveries in ornithology will be made when the interior is better known. Of the mammals even less is known, and the variety of fish around the coast is endless. The meteorology, too, is noteworthy. Hainan is the home of the typhoon, and earthquakes are of frequent occurrence. In the latter case the axis of disturbance runs directly across the island from one side to the other. Of the people Mr. Henry is able to give us much more information. The Chinese immigrants have peopled the coast opposite the mainland, and all the low-lying lands up to the base of the hills, which latter are inhabited by the Lis. But between the two is a people speaking a Loi dialect, the origin of which is unknown. They are like the Chinese in many respects: they wear the same dress, live in the same kind of houses, eat the same food, and intermarry freely with them, but they hold to their peculiar dialect with remarkable persistence. There is a theory that these people are descendants of Miao-tsze, brought ages ago from the highlands of Southern China to act as mediators between the Chinese and the aborigines of Hainan. How far they resemble any tribe on the mainland remains to be determined, but they are wholly distinct in physique, language, and customs from any of the Li tribes. The ubiquitous Hakkas from Canton have also established themselves in Hainan, pursuing agriculture under the most forbidding circumstances, and converting the jungle into cultivated fields with their usual tenacity and success. But the main interest of the Hainan portion of the book centres around the Lis. Here, as everywhere else that they have come into contact with aborigines, the Chinese have adopted the simple classification of "tame" and "wild," "ripe" or "green," to distinguish those who have succumbed to Chinese influence and those who have not They wear the hair twisted into a knot on the top of their heads. The women are all tattooed with blue lines over the face. The process of tattooing is very simple. An incision is made with a sharp knife to the shape of the pattern given; and, while fresh, ordinary Chinese ink is introduced, which gives a blue tinge, and in a few days the wound begins to heal. The Chinese say that the same pattern is preserved for generations in the same family, not the slightest variation being allowed, lest the husband's ancestors should not recognise the wife after death. In a Chinese account of the Lis, translated by Mr. Henry, it is said that their custom is not to cry when their parents die, but to swallow quantities of raw meat, which is their mode of expressing great grief,—a curious circumstance, which possibly may be accounted for by the statement made in the same account, that the Lis originally belonged to a race of birds and beasts, and that being derived from an egg they remained impervious to Chinese civilisation. However this may be, Mr. Henry found them a simple, kindly, hospitable people, who appeared to think nothing too much trouble when assisting the stranger. No idols, or other religious symbols, or indeed trace of a religion at all, was found amongst them, although the traveller looked carefully for them. A curious custom among them is for the young people to have authority in the house, and every question of food, lodging, or purchase of articles is referred to them. The father and mother appear to efface themselves inside the house. At meals the whole family is united. They greet a guest by extending the arms, placing the open hands with the finger-tips touching, or nearly so, and draw them inwards with an inviting motion. They bid farewell in a similarly graceful fashion, extending the open hands with the palms upwards, and slightly inclined outwards, in a movement as if handing one on his way. Their features are rather square, the nose not being so flat as that of the Chinese, and the eyes of a different type. No sign of graves was seen anywhere, and all inquiries failed to elicit any intelligible account of what they do with their dead. The substance of all that the travellers could learn was that they place the body without a coffin in any secluded spot, taking care to replace the earth, and cover it over so that it may not be recognised. They are free from many of the superstitious and idolatrous practices of the Chinese; they have no ancestral worship, and no knowledge of geomancy. They seem to be divided into fifteen or sixteen tribes, which are known under different names, and differ more or less in dress, language, and customs, but all evidently belonging to one homogeneous race, bound together by common ties, and as a rule living on friendly

terms with each other. Mr. Henry thinks they are probably of Malay origin, but his argument, based on the names by which they call themselves, appears somewhat weak. It is more probable that we shall know nothing of the ethnology of the Lis until that of the Lolos, the Miaous, Ius, and many other tribes of Southern and South-Western China and Tonquin, has been studied. Whether the Lis have a common origin with one or all of these, or with the aborigines of Formosa, must for the present remain in the region of conjecture. But there can be no manner of doubt that, in the words of Mr. Henry, Hainan promises much of interest to the traveller and scientific investigator, in its striking natural features, in its imperfectly known flora and fauna, and in the questions that arise as to the race, religion, and probable destiny of its aboriginal people.

GIGLIOLI'S "AVIFAUNA ITALICA"

Avifauna Italica. Elenco delle specie di uccelli stationarie o di passaggio in Italia, colla loro sinonimia vulgare, e con notizie piu specialmente intorno alle migrazioni ed alla nidificazione. Compilato dal Dottore Enrico Hillyer Giglioli, &c. 8vo, pp. 626. (Firenze, 1886.)

A SHORT time ago (NATURE, June 24, p. 168) we noticed the new "Check-List" of North American birds issued by the American Ornithologists' Union, and took occasion to refer to the corresponding "List of British Birds" compiled by a Committee of the British Ornithologists' Union, and published by that Association in 1883. We have now before us a copy of a similar publication upon the birds of Italy, prepared, however, under somewhat different circumstances.

At the International Ornithological Congress held at Vienna in 1884, which was attended by delegates from nearly all the civilised nations of the world (with the strange exception of Great Britain !), Italy was worthily represented by Prof. E. H. Giglioli, of Florence, well known as one of the most learned and enterprising zoologists of that country, and for the excellent series of Italian vertebrates which he has collected together in the museum under his charge. In consequence of the recommendations contained in the report presented to the Italian Government on the results arrived at by the Congress, it was determined to constitute an "Ornithological Office" in Italy under the Ministry of Agriculture, and Prof. Giglioli was made Director of the new Department. In compliance with the resolution passed at the International Ornithological Congress, one of the new Director's first tasks was the compilation of a standard list of Italian birds, or "Avifauna Italica," as it is here shortly termed.

In preparing their various lists of native birds, the delegates at the Congress were invited to follow as a model the catalogue of Austro-Hungarian birds, lately issued by Messrs. v. Tchusi zu Schmidhoffer and v. Homeyer. But Prof. Giglioli could not altogether acquiesce in this recommendation, and, perhaps wisely, preferred to adopt the systematic classification already employed for his series of birds in the collection of Italian vertebrates at Florence already spoken of, which is in fact by far the most nearly complete collection of the kind existing in the Italian kingdom. Besides the correct scientific appellation of each bird, and what is considered as its standard Italian name, Prof. Giglioli has also taken great pains to give all the vernacular terms by which each species is known in the many and various Italian dialects. These are in some cases very numerous, as will be seen on reference to such species as *Lanius excubitor* and *Merops apiaster*, and, although of less interest to foreign naturalists, will make the list of greater value to the native student of Italian birds—whose assistance is specially required in ascertaining many yet unknown particulars concerning the range, times of migration, and mode of nesting of the various species.

As regards the limits of the Italian avifauna, it would seem that Prof. Giglioli is an "Irredentist" of the most extreme type. Not only does he include Corsica and Malta within the Italian zoological region, for which, no doubt, he has every show of reason, but also the Trentino, Istria, and Dalmatia. It is difficult to understand why the line should be drawn at Dalmatia, or why it should not also just as well include Montenegro, Epirus, Greece, and even Macedonia ! This so-called "Italian Region" is divided by Prof. Giglioli into three provinces, namely, a Northern Continental, and a Southern Continental Province, separated from each other by the line of the Apennines, and an Insular Province, consisting of Sardinia, Sicily, and the Maltese Islands.

Not only has our author been very liberal in the extent of country assigned to the Italian Ornis, but, in our opinion, he has also somewhat unduly increased the number of species included in the avifauna by the admission of some of very doubtful authority. The occurrence of *Aquila nipalensis* within Italian limits, for example, does not seem to be supported by any certain evidence. The same may be said of *Caprimulgus asiaticus*, *Chelidon cashmiriensis* (!), *Cotile obsoleta*, *Dendrocygna javanica*, and at least half a dozen other species in Dr. Giglioli's list. It would seem, therefore, that the number of species (443) assigned to the "Avifauna Italica" in the present work may have to be slightly diminished, although, on the other hand, future researches will doubtless result in the discovery of many additions to the series of occasional visitants.

Strange to say, one of the most recent and noteworthy additions to the list of permanently resident Italian birds has been made by an English ornithologist. In 1883 Mr. J. Whitehead discovered in the pine-forests of Central Corsica, a nuthatch perfectly distinct from every other known European species, and for the nearest ally of which we must go to Asia Minor. This nuthatch has been named after its discoverer, *Sitta Whiteheadi*. It is quite possible, therefore, that not merely more stragglers from other parts of Europe, but even new endemic species, may still have to be added to the Italian List.

After finishing his general catalogue, Prof. Giglioli goes into a general discussion of the Italian avifauna, and gives the number and names of the various categories into which the 443 species assigned to it may be divided. The permanent residents are stated to be 207 in number, the summer visitants to be 69, and the winter visitants 36. Those of regular passage are only 9 in number ; those of irregular passage, 8. Besides these, 28 are set down as

of irregular appearance, 80 as stragglers, and 6 as doubtful. Two "ornithological calendars," one giving the times of "migration" and the other those of "nidification," add considerably to the value of this useful work, which must not only of necessity be in the hands of every Italian ornithologist, but which every student of the European Ornis, or of any constituent parts, should have for reference. We trust that the good example thus set by England, America, and Italy will lead to the publication of other similar hand-books.

OUR BOOK SHELF

The Law of Storms, considered practically. By W. H. Rosser. Second Edition. (London: Norie and Wilson, 1886.)

WE welcome with much pleasuie the second edition of this useful little work on storms practically considered. The first edition, briefly noticed by us at the time (vol. xiv. p. 504) appeared ten years ago. Since then the researches of meteorologists have materially advanced the science, notably in establishing on a firmer basis the law of the in-moving spiral circulation of the wind in cyclones, and defining with some exactness the limits of variation of the angle of inclination of the winds as they blow inwards toward the centre of storms. In this view especially the last part of the work has been recast, recent investigations being summarised with no little ability, and the results thereafter applied to navigation. The book, which is professedly a practical one, is specially and admirably adapted to give seamen the best available information in handling their ships in storms.

Ueber Manatherium delheidi, eine Sirene aus dem Oligocän Belgiens. Von Dr. Clemens Hartlaub. *Zool. Jahrb.*, vol. i. (1886).

DR. CLEMENS HARTLAUB'S excellent contributions to our knowledge of the recent Sirenians have lately been noticed in these columns (July 8, p. 214). We have now before us his essay on an extinct form of the same peculiar group of mammals. The luxuriance of fossil forms of the Oligocene of Belgium is well known to all zoologists. Upon materials gathered from the Superior Rupelian beds of Hoboken, near Antwerp, which have already produced remains of *Crassitherium* and *Halitherium*, Dr. Hartlaub founds a new genus of Sirenians, nearly allied to the living Manatee, which he proposes to call *Manatherium*. Its dentition, so far as it is at present known to us, does not materially differ from that of *Manatus*, of which, indeed, it may have been the immediate progenitor; and the necessity for its generic separation from its modern representative is perhaps not altogether evident. The species is named *Manatherium delheidi*, from M. E. Delheid, in whose cabinet of Belgian fossils the remains upon which it is based are contained. Fossil species of true *Manatus* have been described by Leidy and other authors in America, and M. Filhol has assigned some African remains to the same genus. But *Manatherium delheidi* is at present the only European form described as belonging to this exact type of the Sirenians.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Tangent Scale in a Galvanometer

ON account of the correspondence which has recently taken place in this and other journals regarding the use of the tangent scale in a galvanometer, when the plane of the coil makes an angle with the direction of the force in the magnetic field, the following extract from Sir William Thomson's patent, No. 4617 of 1883, and the accompanying remarks, may clear up some of the points that have been raised:—

"In using this instrument I sometimes cause the zero to be at one end of the scale, so that, when the potential is at the prescribed definite amount, the pointer is at the centre mark of the scale of tangents. The deflections are thus more easily observed, on account of the large size of the divisions."

This extract is quite explicit as to taking the zero at one end of the scale, and it is abundantly evident, from the text of the patent and from the illustrative drawings, that the scale referred to is a tangent scale. The essential feature in this use of the tangent scale is that the strength of the current is proportional to the difference between the tangents of the angle corresponding to zero and that corresponding to the deflection due to the current.

The total length of the scale, as shown in the patent already referred to, and used in this Laboratory for over two years, is 120°. Lately, however, the length of the scale has, with considerable advantage, been increased to 147°·8, thus giving twice the sensibility obtainable with the 120° scale.

As regards Prof. Carey Foster's letter to NATURE of October 7 (p. 546), a tangent galvanometer arranged to use the tangent scale in this manner is essentially adapted to measure currents which flow through its own coil in one direction only, but in these instruments, as designed by Sir William Thomson, the necessary accuracy is secured by the following procedure. The index (which has a range of about 95° on each side the middle point of the tangent scale) is caused to point to a mark exactly 90° from the middle point of the scale, and the relative positions of the coil and the scale are then adjusted, so that, when the index points to the 90° mark, the strongest current which can be safely sent through the coil produces no sensible deflection.

J. RENNIE

Physical Laboratory, the University, Glasgow, October 16

On the Connection between Chemical Constitution and Physiological Action

IN the address delivered by Dr. Brunton on this subject before the Section of Therapeutics and Pharmacology at the last meeting of the British Medical Association, a copy of which was published in NATURE, August 19, p. 375, he observes, in alluding to the adoption of more scientific methods in pharmacology:—"This may be said to have begun about twenty years ago, when the researches which my predecessor in this office, Dr. Fraser, made with Prof. Crum Brown upon the connection between physiological action and chemical constitution inaugurated a new era in pharmacology. . . . We might first date the beginning of this age from Blake's attempts to show that a connection exists between the forms in which the various bodies crystallise, and the mode in which they act on an animal body. . . . Nevertheless, I think we may fairly say that it was the experiments of Crum Brown and Fraser which fairly started pharmacology in the new direction in which it has since been steadily advancing." Now it can, I think, be shown that in these remarks Dr. Brunton has not only misunderstood the scope of my experiments, but that he has been led into error on account of his having no definite idea of the meaning of the term chemical constitution, which he has evidently confounded with that of chemical composition. The same confusion of these terms is not only apparent all through the address, but is also found in the paper on the subject by himself and Dr. Cash, published in the *Transactions* of the Royal Society, 1884.

The term chemical composition is well understood, but the meaning of the term chemical constitution is not so well defined. Perhaps the difference between the two terms may be pointed out in the shortest space by an example. If we take the salts of iron, for instance, we know that the salts of the protoxide have a chemical constitution analogous to the other members of the magnesian group with which they are isomorphous, and that this resemblance in constitution connotes not merely that they crystallise in analogous forms, as Dr. Brunton seems to imagine, but also that they form many analogous chemical compounds which resemble those of the other members of the same group more closely than the compounds of any other group. By the addition of oxygen to the protoxide, not only is its chemical composition changed from FeO to

Fe_2O_3, but at the same time a base is formed, the compounds of which are totally different from those of the ferrous oxide, but which resemble in their chemical properties the salts of alumina, Al_2O_3, with which they are isomorphous. Here there is evidently a change, not merely of composition, but of constitution; the addition of oxygen has not only altered the relative weight of the constituents, but has completely changed the structure of the molecule in a definite direction, and impressed on it a constitution analogous to that of the molecule, Al_2O_3, with which it is isomorphous. Now, when I had shown that this change in the constitution of the salts of iron was attended by a marked change in their physiological action, when I had proved that the physiological action of the ferrous salts was analogous to that of the salts of the other members of the magnesian group, which they resemble in chemical constitution, and that the physiological action of the ferric salts was analogous to that of the salts of alumina with which they are isomorphous, and when it was proved that an analogous connection existed between the chemical constitution and physiological action of the compounds of the elements of all the more important isomorphous groups, I think nothing but a complete misapprehension of the meaning of the terms chemical constitution and isomorphism could have led Dr. Brunton to allude to my researches as an "*attempt to show that a connection exists between the form in which various bodies crystallise and the mode in which they act on an animal body.*" The same error as regards the meaning of the term chemical constitution has led Dr. Brunton to fail to comprehend the bearing of the experiments of Crum Brown and Fraser on the question, on which, in fact, they throw no light, although evidently regarded by him as a beacon for subsequent observers. These gentlemen found that by the addition of an ethyl or methyl molecule to strychnia its physiological action was profoundly modified, and concluded that this was owing to the chemical constitution of the substance being changed. As they worked with a reagent, strychnine, of the chemical constitution of which we are still ignorant, it was impossible for them to know if the addition of an ethyl or methyl group had made any change in its chemical constitution. The probabilities are that no such change had taken place, as the new compounds were addition and not substitution compounds. It is not merely for asserting a claim to priority that I make this communication, although I think *suum cuique* a very good rule; but I wish to point out the causes which in my opinion are not only retarding the progress of physiology in this direction, but which are tending to throw it back to where it was before the publication of my earlier experiments nearly fifty years ago.

October 6 JAMES BLAKE

Relation of Coal-Dust to Explosions in Coal-Mines

IT is a stubborn and grievous fact that the loss of life by explosions in coal-mines has risen in the last decade, although the number of explosions has fallen. This points clearly to some essential defect in the remedies adopted. The remedies in use apply apparently to one class of explosions only, hence the decrease in the number of explosions. But, unhappily, there are many to which they do not apply, and those the worst; hence the larger number of lives lost. Some research, which has much engaged me of late, and which I hope soon to make public, has led to the conclusion that possibly the reason of this is about to be, or is even already, detected.

Many, well competent to judge, have thought that too much attention has been given to *gas*, and too little to *dust*. This is a growing conviction, both in Germany and in England, and of late years the dust has had considerable attention. A work, recently published (September 1886) by Messrs. W. N. and J. B. Atkinson (Government Inspectors of Mines), entitled "Explosions in Coal-Mines," strives, and I think fully succeeds in establishing that many of the most disastrous colliery explosions in the last six years have been practically "*dust explosions.*" My conclusion, from certain simple physical and chemical experiments, and from a most careful microscopic examination of coal-dust from various seams worked in this field, is that the Messrs. Atkinsons' view is right, and, moreover, that the attention hitherto given to coal-dust has not only been useless, but absolutely pernicious, since it has lulled into a state of false security.

I have been down several typical coal-pits in this district (the Durham field), such as Seaham, Murton, Silksworth, Pelton, &c., and in some of them I have seen the water-pipes along the main haulage roads supplied at convenient distances with stop-cocks, and have found the dust on such roads so damp as to be rendered harmless, if it had not been so already, but the fine dust which coats the upper faces of the roof-timbers was there untouched, lying often to the depth of half an inch. Now, the *bottom dust*, as we may call the former, I am prepared to prove is almost if not quite universally harmless; and the *upper dust*, as we may call the latter, I am equally prepared to prove is in the highest degree dangerous, and especially a certain portion of it, which I propose to call "*flocculent dust.*" Lastly, I claim to be able to establish that the main source of the really dangerous dust is that portion of the coal called variously "dant," "mother of coal," and "mineral charcoal." The upper, and especially the flocculent dust, possesses physical and chemical properties wanting, or existing in the most shadowy form, in the bottom dust; and the microscopical aspect emphasises these differences, as well as shows the relation of the former to what is very appropriately called "mineral charcoal."

It would pass beyond the limits of a letter to enter into particulars, but the observations and opinions of others may be hereby evoked, and this good work of saving life and property be materially furthered by your kindly admission of this letter to your pages. I will now only add that this ordinary upper dust and this flocculent dust removed, or rendered innocuous, it is my firm conviction that the number of explosions will rapidly fall, and also the loss of life be greatly reduced. This is the goal of my effort. Some practical men may indicate means, and perhaps I, who am a student, may be permitted to suggest that keeping the ventilating air-current saturated with aqueous vapour, rather than direct watering, would, if it be practicable, both lessen the amount of this dangerous dust, and also facilitate its removal. ARTHUR WATTS

Bede College, Durham, October 12

Volcanic Ash from New Zealand

A SAMPLE of the ash ejected during the recent eruption in New Zealand has just reached me through the kindness of Mr. W. Ferguson, Harbour Engineer, Wellington. It was collected sixty miles from the seat of the volcano.

The ash is of a gray colour, of a somewhat darker shade than that from Krakatão. The dark-coloured constituents of the New Zealand ash consist principally of black scoriaceous fragments and ferruginous particles of indefinite character; those of Krakatão consist mainly of magnetite and hypersthene, well defined. The following minerals are contained in the New Zealand ash:—

Plagioclase Feldspar, very fragmentary, limpid, showing polysynthetic striations in the polariscope, and sometimes medial twinning line. The few well-formed crystals observed recall the tabular forms from Krakatão (described in my paper, *Proc. Roy. Dublin Soc.*, vol. iv. p. 291). There is a notable absence of the cellular vesicular covering observed in the Krakatão feldspar, and so suggestive of the pulverisation of a ready-formed pumice.

Hornblende, in elongated prisms, sometimes fibrous longitudinally or striated; colour dark green, pleochroic green longitudinally to brown; extinction makes a small angle, less than 15°, with the prism axis; occurs occasionally included in the feldspars. It is scarce.

Biotite and a golden-coloured mica are common in well-formed crystals of hexagonal outline, remaining dark between crossed Nicols.

Iron Pyrites, free and in embedded grains; striated pale yellow octahedral forms. Scarce.

Magnetite, in lustrous black octahedral forms. Not abundant.

Sulphur, in small broken pale-yellow fragments; burns with the characteristic smell, held over a lamp. Scarce.

Glass, remaining dark between crossed Nicols; variously coloured and often containing crystallites arranged fluxionally. Common.

Of these minerals, mica, hornblende, and sulphur are, so far as my observations go, not present in the Krakatão ash. Comparing it with samples from Krakatão gathered at a comparable distance from the scene of eruption, the New Zealand ash suggests a more hastily formed material, the minerals in common being less perfectly developed in the New Zealand ash. I have not noticed as yet in this ash the hypersthene conspicuous in that of Krakatão. There are some organic remains, calcareous fragments of shells. J. JOLY

Physical Laboratory, Trinity College, Dublin, October 18

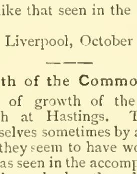
An Abnormal Starfish

A FEW weeks ago I dredged, off the north end of Arran, an interesting specimen of *Porania pulvillus*, Gray (= *Goniaster templetoni*, Forb.). It is rather more than 5 cm. in diameter, and one of the five short rays (that opposite to the madreporite), when viewed from the aboral surface, is seen to be distinctly bifurcated about 1 cm. from its termination. On examining the oral surface, it is found that the ambulacral groove of the abnormal ray divides into two branches at a distance of 2 cm. from the edge of the mouth. One of these branches runs along one of the forks of the ray to its extremity without further complication, but the other branch, belonging to the second fork, divides again 2 mm. from the first bifurcation, so as to form two tracts, which unite with one another 3 mm. further on, thus inclosing a small piece of the ordinary integument in an ambulacral area. Finally, this ambulacral area divides once more close to the tip of the ray. Consequently, there are three bifurcations of the ambulacral area in a space of not more than 1 cm. in length. As there are no signs of injury or disease on the specimen, the abnormal condition seems to have been caused by a tendency to dichotomous division like that seen in the rays of Crinoids and of the Astrophytidæ. W. A. HERDMAN

University College, Liverpool, October 9

Peculiar Growth of the Common Acorn-Shell

A PECULIAR mode of growth of the common acorn-shell (*Balanus*) is met with at Hastings. These crustaceans are known to attach themselves sometimes by a shelly basis to rocks, &c. In this instance they seem to have worked together to form a common shelly tube, as seen in the accompanying life-size figure; still overcrowded, they have also lengthened their outer valves, thus spreading themselves out like the umbels of certain plants, as



many as fifty individuals being sometimes borne on one of these tubes. The columns supporting the pier are completely covered with them up to high-water mark. Except a comparative few of a later generation which—as will be seen in the figure—have attached themselves to the tube, and are in the normal condition, they all appeared to be dead, but of this I am not certain. FRANCIS P. PASCOE

1, Burlington Road, W., October 18

Lunar Rainbow—Halo round the Sun in Connection with the Storm of October 15 and 16

ON the evening of the 9th there was a good lunar rainbow between 7 and 8 p.m. I have not seen one for some years, although about twenty years ago in one winter I saw seven or nine, I forget exactly which.

On the 14th, while coming home between 3 and 4 p.m., I saw a remarkable halo round the sun. I had intended making a sketch of it, but when I arrived there was pressing business, and when I was disengaged it was gone.

The halo was nearly a complete circle, but somewhat pressed in at the sides. Inside, there was a dark mass with a hard defined edge, with the least trace of faint mock suns at the left and right hand lower edges of the circle; outside, the sky was remarkably clear, of a greenish-blue colour. The evening and up to midnight was very hot, which was suceeeded first by heavy rain, then wind that lasted for over forty-two hours; during this time there was considerable heat; afterwards for about six hours it was fine, to be succeeded by heavy rain, and cold; this afternoon (October 17) is fine, with half a gale blowing. I should have mentioned that after the halo the aneroid went down as low as 28·3.

Ramelton, co. Galway J. H. KINAHAN

Mimicry in Snakes

I BEG to remark with reference to Mr. W. Hammond Tooke's letter on mimicry in snakes (NATURE, October 7, p. 547) that I stated a case of mimicry in snakes in the year 1869 (see also *Proceedings* of the Zoological Society, 1870, p. 368), and that I recently published a short paper on Adeniophis, which again treats the case. A. B. MEYER

October 9

The Gale

IT may be worth mentioning as a curious coincidence that three of the most violent gales of recent times occurred on very nearly the same date of the year, namely:—

Sunday, October 14, 1877.
Saturday, October 14, 1881.
Friday, October 15, 1886.

The first of these was the most destructive in its effects as regards this district. W. F. DENNING

Bristol, October 16

Adam's Peak

WITH reference to the "Adam's Peak shadow" I may perhaps mention the following fact:—While at Saas Fee (Canton Valais, Switzerland) this summer, we noticed more than once that the shadows of the Dom and its neighbours stood out clearly defined in the atmosphere. They were manifestly thrown on the cloud and mist that were suspended in the air at the time. The phenomenon was in this case witnessed from below.

Cheltenham, October W. L.

THE MARINE BIOLOGICAL STATION OF BANYULS-SUR-MER

YESTERDAY I had the pleasure of visiting the "Laboratoire Arago," or Marine Biological Station of Banyuls, being the second institution of this sort founded by M. Lacaze-Duthiers in connection with the College of the Sorbonne. I found it in full working order, the session, which lasts here from October to June, having just commenced. The building is placed at the western point of the little bay on which the fishing-village of Banyuls is situated, just beyond the Établissement des Bains, and consists of three stories. The ground floor is one large *salle*, containing basins which are abundantly stocked with marine animals, and is open to the public for inspection as an "aquarium." The lighting of these basins appeared to me to be particularly good, and the collection contains many Mediterranean forms not to be seen in our northern latitudes. The first floor contains the working-rooms, library, and museum; the upper floor contains the apartments of the Director. In M. Lacaze-Duthier's absence the institution is under the charge of M. Henri Prouho, whose official title is "Préparateur au Laboratoire Arago."

The fauna of Banyuls is probably not nearly so rich as that of Naples, but the advantages to English students would be, the nearer access (twenty-six hours from Paris) and the much more healthy situation.

If I understand rightly, the subvention awarded to the "Laboratoire Arago" by the French Government considerably exceeds that proposed to be given by the Treasury to our new Biological Station at Plymouth. P. L. SCLATER

Port Vendres, Pyrénées Orientales, October 15

KEW GARDENS

THE well-known scientific traveller, Dr. Schweinfurth, has recently paid us a visit in England. The *Berliner Tageblatt* of September 12 reports his impressions of our botanical institutions as given in a lecture delivered the previous day before the "Versammlung deutscher Naturforscher und Aerzte." We extract the account of the National Botanic Garden at Kew. Some trifling

inaccuracies have crept into the report, but we leave them as they stand. The impression produced by English scenery, vegetation, and gardening on unaccustomed eyes is difficult to realise by those to whom they are the common-places of every day.

Dr. Schweinfurth opened his discourse with the remark that England under the influence of a summer sun such as it had enjoyed this year deserved to be called the most beautiful country in the world. Indeed, the ordinarily serious and matter-of-fact man of science seemed to be filled with enthusiasm when relating his observations on botanical institutions in England. He assured them that although he had spent weeks in London he was only in a position to offer them a fragmentary report on the subject. It is true the amiable orator had provided himself with very considerable fragments.

He first of all gave a detailed description of the worldwide renowned Kew Gardens, which he compared to a botanical " Ministry for the Exterior," inasmuch as there the reports of all the embassies and agencies flow together. The development of this gigantic establishment presents similar phases to that of the Berlin Botanic Garden. Originally a kitchen garden for the Royal Court, it has during this century expanded to its present size of about 250 acres. Independently of its varied and enormous botanical treasures, Kew Gardens is one of the most beautiful examples imaginable of park-like arrangement. Everything luxuriates in the most glorious foliage; not a dead leaf nor a dry stalk was to be seen. Everywhere the most untiring and intelligent care was evident; and the manner in which the most delicate plants are brought to their fullest development must excite the universal admiration. The entire administration of this extensive establishment is as simple as it is worthy of imitation.

Kew Gardens contain a vast herbarium which is preserved in a simple light building with open galleries running all round, in which work may be done during the day, but no lights are permitted. Along the walls are placed the cabinets containing the dried plants, which are poisoned with a solution of corrosive sublimate. The arrangement of the species is geographical. A magnificent library and an extensive collection of drawings greatly supplement the usefulness of the herbarium.

The way in which the plants are stuck on sheets of paper throughout their whole surface was deprecated by Dr. Schweinfurth.

Further, Kew Gardens contain seventeen large plant-houses, among which he specially mentioned those devoted to orchids, succulents, and tropical plants, including the palm-house, a building of about the same length as the Berlin palm-house, though by no means so lofty. Of the most beautiful part of the Garden, the colossal rockery of Alpine plants impressed Dr. Schweinfurth most, as it was in its greatest floral richness at the time of his visit.

There are also three spacious museums, situated at some distance from each other, which is a disadvantage; though from the enormous number of visitors—sometimes as many as 80,000 in a day—the separation may have appeared necessary. One of the museums contains a collection of useful vegetable products in various stages of development and manufacture. Another building contains the picture gallery founded by Miss Marianne North, consisting of 800 botanical landscapes from all parts of the world. They mostly represent the general aspects of plants, and their purely scientific value is unequal; but the great care with which the fruit is always painted is worthy of all praise.

LEPIDOPTERA IN THE SIKKIM HIMALAYA

DARJEELING has long been celebrated among entomologists as one of the richest localities in the world for insects, and especially for Lepidoptera, which, owing to the fact of their meeting a ready sale among visitors to the station, are collected as a matter of business by twenty or thirty of the Lepcha and Bhotea inhabitants of Sikkim.

But though many of the superb insects found here are common in collections, little or nothing is known as to their distribution, habits, and time of appearance, as no resident naturalist has ever done much at collecting or observing their habits in person. The number of species is so great, many of them so rare or so uncertain in their appearance, and the difficulty of studying their habits so great, that there is ample room for many years' work in this direction, and the lists which have been published by Mr. De Niceville in several recent numbers of the *Journal of the Asiatic Society of Bengal*, together with the very numerous additions to the known species made by Mr. O. Möller, show what a rich harvest is still to be gathered by one who does not fear exposure to the tropical heat and risk of fever in the low hot valleys where most of them are taken.

Having spent several days recently in observing the butterflies of Sikkim, I may give some idea of their habits and haunts.

First, and by far the most numerous, are the butterflies belonging to the fauna of the Indo-Malay region, which inhabit the low damp valleys from the level of the plains up to about 3500 or 4000 feet. This region is extraordinarily rich in the genus Papilio, of which there are at least thirty species almost confined to it, though some of them on hot sunny days fly far up into higher elevations. Most of these species are many-brooded, and begin to appear in March, continuing till the end of the rains to fly in greater or less numbers. Some of them, however, are only single-brooded; almost all of these appear before the rains, from March to the end of May or June. In the hot valleys they fly at all times of the day up till 4 or 5 p.m., and are only to be procured in quantity and in good condition by those who know their habits, the flowering trees they frequent, the wet spots in the sandy banks of rivers, where they associate in great numbers to settle, and the most attractive baits by which to allure them within reach of the net. This is the sort of work which the Lepcha excels in. He likes the wandering free life in the jungles far better than steady work, and, filling his boxes in two or three days without much exertion by waiting in the favourite haunts of the butterflies, he earns a handsome wage by selling his booty at a pice apiece. He will not trouble himself to catch the small and inconspicuous Lycænidæ and Hesperidæ, unless specially instructed to do so; but, as a fact, these two families are the most numerous in species, if not in individuals, and would probably together amount to at least 200 species in Sikkim, almost all of which, as far as I can learn, are found in this zone of altitude.

Nymphalidæ also are very numerous and very varied, though more difficult to procure. The females of some of them, as well as of some Papilios, remain unknown, or are very rare, notwithstanding the abundance of the males. They do not fly much, or frequent the open sunny places, but remain settled high up on trees, or in dense jungle, where it is impossible to penetrate or to use a net. Many large and splendid moths of the family Agaristidæ are mostly day fliers, and innumerable Bombyces, Geometers, and Sphinges also frequent these hot valleys, and are bred or captured in various ways by the Lepchas, but seldom by Europeans. During the rains, when they are most abundant, the risk of fever at night is too great for much lamp work, and breeding is by the natives but little understood. The smaller moths, especially the Microlepidoptera, remain almost unknown, though some of the most showy sometimes find a place in the boxes of the Lepchas.

When we come to the zone of elevation between 3000 and 6000 feet, we come into a climate which produces the

grandest forest in Sikkim, and surely one of the grandest in the world. A mixture of tropical and temperate forms in highest perfection occurs, oaks, chestnuts, magnolias, laurels, and many other giant trees, laden with climbers, orchids, ferns, aroids, and other epiphytes, till the branches break with their weight, mixed with a number of beautiful shrubs and herbaceous plants. But this forest is almost everywhere, unless strictly protected by the forest department, or growing on slopes too steep for cultivation, destroyed by fire or axe, for the purpose of cropping with rice, millet, Indian corn, and potatoes, which are the principal crops of the natives; and owing to the great extension of cultivation, and the immigration of Nepalese into Sikkim and British Bhotan, a tract of really virgin forest between 3000 and 6000 feet is becoming quite a rarity.

Partly on account of this destruction of the native trees, which are replaced in abandoned cultivation by worthless weeds, such as artemisia, and by quick-growing soft-wooded trees of no value, the species of butterflies peculiar to this zone are much fewer in numbers, both of species and individuals, than lower down, and some of the finer and larger species of Adolea, Limenitis, and Athyona, which formerly were not rare in Sikkim collections, appear to be now very scarce or extinct in their old haunts. A little higher up, however, we find a forest of much the same character, though denser, darker, and the trees much more overgrown with moss. At 7000 and 8000 feet rhododendrons appear, and a dense undergrowth of hill-bamboo, called "maling," which forms the principal fodder for ponies in Darjeeling, in some places makes the forest quite impenetrable. Here the sun shines but rarely during the rainy season, and even in the cold weather mist is very prevalent. This forest is the home of some of the most superb insects in the world.

Let us walk up a few miles above Darjeeling into the great forest which covers Sinchul on a sunny morning early in June, and wait on one of the highest peaks, where a small bare space can be found. Flying over the tops of the trees with a rapid soaring flight we shall see that grand insect *Teinopalpus imperialis*, peculiar to these forests, and if lucky enough to attract him to the ground by a bait, or able to reach his resting-place, we may catch one or two in a morning. But his female so rarely flies from her leafy perch that in sixteen years I only know of three or four examples having been taken, and these one may say by accident in unexpected places. *Papilio Krishna* and *P. Minercus*, again, frequent the same forest; but of the former, though males are in places abundant, the female is hardly, if ever, taken. *Herda duma*, *Picris Horsfieldi*, *Neptes Zaida*, and other species, have the same peculiarity, that the females are hardly ever seen; and only long and patient waiting in spots where sunshine is of rare occurrence, will enable the most sharp-sighted collector to obtain them. Some beautiful, though sombre-coloured, Satyridæ, such as *Lophoessa goalpara*, *Yama*, and others, *Raphicera satricius*, *Lethe scanda*, *Dinarba* and *Sidonis*, are peculiar to these shady, damp forests, and flit along the roads when disturbed in dull weather as well as in sunshine; but however active the search, the number of species and of individuals seen in a day will be small compared to the results of a day in the tropical valleys. Higher up still, from 9000 to 12,000 feet, the outer ranges of Sikkim are very poor in diurnal species, though rich in Geometra, and Micro-Lepidoptera, as the climate is too damp and sunless in summer to encourage the appearance of species of Palæarctic genera, which are in places so abundant on the more sunny, grassy hills of the North-West Himalaya.

In the interior, however, where the climate is drier, and where Coniferæ and rhododendrons form the principal features of the forest from 8000 to 11,000 feet, there are a number of European genera and species which I have at present only procured through native collectors, but which I hope to see for myself before long in life. *Papilio Machaon*, *Colias Fieldii*, *Picris brassica*, *Vanessas*, *Argynnis Lathonia*, the lovely *A. gemmata*, are common in these higher, drier, and more flowery regions, whilst *Parnassius*, *Aneis*, *Melitæa*, and other Alpine genera are also found in certain places. The moths of the interior hills are too little known for me to say much about them, but there are great numbers of species of European aspect, and many novelties amongst them may be expected whenever the Tibetan frontier is crossed.

H. J. ELWES

SKETCH OF THE EARLY HISTORY AND SUBSEQUENT PROGRESS OF PALÆOBOTANY[1]

AMONG the many memoirs included in the Fifth Annual Report of the U.S. Geological Survey, just distributed, none evinces more laborious research than the sketch of palæobotany, and no part of this will prove more valuable, both from its exhaustive treatment and its wealth of references, than the section with the above title. The matter divides itself naturally into a history of the scientific, and of the pre-scientific period. To the latter of course belong the speculations of the early Greek philosophers, whose ideas were far more correct than those held fifteen or sixteen centuries later, for they at least recognised that petrifactions had once been living things, and that the mountains in which sea-shells were embedded had once been under the sea. These doctrines were it appears the popular belief of the Romans, and continued to be held until the spread of Christianity caused them to be rejected, and that long period of stagnation to set in, when all natural science was weighed down and subordinated to the religious cosmogony.

We do not find, however, any direct and unequivocal references to fossil plants or wood in either Greek or Latin writers, though such must have been far from uncommon objects in limestone districts, and the history of palæobotany cannot therefore be said to have commenced before the thirteenth century, when Albertus Magnus described most unmistakably the occurrence of petrified wood.[2] Little further mention, however, is made of any fossil vegetable organism until the latter half of the sixteenth century, when we find several writers describing and discussing the origin of petrified wood, which seems to have added fuel to a controversy that had already for centuries been raging concerning the genesis of petrifactions. Building upon Aristotle's doctrine of spontaneous generation, scholastic writers had come to affirm that it was equally possible for stones to grow of any required form as for living animals and plants. Avicenna in the tenth century had conjured up a *vis lapidifica*, and Albertus Magnus in the thirteenth century had imagined a *virtus formativa*. Bauhin dreamed of some subtle Spirit of the Universe, while Libavius opined that fossils grew, like living things, from germs or seeds. Balthasar Klein obtained a petrified stem, one side being stone, the other coal, an object which excited the liveliest curiosity. He sent the specimen to Matthiolus, who, after studying it, came to the conclusion that coal was a third and final step in the process of transmutation, and that just as wood turned into stone, so stone in turn became transformed into coal. Klein's own views about it seem, however, to have been more rational. The discovery in the mines of Joachimsthal of a petrified trunk with the bark on added to the interest already aroused, and kept alive the discussion.

In 1565, leaf-impressions incrusted in tufa were

[1] From the Fifth Report of the U.S. Geological Survey, by Lester F. Ward, condensed by J. S. Gardner.

[2] For all references the Fifth Report of the U.S. Geological Survey, p. 388 *et seq.*, must be consulted.

described by Kentmann, and in 1664 the existence of leaf-impressions in true rock was for the first time published by Major. In 1699, Lhywd, a Londoner, figured and described a number of ferns from the Coal-Measures, which can even now be recognised. These he was inclined to consider due to the *succus petrificus*, a petrifying juice whose action was controlled by the *vis lapidifica*, both petrifying forces having been invented by Kircher in 1655, when he propounded his theory of *seminaria de corpuscula salina* as the true faith regarding petrifactions. Sperling believed in a special stone-making spirit, and Camerarius (1712) held that in the beginning God had supplied the earth's interior with these varied forms, just as he had placed grass and herbage on its surface. Still others were content to regard fossils as mere freaks of Nature. Such-like ideas held the field, and only began to give way during the early years of the eighteenth century, for we find that, as late as 1733, infinitesimal particles were believed by a Dr. Arnold to have been brought together at the Creation to form dead outlines or images of all the living creatures upon or within the world. During all these dark ages, however, there were not wanting writers who held more rational views as to the nature of fossils, and even combated the supernatural explanations of the dominant schools. It was due to fossil vegetables, according to Brongniart, that these crude ideas came to be abandoned. All these theories were swept away by the "Flood theory," the first germ of which is apparently to be found in Luther's commentary on Genesis, where he expresses the belief that surviving indications of the Deluge would be found in the form of wood hardened into stone around the mines and smelting-mills. Several writers between Luther's time and the close of the sixteenth century held the same view, but the Flood theory was for a time drowned in the more fantastic speculations then in vogue, not to come to the surface again until another century had passed. In 1695 Woodward published a work on fossils, in which he maintained that all the solid parts of the earth's crust were loosened by the Flood and mingled promiscuously in its waters, and that at its close everything sank back to the surface according to its specific gravity, the remains of animals and plants assuming the positions in which they are found petrified. The chiefest expounder of this hypothesis, however, was Scheuchzer, whose great work on fossils, in 1709, laid the foundations of palæobotany, though he subsequently rendered himself even more notorious by describing a large fossil Salamander as *Homo diluvii testis*. His work, however, aroused so deep an interest that for many years collectors and writers were busy searching for and describing fresh evidences in support of the Diluvial theory. It had indeed for some time no serious rival, and remained all but universally accepted down to the second half of the eighteenth century, when dissentients first ventured to make themselves heard. The last two decades of the eighteenth century were destined to witness a collapse of the Diluvial theory as rapid as its rise in the first decade, though Hugh Miller even found supporters of it in our own time.

During the seventeenth century the occasional protests of the rational minority, among whom we find Steno, made few disciples; but during the eighteenth their arguments were felt with increasing force. The Deluge hypothesis, faulty as it was, was a great actual advance, for it at least recognised the real nature of the objects, and turned discussion towards the means through which fossils came to be embedded. Though several authors wrote in a truly scientific spirit during this century, it was Blumenbach who first taught with authority that the beings to whose former existence these fossil forms were due were not only antediluvian, but pre-Adamitic, and that, moreover, there had been a series of faunas and floras inhabiting the earth before the age of man. The change in opinion, however, had long been preparing, and prominent among the questions that led up to it were: Are these the remains of the same kind of plants that are now found growing upon the earth? and, When did the originals live that have been preserved by changing into stone? Only two generations since the answers would have been universally that they were plants that grew but a few thousand years ago, and that they either grew where found, or had been brought from other countries by some such agency as the Flood, or else had been destroyed by these agencies and become extinct. Scheuchzer regarded them as plants which could still be found living, citing a number of genera as examples. Among many others who embraced this view was Lehmann (1756), who laboured hard to prove that the impressions of *Annularia sphenophylloides* were flowers of *Aster montanus*, caught in full bloom, and petrified *in situ*. The exotic theory, as it may be called, first appears in a note of Leibnitz, 1706, on the occurrence of impressions of Indian plants in Germany; and in 1718 Antoine de Jussieu discussed the resemblances of the coal plants of St. Chaumont to ferns of the tropics. Parsons (1757) stated that the Sheppey fruits were absolutely exotic, and Dulac soon after compared the coal plants of St. Etienne to American species. These instances are only a few among many, for similar views became commonly held. Volkmann (1720) and others held what may be described as a degeneration theory, believing that antediluvian vegetation was of a higher order, and free from thorns, thistles, and other scourges, while comprising many fruit-bearing trees of which our modern ones are the degenerate representatives. The same authors held at the same time mixed views, thinking that many of the petrified plants might have become extinct during the Deluge or other physical changes, and it was probably this idea that led to the more critical investigation of the stratified rocks, and brought the question as to when the originals lived within the region of practical science.

THE RECENT EARTHQUAKES AND VOLCANIC ERUPTIONS

TERRIBLE as has been the tale of destruction to life and property during the last six years owing to the exceptional activity of the subterranean forces in nearly every part of the globe, we cannot avoid the reflection that scientific men in the future will feel that there have been at least some compensating advantages for these sad losses. Never before, perhaps, have greater opportunities been afforded to us for collecting the real facts, and for testing, verifying, or correcting hypotheses concerning these interesting phenomena; and never, certainly, have such organised efforts been made to deal adequately with the great opportunities which have been afforded to us.

After the earthquakes at Agram, a Commission appointed by the Hungarian Government was sent to examine the district, and the result was a Report of great value and interest, in which the exact details of the actual phenomena observed were carefully sifted from the mass of vague rumours and gross exaggerations with which they had become involved. Admirable monographs on the terrible earthquakes of Ischia in 1881 and 1883 have been prepared by Prof. Mercalli, of Monza, and by our own countryman, Dr. Johnston-Lavis. The tremendous catastrophe which occurred in the Sunda Straits three years ago has already given rise to a vast mass of literature bearing on the subject. Commissions, including very competent observers, were sent to the district by the Dutch and the French Governments, and the former of these has already completed and published its very valuable Report. We may be certain, too, that the more

recent events, in New Zealand and Charleston respectively, will not be allowed to sink into oblivion until every effort has been made to gather to a focus all the light which they are capable of affording to us on the great problems of vulcanology and seismology.

No one can have studied the reports of the late eruption of Tarawera in New Zealand without being impressed by the energy and enterprise exhibited by the local Press of the colony. The first mail after the outbreak brought us very full and detailed accounts collected by correspondents who, braving no inconsiderable risks, travelled over the scene of the catastrophe to collect information, and these accounts were amply illustrated by maps, sketches, and photographs. It must always be remembered, however, that the requirements of journalism and science are different, and to some extent antagonistic: the former demands, above all things, speed; the latter, accuracy. It is often only when the work of the newspaper correspondent is well-nigh forgotten that the scientific man finds himself in a position to deal with the vass mass of unsifted materials—good, bad, and indifferent—which is poured out before him in such wonderful profusion; to him relations of events can never be "stale" if they are capable of being authenticated and of supplying accurate data for the legitimate inductions of science.

In the case of the Tarawera eruption, as in that of Krakatão, it must be always a subject of regret that the topographical and geological surveys of the scene of the outbreak which were made prior to the event appear to have been far from perfect. Tarawera is situated in the midst of a barren region, in the very heart of the native reserve, and it was moreover most jealously guarded from the intrusions of white men by the superstitious Maoris, who used it as the place for depositing their dead. Dr. Hector, the accomplished Director of the New Zealand Geological Survey, confesses that he had never been able to ascend the Tarawera Range, but that from an examination of its flanks he concluded that it was composed of highly acid (rhyolitic) lavas in *coulées* and dykes, and that large quantities of obsidian and pumice were also present. He was thus led to conclude that Von Hochstetter was right in mapping the mountain as belonging to his recent volcanic series. Mr. Percy Smith, the Assistant Surveyor-General of New Zealand, who like Dr. Hector was upon the spot within a few hours of the outbreak, had been more fortunate in obtaining some knowledge of the upper part of the mountain before the eruption. In the year 1874 he ascended the mountain three times, and found its summit to be destitute of any trace of a crater, but to consist of a table-land about three miles long by half a mile wide, divided into two portions by a saddle, and covered by angular fragments of rhyolite, apparently shivered by the action of frost.

There unfortunately still exists some doubt upon the question as to whether Tarawera has ever been in eruption during the period that New Zealand has been occupied by the Maoris. On the one hand, it has been asserted that no traditions of any previous outbreak are preserved among the natives; but, on the other hand, the names given to the parts of the mountain are said to indicate a knowledge on the part of those who first applied them of its volcanic character, and moreover the extreme sacredness attaching to the locality seems certainly to point to the conclusion that there had been something remarkable in its past history.

Certain it is, however, that, up to June 10 last, Tarawera was not by any means regarded as a spot upon which a volcanic outburst might be expected to break out. But after a series of violent earthquakes occurring on the midnight preceding that day, and lasting for about three-quarters of an hour, a great fissure opened, beginning with an orifice on its northern summit and gradually extending south-westwards to a distance of four miles during the next hour and a half; distinct ejections took place from at least seven vents along this line of fissure. The highly heated condition of the materials thrown from these vents, which set on fire trees at a great distance around the mountain, certainly points to the conclusion that molten lava was ejected from the volcano during this its earliest stage of eruption. But that this lava rapidly became consolidated and no longer incandescent on its surface appears to be clearly established by the observations of Dr. Hector, who, watching the steam cloud on two successive clear nights, was unable to detect any trace of a reflected glow upon it. After the first tremendous outburst, this eruption appears to have been almost entirely a hydrothermal one, and to have slowly but gradually declined in intensity.

Immediately after the completion of the first fissure, there opened a second one throwing out enormous volumes of steam. This second fissure, which eventually attained a length of nearly eight miles, running in a nearly north-and-south direction, was that which passed through the famous lake of Rotomahana. Its ejections seem to have been purely hydrothermal in character, and by the masses of ash and mud thrown out, the beautiful sinter terraces have been apparently converted into mud-volcanoes. There still remains some doubt as to whether the second fissure is not to be regarded as a branch of the first-formed one. The eruptions from a number of vents formed along this second fissure have also been gradually diminishing in intensity; but the quantity of steam and of more or less finely comminuted rock ejected from them has been enormous.

According to the latest accounts which have reached us from the district, the ash, which covers the whole country like a great mantle of snow, effectually prevents the completion of the necessary geological observations upon the scene of the eruption. Indeed, Dr. Hector, after a preliminary survey, felt that no useful detailed work could be done until the rains have removed this covering of loose dust and rendered the country more easily accessible. Mr. Percy Smith has, however, ascended Tarawera, and reports the existence of a great fissure four miles long and five hundred feet wide, of which there seems to have been no trace when he ascended the mountain on former occasions.

It is evident from this brief outline that a number of problems of the greatest interest await solution in connection with the recent display of volcanic energy in New Zealand. Never before, perhaps, have better opportunities been afforded of studying the phenomena attending the formation of the fissures along which volcanic ejections take place. It is remarkable, too, that, although the quantity of materials erupted was very great, there were few if any regular cones of the ordinary pattern built up along the fissures. There also remains much to be learnt concerning the actual nature of the materials ejected at different stages of the outburst, and the way in which they were distributed: all the materials at first thrown out still remain buried under the later ejectamenta.

Upon these and many other problems of the greatest importance we may rely on the geologists of New Zealand, both officials and amateurs, for obtaining all possible evidence during the next few months; and when their researches have been completed the New Zealand eruptions of 1886 can scarcely fail to prove among the most instructive which have ever come under the observation of vulcanologists.

That the terrible catastrophe at Charleston will be similarly utilised by the numerous and able geologists of the United States we cannot for a moment doubt. The telegram despatched by Major Powell during the recent meeting of the British Association at Birmingham shows how fully alive he was to the importance of carrying on systematic observations in the district; among his staff of excellent geologists constituting the United States Geological Survey he will experience no difficulty in

selecting observers admirably qualified for this investigation; nor need we fear that the United States Government will be wanting in their accustomed liberality in publishing the Reports on the subject when they are prepared. Nor will the unofficial geologists of the country and private associations be behindhand in contributing to the mass of information gradually accumulating upon the question of the nature and origin of the terrible event.

ON LION-BREEDING[1]

THE Gardens of the Royal Zoological Society of Ireland have become famous among zoological gardens for their breed of lions. While here and there among the zoological gardens of the world a lion cub is born, none save those of Dublin can boast of a period of lion-cub production of nearly thirty years' duration, or of the extraordinary success of the birth of 131 cubs. This being so, we are indebted to Mr. V. Ball for a history of the subject, which has been published in a recent part of the *Transactions* of the Royal Irish Academy. The subject is one of interest in several ways, and the following short abstract of the details will call our readers' attention to it.

In 1855 a pair of lions from Natal were purchased for these Gardens. The exact relationship of these appears to have been unknown, but their first litter was born in 1857. From 1857 to 1885 we find a total of 131 cubs born, of which twenty-one were either born dead or died shortly after birth, and 110 were reared, eighty-six of these latter being sold, greatly to the profit of the Society and to the advantage of very many of the zoological gardens of Europe, Asia, and America. These 131 cubs were the offspring of nine lionesses and four lions; of the latter, one, "Natal," was the father of forty-two cubs; and another, "Old Charley," who was a son of "Natal's," was the father of forty-six; while of the former, one, "Old Girl," who was born in the Gardens in 1859 as one of a litter of five, was the mother of no less than fifty-five cubs, of which forty-nine were reared. This prolific lioness died at the age of 16 years, apparently of old age.

The facts given by Mr. Ball in one of his very carefully compiled tables seem to indicate two periods of the year at which lionesses in a state of semi-domestication produce their young. While the absence of any well-authenticated information as to the period of the year in which lion cubs are born when in a state of nature is quite remarkable, yet Mr. Ball ventures the fairly safe surmise that considering the period necessary for the rearing and education of a cub to be at the least a year, for the cub is often learning to kill its prey when over a year old, it is most improbable that lionesses have more than one litter in a year when in a wild state; but he thinks it probable that the geographical surroundings of the parents may alter this period, and that it may be in the autumn season in the tropics, when the great heats and droughts of summer are over, and in the spring season in more temperate climes, where the summer warmth would be of service to the young offspring; and he very ingeniously speculates that the two periods of maximum production, as observed in the lionesses in the Dublin Gardens, may have been inherited from two corresponding periods, the result of climatal conditions in a wild state. Another remarkable phenomenon comes to light on comparing the curves of production, when modified into curves of conception, with the monthly curves of temperature for Dublin. In doing so, the maximum curve in the one case is found to closely approximate to the maximum curve of temperature, *i.e.* June and July; and the second maximum curve corresponds to the period of lowest temperature, *i.e.* December and January: but it will be remembered that then the animals are kept in well-heated houses, so that this period, as to temperature, may, though the temperature be artificial, be compared to the other, when it is natural.

The cubs when born are noted as distinctly spotted with dark brown on a ground colour which is rather light brown than fulvous; from about one to three months they are perhaps most distinctly defined; and, though along the back the spots are somewhat quadrangular in shape, there is no indication of actual bars or bands.

In reference to the sexes of the cubs, Mr. Ball is able from accurate information to record the sex of 130 of the cubs, and we find 74 were males and 56 females, giving a majority of 14 males in every 100 cubs. This is an interesting and novel addition to our knowledge of the natural history of the large Carnivores.

No lion or lioness lived in the Gardens for a longer period than 16 years, and it seems probable that 12 to 14 years is the average duration of lion life. The cases so often referred to of lions living to an age of 20 to 30, or the case of "Pompey," who died in the Tower in 1760 at the age of 70, stand on no scientific or even reliable evidence.

Under the heading of "The Cause of Success in Breeding," we find some valuable suggestions as to the keeping of these splendid Carnivores; but we searched in vain for the secret of success. Horse-flesh is evidently not dear in Dublin, as the annual cost of the food of an adult lion, being for the most part horse-flesh, only came to 15*l.* in 1885. A series of tables accompanies the memoir, and some illustrations of the cubs of the lioness "Queen," born April 1885, from drawings by Mr. Thomas.

[1] "Observations on Lion-Breeding in the Gardens of the Royal Zoological Society of Ireland," by V. Ball, M.A., F.R.S., Director of the Science and Art Museum, Dublin, and Hon. Sec. of the Royal Zoological Society of Ireland. *Transactions* of the Royal Irish Academy, vol. xxviii. Part 24, August 1886.

NOTES

AN article in NATURE for May 6 (p. 7) drew attention to the fact that this present year is the tercentenary of the introduction of the potato into England, and discussed some of the points of its history. Apart from the purely historic aspects of the question, "Whence did our potato first come?" it was shown that in connection with the suggestion of cross-breeding to strengthen against disease it is very important to know which is the species that for three hundred years we have been cultivating. With a view to drawing the attention of cultivators to the subject, it is proposed to hold a Tercentenary Potato Exhibition at the St. Stephen's Hall, Westminster, from Wednesday, December 1, to Saturday, December 4, and to appoint one of those days for a Conference, when some of the unsettled questions may be discussed. The Exhibition will consist of four sections:—(1) An historic and scientific collection, to include early works on botany, in which the potato is figured; maps showing the European knowledge of the New World three hundred year ago, and the proximity of potato-growing districts to the ports most frequented; early books on travel and voyages in which references to the potato occur; works and papers in which attempts to define the different species are made; illustrations of the species and varieties; contemporary references to the voyages of Hawkins, Drake, Grenville, and Raleigh. (2) Illustrations of potato disease, and works on the subject. (Sections 1 and 2 will be arranged under the advice of a committee of scientific gentlemen who have consented to give their co-operation.) (3) Methods for storing, preserving, and using partly diseased potatoes, &c. (4) A display of tubers of all the various varieties grown. (In this section gold, silver, and bronze medals will be awarded. Each exhibit must be accompanied by a statement of date of planting, locality, nature of soil, &c.)

Mr. John Whitehead, well known to ornithologists for his discovery of a new species of Nuthatch in Corsica, has been travelling in Borneo during the last two years, and has sent home a large collection of birds. Unfortunately the disturbed state of the country in the Brunei district has prevented Mr. Whitehead from accomplishing the chief object of his expedition, the ascent of Kina Balu, and he has therefore been obliged to restrict his labours to the neighbourhood of Labuan and the country round Sandakan. These districts have been well worked for years by Mr. Alfred Everett, Mr. Treacher, Mr. Pryer, and other naturalists, and Mr. Whitehead's collection, although very complete, did not contain anything new to science. He is now travelling in Java, and purposes to explore some of the mountain-ranges in the interior.

Mr. George Reid, a well-known contributor to the pages of Mr. Allan Hume's journal, *Stray Feathers*, has just finished a "Catalogue of the Birds in the Provincial Museum, N.W.P. and Oudh, Lucknow." Mr. Reid is the member of the Committee of Management in charge of the Natural History Department of the Lucknow Museum, and the value of the bird-collection depends upon the series which Mr. Reid has himself given to the Museum, his donation amounting to 1287 specimens out of a total of 2646. The Museum appears to possess a very complete collection of birds from the neighbourhood of Lucknow, and it is encouraging to see that Mr. Reid, in re-organising the natural history portion of the Museum under his charge, has recognised the first duties of a local institution, and has commenced by making a good collection of the birds of the province, of which Lucknow is the capital. Mr. Reid will be glad to entertain the idea of exchanges with other Museums.

The concluding part of the late John Gould's "Supplement" to his "Monograph of the Trochilidæ" will shortly be issued by Messrs. Sotheran. This work, which was left unfinished at Mr. Gould's death, has been completed by Mr. Bowdler Sharpe, who has also nearly finished the great work on the "Birds of New Guinea," which had not long been commenced by the author before he died.

Messrs. Taylor and Francis will shortly publish a work by Mr. T. Mellard Reade, F.G.S., entitled "The Origin of Mountain-Ranges." In addition to containing a systematic theory of mountain-building, with detailed experimental illustrations, the structure and geological history of the great mountain-masses of the globe will be discussed. The work will also contain many maps and sections of mountain-ranges, and a contoured map of the North Atlantic Ocean, together with numerous sketches of mountain-structure and scenery, from Nature, by the author.

Prof. R. H. Richards, of Springfield, Massachusetts, has invented an ingenious and effective application of the zoëtrope for the illustration of the relation between certain isomeric forms. The apparatus exhibits the gradual passing of a cube into an octahedron, a dodecahedron, &c. It can be used also for exhibiting the growth of hemihedral forms of crystals.

A project is on foot for tunnelling the "Great Divide." The Divide is the Rocky Mountains, and the point proposed to be tunnelled is under Gray's Peak, which rises no less than 14,441 feet above the level of the sea. At 4441 feet below the peak, by tunnelling from east to west for 25,000 feet direct, communication would be opened between the valleys on the Atlantic slope and those of the Pacific side. This would shorten the distance between Denver, in Colorado, and Salt Lake City, in Utah, and consequently the distance between the Missouri River, say at St. Louis, and San Francisco, nearly 300 miles; and there would be little more required in the way of ascending or descending or tunnelling mountains. Part of the work has already been accomplished. The country from the Missouri to the foot of the Rockies rises gradually in rolling prairie till an elevation is reached of 5200 feet above the sea-level. The Rockies themselves rise at various places to a height exceeding 11,000 feet. Of the twenty most famous passes, only seven are below 10,000 feet, while five are upwards of 12,000 feet, and one, the Argentine, is 13,000 feet. Of the seventy-three important towns in Colorado, only twelve are below 5000 feet, ten are over 10,000 feet, and one is 14,000. The point from which it is proposed to tunnel is 60 miles due west from Denver, and although one of the highest peaks, it is by far the narrowest in the great backbone of the American continent.

At a meeting on Tuesday of the Committee of the subscribers to the British School of Archæology at Athens, Prof. Jebb said the School had been erected and paid for, Mr. F. C. Penrose had been appointed Director, and a provisional income of 400*l.* a year for three years had been raised; but additional funds were required. Prof. C. T. Newton, in urging the importance of having a great School of Archæology, suggested that there should ultimately be raised a special fund for the payment of the travelling expenses of the students at Athens. On the motion of Prof. Jebb, a Managing Committee was appointed.

On Saturday last, after six in the evening, several shocks of earthquake were felt in Alsace and many other parts in the nearest Alpine valleys, in the Vosges, and in the Black Forest. At Strasburg there had not been any earthquake for almost 200 years. The direction of the movement was from north to south. Several of the shocks were rather severe.

A telegram from Halifax, October 14, states that a shock of earthquake lasting ten seconds had been felt at Sydney, Cape Breton Island, Canada.

The Rev. J. H. Abrahall writes from Combe Vicarage, Woodstock, that on September 30, while near Blenheim Park, walking westwards about 7 p.m., he was aware of a sudden splendour above him on his left. Turning towards it, he saw a meteor moving eastward. It pursued a level path beneath the Milky Way about a sixth of the distance from the zenith to the horizon. It was bigger than Jupiter, and of a yellow hue. A reddish flame curled, crest-like, backward from its top. Its course was slow, and it quenched the light of the stars it passed over. It soon vanished, leaving no luminous train.

We have already referred to two successive instalments of Mr. Taylor's papers on the aborigines of Formosa, which have appeared in the *China Review* of Hong Kong. The third and last is now before us. It first describes the Pepo-huans, who are really not aborigines at all, but who stand between the savages and the Chinese settlers, speaking the language of both. They are believed by themselves and the other tribes to be the result of mixed marriages between whites and Chinese, who have traded to the country or been wrecked on the coasts, and aboriginal women. They are a simple, harmless, peaceful people. The Diaramocks, who inhabit the mountain-ranges which form the north-western boundary of the Tipuns (already described), are a fierce and intractable race, addicted to cannibalism. Little is known about them, as they hold aloof from other tribes. They are supposed to be the true aboriginal inhabitants of Formosa. There is also said to be a tribe of red-haired savages, living amongst the central mountains, but no authentic information has been gained respecting them. The inhabitants of Formosa generally are described as inquisitive and intelligent. The Chinese have a proverb to the effect that when the savages take to wearing trousers there is no opening

left for a Chinaman. The custom of head-hunting which prevails in some districts is mainly intended to prove the valour of the young men. On the whole, Mr. Taylor has given us a large number of details, classified under various tribes, which were not known before.

IN the September number of the *Meteorologische Zeitschrift*, Prof. Hann gives the results of many years' observations on the temperatures of the various parts of the Vienna forest. The forest valleys have a considerably lower temperature than the open land outside. This difference is smallest in winter and greatest in summer. But there is no similar effect during the course of the day, for the afternoon difference is not the greatest. It is actually least in the warmer hours of the day, and greatest in the cooler part. In the early morning and evening the influence of the forest in lowering the temperature of the air is greatest.

THE last number (vol. lv. Part 2, No. 2, 1886) of the *Journal* of the Asiatic Society of Bengal has for its first paper a list of butterflies taken in Kumaon, a district in the middle portion of the Himalayas, lying between Garhwal and Nepaul, by Mr. Doherty, of Cincinnati, who spent several months towards the close of last year in the expedition. Mr. Doherty's failure in the higher regions bordering on Tibet leads him to advise entomologists that the three summer months are the only good ones for collecting, either on the desert plains of Tibet, or in the deep valleys of the Himalayas sheltered by the outer range from the violence of the monsoon rains. Dr. Barclay contributes two papers—one on a second species of Uredine affecting the Himalayan spruce-fir (*Abies Smithiana*, Forbes), the first being described in the first paper of the present volume; the second paper by the same writer relates also to new species of Uredine parasitic in the deodar (*Cedrus deodara*, Loudon). Mr. Atkinson concludes his six papers on the Indian *Rhynchota*, which have been compiled in order "to provide those who may become interested in this order of insects with some guide to their classification and arrangement, and was at first devoted to the correction of our only English list, but this became so unsatisfactory that it was found better to revise the whole on the basis of Stål's numerous and elaborate essays." The number of species described under each family is: *Cicadidæ*, 115; *Cercopidæ*, 67; *Membracidæ*, 33; *Jassidæ*, 38; *Fulgoridæ*, 204. These figures, Mr. Atkinson adds, could probably be doubled in a few years, for the number of *Jassidæ* alone awaiting examination should add several hundred species to the Indian fauna. Dr. King adds three apparently new Primulas from the higher parts of the Eastern Himalayas to the *Primulaceæ* of Sir Joseph Hooker in his "Flora of British India." Finally M. de Nicéville has a paper "On the Life-History of Certain Calcutta Species of *Satyrinæ*, with special reference to the Seasonal Dimorphism alleged to occur in them."

ANOTHER consignment of German carp (*Cyprinus carpio*) is to be imported by the National Fish Culture Association to meet the growing demand for this species, which is superior to its congener of this country. The Association, through its secretary, Mr. Oldham Chambers, has urged upon those possessing disused waters to introduce the carp, both the mirror and leather species, therein. Although the German carp belongs to the same genus as the English, the former is being imported to improve the latter, which have deteriorated in flavour and condition owing to lack of cultivation. The fish will be committed to the charge of the Marquess of Exeter's pisciculturist, who has proceeded to Germany for the purpose of bringing them over, together with other species.

AN amusing incident occurred last week at the Colonial and Indian Exhibition Aquarium, where a remarkable raven from the Isle of Mull is now on view. On being fed it is the habit of this bird to hide the remnants of its repast in various parts of its habitat, and exhume them when prompted by hunger to renew the meal. One day a rat invaded the spot, and commenced to excavate for the hidden articles of consumption. Enraged at this proceeding, the raven fell upon the rodent, and gored it to death after a severe struggle on both sides.

MESSRS. WHITTAKER AND CO. will issue in the course of next week "On the Conversion of Heat into Work, a practical Hand-book on Heat Engines," by Mr. Wm. Anderson, M.I.C.E.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♂) from India, presented by Mr. T. L. Brewer; a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Mr. Walter C. Horsley; a Bonnet Monkey (*Macacus sinicus* ♂) from India, presented by Mrs. Samuel Lloyd; a Common Squirrel (*Sciurus vulgaris*), British, presented by Miss F. Westrup; two Grey Seals (*Halichœrus grypus*) from the Island of Canna, N.B., presented by Mr. R. Thom; a Varying Hare (*Lepus variabilis*) from Perthshire, presented by Mr. T. West Carnie; a Goshawk (*Astur palumbarius*) from France, presented by the Baron D'Epremesnil; a Golden Plover (*Charadrius pluvialis*), British, presented by Mr. G. Smith; ten Common Vipers (*Vipera berus*), British, presented by Mr. C. F. McNiven; two Black-footed Penguins (*Spheniscus demersus*) from South Africa, purchased.

OUR ASTRONOMICAL COLUMN

THE BINARY STAR τ CYGNI.—Mr. J. E. Gore (who has taken up this branch of astronomy with great vigour) has published in the *Astronomische Nachrichten*, No. 2749, elements of the orbit of τ Cygni. Using the measures of Dembowski, Burnham, Frisby, and Tarrant, he finds:—

$P = 53{\cdot}87$ years	$\Omega = 83^\circ\ 0'$
$T = 1863{\cdot}99$	$\lambda = 205^\circ\ 26'$
$e = 0{\cdot}3475$	$a = 1''{\cdot}19$
$\gamma = 44^\circ\ 40'$	$\mu = -\ 6^\circ{\cdot}68.$

These elements represent the observations fairly well. It must be remembered, however, that the measures only extend over a period of ten years, and the orbit must therefore be considered, as Mr. Gore states, to be provisional only.

THE LICK OBSERVATORY.—We learn from *Science*, vol. viii. No. 190, that the following plan has been devised by Prof. Holden for the working of the great telescope:—"We mean to put the large telescope at the disposition of the world by inviting its most distinguished astronomers to visit us one at a time, and to give to them the use of the instrument during specific hours of the twenty-four. Each day there will be certain hours set apart when the Observatory staff will relinquish the use of the equatorial to distinguished specialists who will come from the United States and from Europe to solve or to attack some one of the many unsolved problems of astronomy. In this way we hope to make the gift of Mr. Lick one which is truly a gift to science, and not merely a gift to California and to its University."

COMET BARNARD (1886 *f*).—Dr. J. von Hepperger has published the following elements and ephemeris for the comet discovered by Mr. Barnard on October 4:—

$$T = 1886 \text{ December } 24{\cdot}3064 \text{ Berlin M.T.}$$

$$\left.\begin{aligned} \pi - \Omega &= 78^\circ\ 56'\ 20'' \\ \Omega &= 140\ 17\ 55 \\ i &= 93\ 33\ 52 \end{aligned}\right\} \text{Mean Eq. } 1886{\cdot}0.$$

$$\log q = 9{\cdot}91236$$

Error of middle place ($O - C$).

$$d\lambda \cos\beta = -\ 8 \qquad d\beta = -\ 1.$$

Ephemeris for Berlin Midnight

1886	R.A. h. m. s.	Decl. ° ′	Log Δ	Log *r*	Brightness
Oct. 22	11 16 13	3 57·6 N.	0·2961	0·1434	1·86
26	11 27 15	4 21·1	0·2747	0·1296	2·21
30	11 39 16	5 53·2	0·2520	0·1093	2·66
Nov. 3	11 52 28	7 1·7 N.	0·2281	0·0916	3·22

The brightness on October 6 is taken as unity.

10 SAGITTÆ.—Mr. S. C. Chandler has discussed, in the *Astronomische Nachrichten*, Mr. Gore's observations of this star, together with some of his own, and some observations made for the Harvard and Oxford Photometric Catalogues. The result of his inquiry is to give M = 1885 December 4d. 9h. 36m. G.M.T. + 8d. 9h. 11·0m. (E – 391), the duration of increase being 3·00d., and of decrease 5·38d., and the magnitude at maximum being 5·6, and at minimum 6·4. Mr. Chandler considers it likely that the period will prove to be within two minutes of the truth. Mr. Espin's value, however, is 1h. 28m. shorter.

THE OBSERVATORY OF RIO DE JANEIRO.—M. Cruls, in a communication to the Paris Academy of Sciences, states that the long-talked-of transference of the Rio Observatory is about to be commenced. The site chosen lies nearly on the same parallel as the present Observatory, but two minutes farther to the west. The Brazilian Observatory possesses, from its proximity to the tropic, an advantage over all others, in that for forty days in the year the sun's zenith distance does not exceed 1°. M. Cruls anticipates that in the new edifice he will be able to undertake, with success, observations of terrestrial magnetism, and of atmospheric electricity, and he would wish to set up a delicate seismograph for recording slight movements of the soil. He trusts also that the Observatory will bear its share in the great photographic survey of the heavens proposed by Admiral Mouchez.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *OCTOBER* 24–30

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on October 24

Sun rises, 6h. 41m.; souths, 11h. 44m. 16·6s.; sets, 16h. 47m.; decl. on meridian, 11° 50′ S.: Sidereal Time at Sunset, 18h. 59m.

Moon (New on October 27) rises, 2h. 37m.; souths, 9h. 19m.; sets, 15h. 48m.; decl. on meridian, 4° 45′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	8 22	12 47	17 12	18 30 S.
Venus	5 41	11 9	16 37	6 58 S.
Mars	10 44	14 37	18 30	23 30 S.
Jupiter	5 33	11 3	16 33	6 37 S.
Saturn	21 24*	5 26	13 28	21 18 N.

* Indicates that the rising is that of the preceding evening.

Oct.	h.	
30	10	Mars in conjunction with and 6° 5′ south of the Moon.

Variable Stars

Star	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·2	81 16 N.	Oct. 24,	4 51	*m*
			,, 28,	4 30	*m*
R Ceti	2 20·2	0 42 S.	,, 29,		*M*
Algol	3 0·8	40 31 N.	,, 24,	23 3	*m*
			,, 27,	19 52	*m*
λ Tauri	3 54·4	12 10 N.	,, 25,	22 30	*m*
			,, 29,	21 22	*m*
U Ophiuchi	17 10·8	1 20 N.	,, 24,	1 19	*m*
			and at intervals of	20 8	
β Lyræ	18 45·9	33 14 N.	Oct. 30,	21 30	*m*
S Vulpeculæ	19 43·7	27 0 N.	,, 28,		*M*
R Sagittæ	20 8·9	16 23 N.	,, 29,		*m*
δ Cephei	22 24·9	57 50 N.	,, 25,	2 30	*M*
			,, 28,	21 30	*m*

M signifies maximum; *m* minimum.

Meteor Showers

The present week offers fewer active radiants than the one just past. The following radiants are, however, represented:—Near β Canis Minoris, R.A. 105°, Decl. 12° N.; and from Cancer, R A. 133°, Decl. 21° N. Both yield swift meteors, especially the latter. October 24 and 29 are fireball dates.

GEOGRAPHICAL NOTES

THE last volume of the *Izvestia* of the Caucasus Geographical Society (vol. viii. 2) contains a great variety of geographical information. General Stebnitzky contributes a most interesting paper on the figure of the earth, being a discussion of results obtained from pendulum-observations in connection with the opinions expressed by M. Faye. An excellent map of the province of Kubañ, on a scale of 13 miles to an inch, is accompanied by a sketch of the colonisation of the province, which already has a Russian population of more than one million inhabitants. M. Koshkul gives a short description of the "Naphtha Mountain," in the Transcaspian region. The telegraphic determination of the longitudes of Tiflis, Shemakha, and Baku acquires the more interest, as it allows of the determination of the general error (14″·3) of the Caucasian triangulation and the deviations from the vertical line, due to local causes at these three places. A list of points whose positition has been determined by the triangulation made in the Transcaspian region, as also in Khiva and Bukhara, is given by MM. Pervas and Gedeonoff, and will be most welcome to cartographers. Among the notes we notice the following:—On the Caucasians of Kubañ, due to M. Zagursky, whose researches on the languages of the Caucasus are always so great a help to ethnography; a list of the Caucasian population in Kubañ in 1883, from which we learn that from Kubañ alone no less than 13,600 Circassians have emigrated since 1871; M. Chantre's craniological measurements are summed up by M. Zagursky; and M. Lessar contributes a paper on the north-western frontier of Afghanistan. The "Appendix" shows where the chief attention of the Caucasus Geographical Society is now directed. They contain translations of a work dealing with Armenia (the "Toros Akhpar" guide through Armenia, by the Archbishop Garegin Srvandzyantz); of the Turkish "Salname" for the Erzeroum Vilayet; of notes on Syrian-Khaldeans, by a native from Hosrabad; and of Mr. Charles Wilson's lecture on Asia Minor, delivered before the London Geographical Society.

THE first fascicule of the full Reports of the Polar Meteorological Station at the mouth of the Lena has just been published. It is the first fascicule of the second part, and contains the meteorological observations made since September 1, 1882, to August 31, 1883, compiled by M. Eigner, and published under the supervision of Dr. R. Lenz; the second fascicule of the same volume will contain the meteorological observations in 1883–84; while the first volume is reserved to magnetical observations, and the third will be devoted to the non-obligatory observations, among which the auroræ will occupy a prominent place. The meteorological observations now published, comprise the pressure of air, the temperature, elasticity of vapours, relative moistness, force and duration of wind, nebulosity, snow and rain, as also the temperature on the surface of the soil and the snow, and at depths of 40, 80, and 160 cm., these last two missing for the months of July and August 1883, in consequence of an accident to a thermometer. All observations are given in full, that is, for every hour, as also the monthly averages. The daily range of all elements is also represented by curves, whose scale is exactly that accepted for the publication of the French observations at Cape Horn; the work is accompanied, moreover, by a map of the mouth of the Lena, and of the station itself, as also by a drawing representing the station amidst the tundra, on the banks of a branch of the Lena. A full description of the instruments and their corrections is given both in Russian and in German.

THE October number of the *Proceedings* of the Royal Geographical Society has for its leading paper one by Sir Francis de Winton, on the Congo Free State, the conditions of its administration by Europeans, and its probable future. In this latter respect Sir Francis de Winton is naturally inclined to look on the favourable side. Of more strictly geographical interest is the letter from Mr. Grenfell recounting his latest explorations in the missionary steamer *Peace*, of the tributaries of the Congo, between Leopoldville and Stanley Falls, with very detailed maps.

Petermann's Mitteilungen for October contains a long paper by M. Nikitin, the chief geologist of the Russian Geological Committee on Glaciers in Russia. His object is to lay down the limits of the traces of glacial action in that country. Lieut. von François's journeys in the southern Congo basin are the subject of the next paper; and, finally, there is a brief account

of the Cunos or Tulé Indians of Darien, from the reports of a missionary sent to labour among them to Bishop Thiel, of Costa Rica, who has communicated them to Herr Polakowsky.

THE current *Mittheilungen* (Band xxix. Nos. 7 and 8) of the Geographical Society of Vienna contains letters from Dr. Lenz and Herr Baumann, from the Congo, and part of an account by Dr. Holub of his present journeyings in South Africa. Of special interest are two papers on the Hauslab cartographical collection. One describes the general extent and contents of the collection, which is in three parts: (1) books; (2) engravings; (3) maps. The last contains about 4500 sheets, and is especially rich in old specimens of cartography. Two of these form the subject of a second paper: they are a globe, the author of which is unknown, but which probably dates from soon after Columbus; the other is a chart of the Mediterranean dated 1513.

THE FIFTY-NINTH MEETING OF GERMAN NATURALISTS AND PHYSICIANS, BERLIN, SEPTEMBER 18–27

[FROM OUR BERLIN CORRESPONDENT]

THE present has in every respect been the most important of these annual meetings. Even in the number of visitors it far exceeds any similar reunion since the foundation of the Society, as many as 2224 members, and 1931 associates, or 4155 altogether, having entered their names, while no less than 1496 ladies took part in the general proceedings and social gatherings. It may here be remarked that the constitution of the German Naturalists' Society differs essentially from that of the British Association, as it exists only so long as the meeting lasts, and consists of members—that is, of persons who have published treatises, other than dissertations for academical honours, on general scientific and medical subjects—and of ordinary associates. Two or three sittings are devoted to the formal proceedings, such especially as the choice of the following year's place of meeting, and of the two leaders, whose duty it is to summon the next assembly, and arrange the work on hand. The general sittings are occupied with matter of universal interest, and are attended in common by all members, associates, and ladies. The more strictly scientific work, however, is distributed amongst the several Sections, thirty on this occasion, each of which is presided over by a freshly elected chairman, and set apart for the discussion of papers by specialists. After the last general sitting, at which the annual meeting is officially dissolved, it ceases for the time being to exist. Enjoying no special source of income, and keeping no permanent records, it possesses in the officers appointed to arrange for the next gathering the only germ of a new and equally ephemeral existence. To this temporary organisation corresponds the manner in which its proceedings are issued. An official journal, published only while the gatherings are held, contains the proceedings of the general sittings *in extenso*, those of the various Sections in shorter or more detailed reports. It may here be mentioned that, at the suggestion of the administration, a Commission was on this occasion appointed for the purpose of reporting to next year's meeting any proposals for a modification of the statutes. Reference was made more especially to such a change as would confer on the German Naturalists' Society a more stable existence; in fact, an organisation somewhat similar to that of the British Association.

Of the 4155 members and associates present, Berlin was represented by 1444, other places by 2711 (including 429 foreigners), as under: Europe, 347; America, 54; Asia, 18; Africa, 6; Australia, 4. Most of the leading representatives of German science were present, although illness unfortunately prevented the attendance of Von Helmholtz, Kirchoff, and Wislicenus, the first and last of whom had undertaken to deal with some matters of general interest.

Under the presidency of the two administrators, Prof. Rudolf Virchow and Prof. A. W. Hofmann, the first sitting was held on Saturday, September 18, when an audience filling the spacious Circus Renz was addressed by Herr Virchow on the development of the Society from modest beginnings, and its present importance for the mutual interdependence of the various branches of the natural and therapeutic sciences. He dwelt on the progress made since the first meeting in Berlin, in 1828, under the presidency of Alexander von Humboldt, which had been attended by Oerstedt, Berzelius, Gauss, Weber, Johannes Müller, Mitscherlich, Rose, Magnus, Ehrenberg, but from which Goethe had absented himself. The importance of these illustrious *savants* for the development of the physico-chemical and biological sciences, and the continuity of their researches with the problems now under discussion, formed the conclusion of this highly instructive opening address. After receiving the felicitations of the representative of the Minister of Public Instruction, Von Gossler, of the Berlin Oberbürgermeister, and the Rector of the University, and after the meeting had made choice of Wiesbaden for next year's gathering under Prof. Fresenius and Dr. Pagenstecher, Dr. Werner Siemens discoursed on "The Scientific Character of the Age." The speaker dilated on the spread of the natural sciences through these periodical gatherings and through their introduction into the school-room, thus influencing the *technique* of the arts, which in their turn react powerfully on the social relations, so that, by his command of the forces of Nature, man is now enabled to produre the necessaries and the pleasures of life in greater abundance with less expenditure of time and labour. He is certainly not able to overcome all the evils inherent to the present period of transition; but our scientific age promises to discover all the remedies calculated to alleviate and cure the ills from which we now suffer.

At the second general sitting, held on September 22, Prof. Pohlmann, of Buffalo, conveyed the greetings of the American Association to the German Naturalists, together with a general invitation to the International Medical Congress to be held next year in Washington. Prof. Ferdinand Cohn, of Breslau, then spoke on "Vital Questions," dealing with the nature of life from the present scientific stand-point, in reference more especially to the simplest living beings, the lowest plants, whose vital functions he described in attractive language. He considered that we had already half solved the riddle of life, inasmuch as we had grasped its mechanism and the physical and chemical forces which set it in motion. But we have to face other phenomena and active forces, which must be clearly fathomed by the more fortunate efforts of future research, so that the full solution of the problem of life may perhaps be deferred to a remote period. The next speaker was Herr George Schweinfurth, from Cairo, who spoke on "Europe's Mission and Prospects in Central Africa," arguing energetically for the possibility and necessity of colonising that region. He described the wealth of this continent, both in natural resources and available labour, which by colonisation alone could be properly utilised in order not only to insure a happy and worthy future for the natives of Africa, but also to open a wide field of fruitful activity for the already crowded populations of Europe. He warmly combated the assumptions that its tropical climate closed the door of Africa to most Europeans, and that acclimatisation was impossible. Under certain precautionary measures, and when Western culture has reclaimed Africa by railways, draining, disafforestation, tillage, and stock-breeding, just as Europe itself has been reclaimed by the hand of man, then the white race will find itself as much at home in Africa as the Negro. After the transaction of some formal business, Prof. His, of Leipzig, addressed the meeting on "The Development of the Zoological Station at Naples, and on the Growing Urgency of a Scientific Central Establishment." From the information gained by repeated personal visits, he gave a vivid description of the Neapolitan Station, and concluded by indicating the chief objects of such a central institution as he considered should now be founded. Amongst these objects he mentioned the investigation of the anatomy of the brain, which could be best carried out in such a central station.

At the third general sitting, held on September 24, Dr. Ludwig Wolf, of Dresden, reported on his journey to Central Africa, describing his route from Leopoldville, on the Kassai, and its copious affluent, the Sankuru, through the domain of the Bakutu, the Bakuba, the Balula, and Lunda peoples. He gave an animated picture of his experiences on this journey, which he made as a member of the Wissmann Expedition. He was followed by Prof. Neumayer, of Hamburg, who urged the necessity of Antarctic exploration, dilating especially on its importance for geology and palæontology. From it he anticipated an answer to the question how, as he assumed, the South Pole had been a centre of dispersion for living organisms throughout the southern, as the North Pole is now generally supposed to have been for the northern hemisphere. He further dwelt upon the great value of terrestrial magnetic observations in high southern latitudes, since in the far north a whole chain of stations had recorded the extent of the fluctuations of the magnetic needle. Simultaneous observations in the north and south will alone enable us to arrive at definite conclusions on the nature of

terrestrial magnetism and its relations to the earth-currents, Polar lights, and solar energy. Prof. Bergmann, of Berlin, followed with some remarks on the relations of modern surgery to the treatment of internal ailments. After some formal proceedings, the third general sitting, and with it the fifty-ninth gathering of the German Naturalists and Physicians, were brought to a close.

In a brief report of this nature it would be impossible to do more than refer in the most summary way to the work done in the several Sections, of which twelve were devoted to scientific and eighteen to medical subjects. Altogether 522 topics were discussed, and 155 demonstrations carried out. Most of the proceedings will be published in special scientific journals, and here it will suffice to mention more especially the remarkable synthesis of coniine, the poisonous alkaloid of hemlock, effected with surprising success by Prof. Ladenburg. Thanks to this achievement, the artificial production of a vegetable alkaloid may now for the first time be regarded as successfully accomplished. In the physiological department the question of the localisation of the cerebral functions gave rise to an animated discussion, in which Profs. Hitzig, Munth, and Soltz took part. In the section devoted to the subject of scientific instruction, Prof. Haeckel pleaded strongly for a severer training in this branch of knowledge amongst young students. It may be mentioned in conclusion that, in connection with this meeting, an exhibition of scientific instruments, apparatus, and educational appliances was held in the apartments of the Academy of Arts and Sciences. There was a good show of instruments of precision, microscopes, electric, medical, and other appliances, which attracted a large number of visitors during the few days the exhibition lasted, from September 16 to 26.

THE HARVEIAN ORATION

DR. PAVY, F.R.S., delivered the Harveian Oration at the Royal College of Physicians on Monday afternoon. After giving the directions marked out by the founder of the Oration—viz., to commemorate the benefactions that have fallen into the possession of the College and to search and study out the secrets of Nature by way of experiment—the orator alluded to the augmentation which the income from the endowment of the Croomian Lectureship has recently undergone, by which the amount available is raised from 10*l.* to 200*l.* per annum; and to the sum (2000*l.*) bequeathed by the late Dr. Gavin Milroy. He next spoke of the course pursued by Harvey as set forth by Lord Bacon, in his "Novum Organum," or "true directions concerning the interpretation of Nature." Instead of giving himself up, as others had done before him, to arguing out conclusions from accepted axioms, Harvey struck out, Dr. Pavy continued, into the hitherto untrodden path of inquiry—that of induction—and sought knowledge by a direct appeal to Nature through the medium of observation and experiment. "It were disgraceful," he says, "with this most spacious and admirable realm of Nature before us, did we take the reports of others upon trust, and go on coining crude problems out of these, and on them hanging knotty and captious and petty disputations. Nature is herself to be addressed, the paths she shows us are to be boldly trodden." In the discovery of the circulation Harvey applied the principles of induction and argued upon them in a strictly logical way. He showed himself to be a good and careful observer, judged even by the standard set forth by John Stuart Mill on the process of observing. The experiments which Harvey conducted on the arteries and veins, to assist him in his inquiry, were founded upon a well-devised plan. Dr. Pavy next spoke of the new departure in physiology which Harvey's discovery established, of the opposition with which his views were received, and remarked that the high position in his profession he had attained did not suffice to secure his escape from the effect of the prejudice against innovation entertained by the multitude. Aubrey tells us he had "heard him say that after his book on the circulation of the blood came out he fell mightily in his practice; 'twas believed by the vulgar that he was crack-brained, and all the physitians were against him." Harvey lived, however, to see his doctrine generally accepted. The orator next referred to one issue of research derived, he said, from the labours of the present day, which has already yielded much good and useful fruit and gives promise of yielding much more. "Belonging to the realm of living Nature there are," he continued, "small organisms, the existence of which we must have remained unconscious of in the absence of the aid of the microscope. These bodies are known by the name of bacteria or bacilli, and, while some difference of opinion has existed, it is generally thought that they are organisms belonging to the vegetable kingdom. There is nothing in their appearance to strike the observer that they possess any significance, and yet by recent research it has been found that they play a most important part as constituents of the living world." The experiments of Spallanzani, Schulze, and Swann, were next described by Dr. Pavy, the natural conclusion to be drawn from which, he said, "goes far towards absolutely establishing that the air contains the germs of living organisms, and that it is these that constitute the source of the microscopic organisms found to become developed in the presence of organic matter, which some have contended take rise spontaneously. This view is supported by the researches of the present day, and nothing that would bear the scrutiny of strict investigation has ever been adduced against it. It stands at the foundation of our modern notions regarding the *rôle* played by bacilli, and thus occupies a position of weighty importance with reference to the matter. The step from the action exerted by bacteria as agents exciting the decomposition of organic products to that which brings them before us as a source of disease is not a large one. In the one case they lead to change which would not otherwise occur, and in the other they disturb the order of changes naturally taking place and thus induce an abnormal state; and although there is nothing in their morphological characters to show the reason, different trains of phenomena—in other words, different diseases—are occasioned by different kinds of bacilli. . . . Through the indefatigable researches of Pasteur and others the distinguishing form and life-history of certain of these organisms have been clearly made out. Placed under suitable conditions, it has been found that they can be reared or cultivated artificially, and one of the most marked and important characters belonging to them is the enormous extent of self-propagating power they possess. This accounts for the rapid spread that is observed to take place of an infectious disease, if allowed to progress without controlling measures being brought to bear upon it. We have to deal, then, with something that lives and grows by virtue of a power pertaining to itself. Permit this living growth—this parasite, in fact—to become dispersed and to enter the system of a living person, and presuming it has lodged upon a soil supplying suitable conditions for its development, it will thrive and multiply and give rise to a series of phenomena which the physician has no power to arrest. Once the bacillus is implanted and the disease established, all that the physician can do is to see that the patient has fair play—that he is kept under the most favourable conditions for battling successfully against his enemy. What is to be philosophically aimed at, however, is to check the spread—to bar the transmission of the parasite from one person to another, by attacking it outside the body; and this, with the application of the proper measures of disinfection, can with facility be done, but naturally the facility of preventing extension stands in proportion to the degree of limitation at the time existing. The spark of fire is with the greatest ease extinguished, but let it kindle into flame, and in proportion as the flame spreads the difficulty becomes greater to get the conflagration under. This is one way in which the attack upon the bacillus may be made, and the ravages of disease restrained. Another way, by quite a different line of tactics, presents itself; and the knowledge of this is due to the researches that have been recently conducted. The vulnerable point to which I am alluding lies not in connection with the bacillus itself, but with the condition of the medium upon which it may chance to fall. It has been found that the parasite requires virgin soil for its growth. This observation stands in harmony with the result of common experience as regards disposition to contract infectious disease. It has been from remote times generally known that a person who has passed through one attack of an infectious disorder is not liable to the same extent as before to become affected on exposure to contagion. An influence has been exerted giving rise to more or less protection being afforded against a recurrence of the disease. Now it happens that by certain means the bacillus may be brought into such a weakened state as only to occasion, when introduced into the system of an animal, an effect of a mild nature, not dangerous to life, instead of the ordinary form of disease; but the effect produced, and this is the great point of practical importance, is as protective against a subsequent attack as the fully-developed disease. There are two methods by which attenuation in virulence of the disease-producing organism may be brought about—

by conducting their artificial cultivation in a particular way, and by transmission through the system of an animal differing in nature from that in which the disease naturally occurs. When the chain of discoveries reached the point of showing that bacilli could be reared outside the body in an artificial soil or cultivating medium, a great advance was made towards obtaining a full knowledge about them, as it placed the observer in a more favourable position for the successful prosecution of research by enabling him to vary and control his conditions in a manner that could not otherwise have been effected. Although much has been accomplished, it must be said much still remains to be done. In the case of a few bacilli the life-history has been pretty clearly made out. Cultivated in a certain way they retain their virulence, no matter through how many successions they pass. The last product in a series of successive cultivations is as virulent as the parent stock. By modifying the conditions under which the cultivation is carried on, the successive products of descent may be gradually weakened until they become harmless. Such being the case, any desired degree of attenuation may be obtained, and by inoculation with a virus brought down to the proper strength the non-fatal affection may be occasioned which gives immunity from subsequent liability to take the disease under exposure to contagion. The knowledge thus acquired has been already practically turned to account upon a large scale for checking the ravages of that exceedingly fatal disease among cattle known as anthrax, or splenic fever, and through the success attained much sacrifice of life has been averted. If this can be accomplished for one disease, and more than one can be mentioned, is there not ground for believing that means will be found for placing others of the class in the same position? Attempts are being made in this direction. All eyes throughout the civilised world are, indeed, at the present moment fixed upon the work of Pasteur in Paris with reference to hydrophobia. It would be a great achievement for this frightful disease to be brought under subjection, and certainly the results that have been obtained appear to give hopes that an approach to something of this kind has been arrived at. Looking at the nature of the disease, there is nothing inconsistent with its being dependent upon a bacillus, or microbe, as Pasteur calls it. On the contrary, owing its origin as it does, when occurring naturally, to inoculation with the poisoned secretion of an affected animal, and taking into view the facts that have been learnt in connection with its transmission by artificial inoculation, evidence points to such in reality being the case. If due to a bacillus, why may not this bacillus be open to attenuation in the same manner as that of anthrax? If thus open to attenuation, why not susceptible of producing a non-fatal form of affection? And if this condition has been produced and passed through, why should not protection be thereby given against the subsequent development of the disease as a result of the primary inoculation from the bite of the rabid animal? Such a train of reasoning is quite legitimate, and for the application of the principle of action to which it leads, there is this advantage on the side of hydrophobia, that from the prolonged period usually taken for incubation after the introduction of the poison in the ordinary way, time is given for the artificial inoculations by subcutaneous injection to produce their effect and to render the system refractory to the further development of disease. I have been an eye-witness of Pasteur's work. It is from the nerve centre, the seat from which the symptoms of the disease start, that he obtains his virus. Employed for inoculation in a fresh state it produces a fatal disease, and the disease has been transmitted successively on through a number of animals, with the result that the last affected animal yields as strong a virus as the first. Kept in a pure, dry air, attenuation advances, and after a certain time the nerve centre loses its disease-producing power. Used for inoculation at a given period of preservation it produces an effect which renders an animal resistant to the influence of inoculation with the virus in a fresh state, and Pasteur contends that it acts similarly when the virus has been introduced in the ordinary way. The treatment of persons bitten by rabid animals by inoculation with attenuated virus has now been on its trial a considerable time, and a large experience gained. Judgment, it must be stated, still stands in suspense; but it must also be said that the results obtained tell decidedly in favour of the view advanced. The other method by which it has been recently experimentally found that the virulence of bacilli can be weakened is by transmission through an animal of a different nature from that in which the disease naturally occurs. This, in reality, represents the principle at the foundation of the system of vaccination, discovered by Jenner at the close of the last century. It may now be regarded as an accepted conclusion that vaccine-lymph is the virus of small-pox modified by transmission through the cow. Jenner's discovery consisted in showing that the result of vaccination with the lymph of cow-pox affords as much protection against small-pox as an attack of small-pox itself. This was the fact he educed, but the knowledge possessed in his time did not permit of its being looked at in any further way than as a simple fact or truth of Nature. Viewed, however, with the light that has been thrown upon it by the researches of the present day, we see not only the fact, but also its explanation: we see that the principle of action of the procedure proposed by Jenner, which has conferred such incalculable benefit upon mankind, is based upon the attenuating effect upon the small-pox virus of the human species by transmission through another animal; and knowing this, the prospect is presented of its being rendered susceptible of application for the control of other diseases. Whether this should prove so or not, at all events advantage is gained by the knowledge acquired. Need I say anything more to exhort you, in accordance with the duty that has devolved upon me? Surely the acquirement of knowledge, giving us as it does greater power in the exercise of our calling, and thereby promoting the high and noble object of rendering our lives more useful to our fellow-creatures—surely this is a sufficient incentive, following the words of Harvey, 'to search and study out the secrets of Nature by way of experiment.'"

NOTE ON THE ASTRONOMICAL THEORY OF THE GREAT ICE AGE [1]

THE following calculation has convinced me that Mr. Croll's theory affords an adequate explanation of the Ice age. I compute the total quantity of heat received by each hemisphere of the earth during summer and winter respectively as follows:—

Let $2H/a^2$ be the quantity of sun-heat falling perpendicularly on an area equal to the section of the earth at the mean distance a from the sun in the unit of time.

Let δ be the sun's north declination. Then the share received by the northern hemisphere will be

$$\frac{H}{a^2}(1 + \sin \delta),$$

and by the southern

$$\frac{H}{a^2}(1 - \sin \delta).$$

At the distance r, and in the time dt, the heat received in the northern hemisphere will be

$$\frac{H}{r^2}(1 + \sin \delta) \,.\, dt;$$

but we have

$$r^2 d\theta = h dt,$$

whence the expression becomes

$$\frac{H}{h}(1 + \sin \delta) \,.\, d\theta;$$

but we have

$$\sin \delta = \sin \theta \,.\, \sin \epsilon,$$

where ϵ is the obliquity.

The total heat received by the northern hemisphere from the vernal to the autumnal equinox is

$$\int_0^\pi \frac{H}{h}(1 + \sin \epsilon \sin \theta) \,.\, d\theta = \frac{H}{h}(\pi + 2 \sin \epsilon).$$

We have thus the following theorem:—

Let $2E$ be the total sun-heat received in a year over the whole earth; then this is divided into shares as follows:—

$$\text{Northern hemisphere, summer, } E\frac{\pi + 2 \sin \epsilon}{2\pi},$$

$$\text{,, winter, } E\frac{\pi - 2 \sin \epsilon}{2\pi};$$

with identical expressions for the summer and winter in the southern hemisphere.

[1] Paper read at the Royal Irish Academy on May 24, 1886, by Sir Robert Stawell Ball, LL.D., F.R.S. Communicated by the Author.

If we make $\epsilon = 23^\circ 27'$ we find that the heat received during the summer (equinox to equinox) of each hemisphere is $\cdot 627\,E$, while the heat during the winter of each hemisphere is $\cdot 373\,E$. More briefly still. If each hemisphere receives in the year a quantity of sun-heat represented by 365 units, then 229 of these are during summer, and 136 during winter. These figures are independent of the eccentricity of the earth's orbit.

The length of the summer is defined to be the interval when the sun's centre is above the equator. The length will of course vary with the eccentricity and with the position of the equinoxes on the orbit. We need only take the extreme case where the line of equinoxes is perpendicular to the major axis of the orbit. The maximum difference between the length of summer and of winter is thus

$$365 \text{ days} \times \text{eccentricity}.$$

I take the maximum eccentricity of the earth's orbit to be

$$0\cdot 0745,$$

this being the mean of the values by Leverrier, Lagrange, and Stockwell (see Croll, "Climate and Temp.," p. 531), and, therefore, the greatest difference between summer and winter will be about 33 days, *i.e.* one season is 199 days, and the other is 166 days.

The total quantity of heat received during the year on each hemisphere is practically independent of the eccentricity; but the mode in which that heat is received at the different seasons will vary, and thus give rise to the following extreme cases:—

GLACIAL

(Summer)	229 heat units spread over 166 days.	
(Winter)	136 ,, ,, 199 ,,	

INTERGLACIAL

(Summer)	229 heat units spread over 199 days.	
(Winter)	136 ,, ,, 166 ,,	

We hence deduce the following, where unity represents the mean daily heat for the whole year on one hemisphere:—

GLACIAL

Mean daily sun-heat in summer (short) ...	1·38
,, ,, winter (long) ...	·68

INTERGLACIAL

Mean daily sun-heat in summer (long) ...	1·16
,, ,, winter (short) ...	·81

PRESENT (NORTHERN HEMISPHERE)

Mean daily sun-heat in summer (186 days)	1·24
,, ,, winter (179 days)...	0·75

These figures exhibit a thermal force of great intensity. The unit represents all the mean daily heat received from the sun by which the earth is warmed up from the temperature of space. The heat unit in fact maintains a temperature perhaps 300°, or even more, above what the earth would have without that heat. Each tenth of a unit may thus roughly be said to correspond to a rise or fall of mean temperature of 30° or more. The long winter of 199 days, when the average heat is only two-thirds of a unit, leads to the accumulation of ice and snow, which form the Glacial epoch. The short winter of 166 days, where the temperature is ·06 of a unit above that of our present winter, presents the condition necessary for the mild interglacial epoch.

THE BRITISH ASSOCIATION

SECTION H—ANTHROPOLOGY

The Native Tribes of the Egyptian Súdan, by Sir Charles Wilson, K.C.B.—These may be divided into four distinct groups—the Hamitic, Semitic, Núba, and Negro; but the first three only were dealt with in this paper. The largest tribe in the Súdan is the Kabbabish. They extend from Dongola to the confines of Darfúr; they speak a pure Koranic Arabic, and have a tradition that they came from Tunis; they are possibly of Berber descent, but the Sheikhs are apparently of Arab origin. They are divided into two great branches and several minor clans. One clan, Kawahleh, appears to be of Arab origin.

The Celtic and Germanic Designs on Runic Crosses, by Prof. W. Boyd Dawkins.—The author said that although it is generally assumed by archæologists that the early Irish manuscripts, such as the illuminated Gospels of St. Cuthbert and St. Chad, are of pure Irish art, and that consequently the interlacing "rope-" or "basket-work" pattern is distinctly Irish and Celtic, such an assumption is not warranted by experience. A consideration of the distribution of the designs on ornaments and monuments in the British Isles and in France, Scandinavia, and Germany, lead to the conclusion that the art was probably derived from the centres of civilisation in South Europe, principally Greek and Etruscan, and it has clearly been proved by Chantre to have been introduced into France from Italy. The square interlacing pattern does not occur in France or the British Isles in association with any remains of a date anterior to the movement of the Germanic tribes against the Roman Empire, and as it is only found in regions into which the German tribes penetrated, it may be concluded that it is distinctly Germanic, and not Celtic, still less "pure Irish."

The Scientific Prevention of Consumption, by G. W. Hambleton.—There are two distinct objects to be accomplished in the prevention of consumption. On the one hand we have to secure an adequate amount of breathing capacity in proportion to the rest of the body, and on the other to prevent either compression of the chest or injury to the lungs. This can be done by adopting those measures that tend to the development of the breathing capacity, and suppressing or obviating those conditions that compress or injure the lungs. By adopting measures is meant placing men, women, and children under conditions of habitation, clothing, education, and urging upon them habits that tend individually and collectively to develop the lungs.

Dragon Sacrifices at the Vernal Equinox, by George St. Clair, F.G.S.—The object of this paper was to show that human sacrifice, which prevailed extensively in early times, was a custom connected especially with the vernal equinox, and that the offerings were made to appease a mythical dragon which made its demand at that time. The dragon of mythology was identified and defined, and it was shown in what sense he opened his jaws at the spring season of the year. Human sacrifice was practised more especially at the spring of the year, or (in other instances) in honour of deities who once presided over equinox constellations. Artemis and Cronus, to whom this homage was chiefly shown, were both connected with the zodiacal sign Scorpio, and, according to M. Ernest de Bunsen, Scorpio was the starting-point of the primitive calendar. If the festival of Saturn did not get displaced or misplaced through the precession movement, it was still a festival in honour of the god of the under-world, and that meant death and the grave. Tradition says that human sacrifices were abolished by Hercules. As Scorpio rises with Hercules, and ceases to be a dark sign, the mythology is consistent with itself.

Evidence of Pre-Glacial Man in North Wales, by Dr. Henry Hicks, F.R.S. — The author in this paper described the conditions under which a number of flint instruments were discovered during the researches carried on by Mr. E. B. Luxmore and himself in the Ffynnon Beuno and Cae Gwyn Caves, in the Vale of Clwydd, in the years 1884–86. Last year a grant was made by the British Association for the purpose of carrying on the explorations, chiefly with the object of obtaining further evidence as to the age of the deposits in the caverns. The results obtained this year are highly confirmatory of the views which he (Dr. Hicks) had previously held, and have a very important bearing on the antiquity of man in Britain. It was found that the main entrance to the Cae Gwyn Cave had been blocked up by a considerable thickness of Glacial beds, which must have been deposited subsequently to the occupation of the cave by the Pleistocene mammals. A shaft was dug through these beds in front of the entrance to a depth of over 20 feet, and in the bone-earth, which extended outwards under the Glacial beds on the south side of the entrance, a small well-worked flint flake was discovered. Its position being about 18 inches beneath the lowest bed of sand, it seemed to be clear that the contents of the cavern must have been washed out by marine action during the great submergence in mid-Glacial times, and then covered by marine sand and an upper covering of boulder-clay. He believed that the flint implements, lance-heads, and scrapers found in the caverns were also of the same age as this flint flake, and hence that they must have been the work of pre-Glacial man.

The Recent Exploration of Gop Cairn and Cave, by Prof. Boyd Dawkins.—This was a paper on the exploration of Gop Cairn and Cave, near Gop Hall, New Market, St. Asaph, now being carried on by Mr. Pochin, Mr. P. G. Pochin,

and the author. The cairn commonly known as "Queen Boadicea's Tomb" was composed of blocks of limestone, about 40 feet high, 300 feet long, and 200 feet broad. A shaft was sunk near the centre of the cairn, but the only remains discovered were a few refuse heap bones of hog, sheep or goat, ox or horse, too fragmentary to be accurately determined. They were, however, of the character found almost universally in Britain in the burial-places of the Neolithic and Bronze Ages. The cairn itself was similar in character to one near Mold, in the same district, in which a skeleton was discovered in 1832 lying at full length, clad in a golden corselet, and adorned with 300 amber beads. An urn full of ashes and other remains was also met with. While the cairn was being attacked, a cave was discovered 141 feet to the south-west, and there were found bones and teeth of various animals which belonged to the Pleistocene age, and similar to those discovered in the caves of the Vale of Clwydd. Above these was found a deposit containing fragments of charcoal and large quantities of broken bones of wild and domestic animals. Slabs of limestone burned on their upper surface were also found, and pointed out the position of the fireplace. The date of this upper deposit was fixed by several fragments of pottery, which was in its characteristics similar to that of the Bronze Age. Besides these, a large number of human bones were found, increasing in number as the explorers dug their way to a square sepulchral chamber, 4 feet 10 inches by 3 feet 10 inches. This chamber was packed with human skulls and bones of all ages in the greatest confusion, and evidently interred from time to time. Among the bones were found two jet ornaments, a beautifully polished flint flake, with edges carefully bevelled, and some fragments of rude pottery of the kind commonly found in sepulchral urns of the Bronze Age. The chamber and the deposits showed that caves had probably been used for habitation and sepulture in North Wales in the Bronze Age, as they had already proved to have been used in the Neolithic Age. The human remains threw great light on the ethnology of the district in the Bronze Age, and proved that in the Neolithic Age the population of that part of Wales was of the oval-headed Iberic type, so widely spread throughout Europe. All the skulls were of this type save one, which possessed all the characteristics usually found in a round-headed Celt of the Bronze Age, and the presence of this skull in a sepulchre of the Iberic people appears to mark the beginning of the fusion of the two races, which has been going on ever since, and by which the Iberic type is at the present time being slowly obliterated.

On Bowls' Barrow, near Heytesbury, in South Wilts, by W. Cunnington.—These researches, the writer stated, had been made at the east end of the barrow, where the original cist had been found empty, but with a skull near it. Several other skulls were also found in a more or less broken condition. Covering the floor of the barrow near where the skeletons were found was a black unctuous earth, which had been found to contain a large quantity of ammoniacal salts. Separated from the cist at the east end of the barrow several horns of oxen had been found, in addition to those that were found there some years ago. The skulls and other human remains which had been found were clearly primary burials, and were covered by large blocks of Sarsen stone, some of which weighed from 200 lb. to 300 lb.

The Crania and other Bones found in Bowls' Barrow, by Dr. J. G. Garson.—The author said that the skulls are of large size, and long and narrow in form. In general outline they present two distinct forms, namely, the elongated oval, and what is called the coffin-shaped. They all conform in every respect to the long barrow type, and are all those of adult males.

Papuans and Polynesians, by the Rev. George Brown.—The object of this paper was to show that the two races had a common origin. Mr. Brown said he had worked for many years among the purest types of Polynesians and of Papuans, and in reducing the languages to writing he became convinced that, from the point of view of language and from their manners and customs, some of the difficulties in assigning to the two races a common origin were not so insuperable as they appeared. He considered that the basis of the Polynesian race was Papuan with an Asiatic admixture. The idea that cannibalism existed because of the love of animal food and the inability to gratify this appetite in any other way was all nonsense: in ninety-nine cases out of a hundred it was only practised as a means of revenge. The author gave a description of the etiquette and general manners and customs of the two peoples, and, summing up his argument, said the points of similarity were so much more numerous and marked than the points of difference that as they inquired further they would find no insuperable difficulty in giving them one common origin.

What is an Aryan? by Sir George Campbell, K.C.S.I.—The great difficulty which we had in distinguishing the Aryan was that the Aryan race was seldom pure. Almost all the Aryans we met with were a very mixed race, but by their features and colour they were easily distinguished from the Turanian and Negro races. The difficulty in distinguishing between the Aryans and the Semites lay in their features, and if the rather high features, which we called Jewish, were the real types of the Semites, what were the types of the Aryans? There were two distinct branches of Aryans with which he had long been in contact—the dark branch found in India and Asia, and the fair branch, which included the whole of Europe and Asia Minor. Then in part of Western Asia, in the Hindu Kush, there was a whitey-brown variety of the race, which might be classed as the intermediates, and this he believed to have been the original habitat of the Aryan race. The question, of course, was what was the original Aryan—white, brown, or whitey-brown? and he was inclined to think that he was a whitey-brown, and that his primæval seats were in the higher recesses of the Hindu Kush, and that the branch which went into India had become darker by admixture with the aborigines, while those who went into Europe had become fairer or been completely blanched into whiteness by similar admixture with the fair races. As to features, he had come to the conclusion that the high prominent features which we were accustomed to speak of as distinctive of the Semite races were the real original features of the Aryan, and that the Jews had acquired them only by admixture with the Aryan races. The true type of Semite he believed was to be found in the Southern Arab.

On the Influence of the Canadian Climate on Europeans, by Prof. W. H. Hingston, M.D.—After describing the physical, geographical, and climatic characteristics of the country, the author proceeded to say that the heat of the summer in Canada was more easily endured than the moist humid summer weather often experienced in Europe. The skin was called into greater activity, and the heat of the summer weather acted very strongly on the liver, but if European residents adopted the indigenous customs of the country, lived moderately and temperately, and led active lives, their livers would give them no trouble. The cold weather in winter stimulated people to activity. The mortality in early life was large, because in no country in the world were there so many children, but the mortality in adult life was not large. With the exception of Malta, the Canadian stations used to be considered the healthiest posts of the British army, and there were really no diseases peculiar to the country, while many which prevailed in England and in Europe had no existence there.

The Life-History of a Savage, by the Rev. George Brown.—The author gave an account of the life-history of a native of New Britain, an island in the Polynesian group, about forty miles north-east of New Guinea. He commenced with the birth of the example child, and said that when a child was born to the Papuan people who occupied this land, a warm banana-leaf was wrapped round his body, and he was fed with the expressed juice of the cocoa-nut, and left ever afterwards to be "dressed in pure sunshine." He described the children's games of the people, and the initiation of the boy as he grew up into certain secret rites, and the ceremonies at the various feasts, especially on his marriage, and the feast when he was taught to curse his enemies. On the occasion of his marriage there was an interchange of goods and a distinct payment for the wife. Presents were also given by the women to the bride and by the men to the husband, and after a broom had been given to the former, and a spear to the latter, a stick was given to the man. The spear meant that the husband was to protect his wife, and the broom that with it the wife was to do her household work, and the stick was a symbol of his authority, or, in plain English, "Here's the stick with which to whack her if she does not." At the time of death the cries of the friends of the deceased were very piteous and touching. The dead person was cried to to come back, was expostulated with for having left his friends, was entreated to say how his friends had offended him, and so on, the mourners seeming to be speaking in the very presence of the spirit of the dead person. Many of the things which we should call good they also called good, but they had a definite

idea of a future state and also of punishment for one offender, the niggardly man. When an old man came near death he was placed upon a litter, and carried round to see the old scenes amid which he had passed his life—his canoe, the sea, and all the old familiar subjects, and then he was taken back to wait his time. After death he was placed in a sitting posture and taken into the public square, with his weapons by his side, and before him the people placed offerings of their valuable goods and money.

Notes on Photographs of Mummies of Ancient Egyptian Kings recently Unrolled, by Sir William Dawson, F.R.S.—The photographs representing the mummies of Seti I., Rameses II., and Rameses III. were communicated by Dr. Schweinfurth, of Cairo. They are of great interest as enabling us to see the actual features of these ancient Egyptian kings, and to compare them with their representations on the monuments and with modern Egypians. It appears that the features of Seti are scarcely of Egyptian type, as represented either by the monuments of the older dynasties or by the present Egyptians; though, as Dr. Schweinfurth shows in a drawing accompanying the photographs, a similar style of countenance still exists among the Copts. It also appears that the features of Rameses II. strongly resemble those of his father, and are very like those of some of his statues. Both Seti and Rameses have narrow and somewhat retreating foreheads, and strongly developed jaws, indicating men of action rather than of thought; and both were men of great stature and bodily vigour, and seem to have lived to advanced ages.

Prehistoric Man in Manitoba, by Mr. C. N. Bell, F.R.G.S. (Winnipeg, Canada).—The author announced the existence in the Canadian North-West of sepulchral mounds, and pointed out the hitherto unknown fact that there is a continuous line of mounds from the mound-centres of the Mississippi River, down the Red River, to Lake Winnipeg. Human remains, much decayed, were found in the mounds, all buried by being placed on the surface under heaps of earth in which patches of charcoal and ashes frequently occurred, though no remains of funeral feasts, as bones, &c., were met with. Indians, when first met with, buried weapons with their warriors, but none were found in these mounds, though implements and ornaments of shell, bone, and stone were common, as well as pottery, which latter was unknown to the Indians of North-West Canada on the arrival of white emigrants. One mound had a floor of burnt clay and boulders, similar to the sacrificial mounds and altars of Ohio. Ornaments were found made of sea-shells, which must have been carried 1200 miles from their native waters. These mounds, from Lake Winnipeg to the Gulf of Mexico, were of the same character, and very likely were made by one race, though the whites found great diversity of mortuary customs prevailing among the Indian tribes inhabiting that great tract of country.

Notes on a Tau Cross on the Badge of a Medicine-Man of the Queen Charlotte Isles, by R. G. Haliburton.—Mr. Haliburton said this badge was noteworthy, as Queen Charlotte Isles form one of the most isolated groups of the Northern Pacific. They lie off the west coast of British Columbia. This symbol was used by the Indians on large sheets of copper, to which they assigned a high value, and each of which they called a *Tau*. The connection of that name with the symbol is warld-wide. Our T is simply the tau symbol, and is called *tee* or *tau*. The medicine-men represent the tau sometimes on the forehead. The ancients used to mark the captives who were to be saved with a tau or cross; Ezekiel refers to this, and the word he uses for "the sign" to be marked on the foreheads of them that are to be saved really is the "tau" or "cross." No one has divined why the *scarab* was so sacred. He was led to a solution by seeing an exaggerated *tau cross* on the back of a scarab. On looking into the Egyptian name for the scarab he found it to be *tore*, and that the sutures on the beetle form a tau cross. But the same name is applied to the same beetle by our peasantry—*tor-beetle* or *dor-beetle*. Wilkinson represents a god with a scarab for a head, one of the names of which was *Tore*. The use of the prehistoric or pre-Christian cross is world-wide.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—This term begins under a new official *régime*. Prof. Jowett, Master of Balliol, retires from the Vice-Chancellorship, and Dr. Bellamy, the President of St. John's College, succeeds to his place. The Master of Balliol's four years of office have seen several important reforms in which he bore a prominent part. Among them we may mention the alteration in Honour Classical Moderations, the disestablishment of the Examination in the Rudiments of Faith and Religion, the establishment of a University course for medical students, and the abolition of Pass Classical Moderations in favour of Preliminary Examinations for students of law, natural seience, and mathematics. The last reform, indeed, has not yet become law; but the necessary steps to complete the legislation are already taken, and the Statute will doubtless pass Convocation during the present term.

Scholarships in Natural Science are announced this term for competition at Balliol, Trinity, and Christ Church.

The following scheme of lectures in Natural Science is announced for the present term:—

Physics.—Prof. Clifton lectures on General Electricity, and Mr. Selby on Electrostatics treated Mathematically. Practical instruction in Physics is given in the Clarendon Laboratory by Prof. Clifton and Messrs. Walker and Selby.

At Christ Church, Mr. Baynes lectures on Fourier's Theorem. At Balliol, Mr. Dixon lectures on Elementary Light and Heat.

Chemistry.—Prof. Odling lectures at the Museum on the Benzoic Compounds. Mr. Fisher gives a course of Inorganic Chemistry, and Dr. Watts a course of Organic Chemistry. Practical instruction is given by the above, and by Messrs. Baker and Marsh.

At Christ Church, Mr. Vernon Harcourt gives a course of lectures on Inorganic Chemistry for the Preliminary Examination. Practical instruction is also given at the Christ Church and Balliol Laboratories.

Animal Morphology.—Prof. Westwood lectures on the Hexapod Arthropoda. Prof. Moseley lectures on Comparative Anatomy. Mr. Baldwin Spencer gives an elementary course on the same subject. Mr. Hatchett Jackson lectures on Comparative Embryology. Mr. Barclay Thompson lectures on the Osteology, Odontography, and Distribution of Mammals. Practical instruction is given by Prof. Moseley, Mr. Spencer, and Mr. Robertson.

Physiology.—Prof. Burdon-Sanderson lectures on Circulation, Respiration, and Bodily Motion. Mr. Dixey lectures on Histology; and Mr. Hatchett Jackson on Elementary Physiology. Practical classes are conducted by Messrs. Dixey and Gotch.

Human Anatomy.—Mr. A. Thomson lectures on the Central and Peripheral Nervous System, and Digestive System. He also gives demonstrations on Topographical Anatomy, and has a daily class for Dissection.

Medicine.—Dr. Darbishire gives demonstrations at the Radcliffe Infirmary, in Physical Diagnosis and Regional Anatomy, and Mr. Winkfield gives demonstrations in Surgical Diagnosis.

Botany.—Prof. Bayley Balfour lectures at the Botanic Garden on Vegetable Morphology and Physiology.

Mineralogy.—Prof. Story-Maskelyne lectures at the Museum on Minerals occurring in Lodes.

Geology.—Prof. Prestwich lectures at the Museum on the Principles of Geology.

Anthropology.—Dr. Tylor lectures on the Development of Culture, Sign Reading, &c.

Mr. A. L. Selby, B.A., Demonstrator of Physics in the Clarendon Laboratory, has been elected a Fellow of Merton College.

Mr. H. B. Dixon, M.A., of Trinity College, has been elected a Fellow of Balliol College.

SCIENTIFIC SERIALS

IN the *Journal of Botany* for August Mr. J. G. Baker concludes his notes on British Rubi, and Messrs. Roy and Bisset contribute the second and concluding part of their notes on Japanese Desmids (illustrated).—The number for September commences with an interesting and important paper by Mr. G. Massee, on the structure and functions of the subterranean parts of *Lathræa squamaria*, L. (also illustrated). He regards the plant as of saprophytic rather than parasitic habit, the disks or haustoria on which its parasitism depends being frequently entirely absent from old plants. In some instances, but not all, the roots are covered with the mycelium of a fungus similar to that described by Kamienski in the case of

Monotropa.—The instalment of Mr. J. G. Baker's synopsis of the Rhizocarpeæ is occupied by a monograph of the forty species of *Marsilea.*—The remaining articles in these and those in the October number are of less general interest, or are reprints or reports.

Rivista Scientifico-Industriale, September 15.—Experiments on the electric conductivity of vapours and gases, by Prof. Giovanni Luvini. The important experiments here described have been carried out for the purpose of exposing the commonly accepted fallacy that moist air and gases in general are good conductors. Having already argued against this view in his recent memoir on the origin of atmospheric electricity, the author now clearly shows by a series of carefully conducted experiments that such bodies as moist air, aqueous vapour, and other gases are under ordinary pressure absolute non-conductors. Under pressures varying from 16° to 100° C. none of the vapours tested by him betrayed the least conductivity, all acting as excellent insulators. He promises to resume the subject in his work on the Polar auroras, to which the present essay and the memoir on the origin of atmospheric electricity serve as introduction. The conclusions so far arrived at, combined with Faraday's memorable experiments on the causes of the electricity in Armstrong's hydro-electric machine, tend to show that gases and vapours are not even electrified by friction with themselves or with solid or fluid bodies. Henceforth physicists must reject, as erroneous, all such theories respecting the electricity of machines, of the air, or the clouds, as rest on the assumed conductivity of moist air or on the property of gases to be electrified by friction. It is pointed out that, were the saturated atmosphere and clouds really good conductors, such a phenomenon as lightning would be simply impossible, or at all events extremely rare.—Separation of nickel from cobalt, by Pietro Gucci. For the new method here proposed and described it is claimed that it is both easier and much more expeditious than that of Fischer and Stromeyer, also that it determines the presence of the smallest particle of nickel in any quantity of cobalt.—New hygrometric formula and tables, by Prof. Paolo Cantoni.

SOCIETIES AND ACADEMIES

LONDON

Entomological Society, October 6.—Robert McLachlan, F.R.S., President, in the chair.—Mr. W. Bartlett Calvert, of Santiago, Chili, was elected a Fellow.—Mr. McLachlan exhibited a number of seeds of a Mexican species of *Euphorbiaceæ*, popularly known as "jumping seeds," recently received by him from the Royal Horticultural Society. He stated that these seeds were known to be infested with the larvæ of a species of *Tortricidæ*, allied to the apple *Tortrix*. They were first noticed by Prof. Westwood at a meeting of the Society held on June 7, 1858, and the moths bred therefrom were described by him as *Carpocapsa saltitans*. These seeds have since, from time to time, been referred to both in the United Kingdom and America.—Mr. Roland Trimen exhibited and read notes on some singular seed-like objects found in the nests of *Termites*, and also in those of true ants, in South Africa. They were apparently of the same species as those from the West Indies, described in 1833 by the Rev. L. Guilding as *Margarodes formicarius*, which was usually referred to the *Coccidæ*. They were of various shades from yellowish pearly to golden and copper colour, and were strung together by the natives like beads, and used by them as necklaces.—Mr. W. F. Kirby exhibited, on behalf of Mr. John Thorpe, of Middleton, a long series of buff and melanic varieties of *Amphidasis betularia*, and read notes on them communicated by Mr. Thorpe.—Mr. Kirby also exhibited, on behalf of Mr. Nunney, a dark variety of *Argynnis aglaia* from Caithness, and a tawny-coloured variety of *Vanessa urticæ* from Bournemouth.—M. Alfred Wailly exhibited a fine series of Saturnias and other Bombyces, mostly bred by him, from South Africa; also specimens of *Dirphia tarquinia*, *Attacus orizaba*, *Platysamia cecropia*, *P. ceanothi*, *Callosamia angulifera*, and *C. promethæa*, from Central America. M. Wailly stated that several of the large South African *Saturnidæ* formed no cocoons, the larvæ entering the earth to undergo the change to the pupal state. Mr. Trimen said he was able to confirm this statement.—The Rev. W. W. Fowler exhibited a number of minute *Acari* which had been doing injury to fruit trees near Lincoln.—Mr. Poulton gave an account of the experiments recently made by him with the larvæ of several species of the genus *Vanessa*, for the purpose of ascertaining the relations of pupal colour to that of the surface on which the larval skin was thrown off, which had formed the subject of a paper lately read by him before the British Association. He also exhibited the frame constructed by him for the purpose of these experiments.—Mr. Slater exhibited a specimen of *Prionus coriarius* found in Devonshire on fennel, and a specimen of *Calandra palmarum* from Pembroke Dock.—Mr. Enock exhibited *Mymar pulchellus*, and a specimen of *Atypus piceus* recently taken on Hampstead Heath.—Mr. Elisha exhibited a series of *Gelechia hippophaella*, bred from larvæ collected at Deal on *Hippophaë rhamnoides*.—Mr. Billups exhibited *Echthrus lancifer*, a species of *Ichneumonidæ* new to Britain, taken at Walmer on August 15 last. He remarked that Brischke had bred members of this genus from *Sesia sphegiformis*, *S. formicæformis*, and *Leucania obsoleta*; but that in this country the genus was little known, only one species being mentioned in Marshall's list of *Ichneumonidæ*.—Mr. E. A. Butler exhibited living specimens of *Chilacis typhæ*, received from the Rev. E. N. Bloomfield, of Guestling, Hastings; and a pair of *Harpalus discoideus*, obtained in August last, near Chilworth, Surrey.—Mr. A. J. Rose exhibited specimens of a mountain form of *Lycæna virgaureæ*, recently collected by him in Norway.—Mr. Champion exhibited *Teratocoris antennatus* and *Drymus pilicornis*, taken near Sheerness.—Mr. W. White exhibited a specimen of *Chelonia caja* with abnormal antennæ, and read notes on the subject.—Mr. Elisha read a paper on the life-history of *Geometra smaragdaria*.—Mr. C. O. Waterhouse communicated a paper on the tea-bugs of India and Java.

SYDNEY

Linnean Society of New South Wales, Aug. 25.—Prof. W. J. Stephens, M.A., F.G.S., President, in the chair.—The following papers were read:—Note on *Eucalyptus leucoxylon* (F. v. M.), by W. Woolls, Ph.D., F.L.S. In the "Flora Australiensis," vol. iii., two Eucalypts previously regarded as distinct species (*E. leucoxylon*, F. v. M., and *E. sideroxylon*, A. Cunn.) were united under the former name. Dr. Woolls has long thought that this step was a mistake, and in his paper he gives reasons based upon the examination of specimens of both forms, in favour of their specific distinctness, and of the restoration of Cunningham's name to the red-flowering iron-bark of New South Wales, the other name being restricted to the white gum of Victoria and South Australia.—Contributions towards a knowledge of the Coleoptera of Australia, No. III., by A. Sidney Olliff, F.E.S. This paper contains notices of several new species of *Nascio*—a genus of Buprestidæ—of which two are named *L. munda* and *N. multesima*. Additional localities for some previously known species are also given, *N. carissima* being recorded from Sydney.—List of the Orchideæ of the Mudgee District, by Alex. G. Hamilton. In this paper, which is a contribution towards a knowledge of the geographical distribution of plants in New South Wales, fifty-seven species of orchids are enumerated as occurring in the Mudgee District; and particulars are given concerning their habitats and the months during which they flower. In addition a comparison of the orchids of this district with those of the county of Cumberland and of the other Australian colonies is also given.—On an undescribed species of *Chilodactylus* from Port Jackson, by E. P. Ramsay, LL.D., F.R.S.E., and J. Douglas Ogilby. Under the name of *Chilodactylus polyacanthus*, a new species of Morwong is described, and its affinity to *C. carponemus*, Cuv. and Val., is discussed.—Dr. Ramsay exhibited a number of very rare birds from Derby, North-West Australia, recently collected in that district by Mr. Cairns. He particularly drew attention to the following:—*Poephila acuticauda*, *Poephila mirabilis*, *Donacicola pectoralis*, *Emblema picta*, *Estrelda annulosa*, *Estrelda ruficauda*, *Pœcilodryas cerviniventris*, *Smicrornis flavescens*, *Pardalotus rubricatus*, *Pardalotus uropygialis*, *Malurus coronatus*, *Malurus cruentatus*, *Cacatua gymnopis*, *Climacteris melanura*, *Geophaps albiventris*, *Astur cruentus*, *Trichoglossus rubritorquatus*.—Mr. Macleay exhibited the following new or rare reptiles and fishes collected by Mr. W. W. Froggatt in the vicinity of Cairns, Queensland:—Snakes: *Tropidonotus picturatus*, Schlegel, *Dipsas boydii*, Macleay, *Hoplocephalus assimilis*, Macleay, *Hoplocephalus nigrostriatus*, Krefft, *Nardoa crassa*, Macleay, and *Dendrophis bilorealis*, Macleay. Lizards: *Varanus ocellatus*, Gray, *Varanus*, sp. ?, *Hinulia*, n. sp., four species of Geckotidæ unknown, one with tail of remarkable width, and several other unknown lizards. Fishes: *Dules Haswellii*, Macleay, *Aristeus rufescens*, Macleay,

Serranus lanceolatus, Bleek, a species new to Australia, and a species of *Eleotris*, probably undescribed, remarkable for its minute scales. Collected from the same district were a number of frogs, among which Mr. Fletcher pointed out examples of *Hyla dolichopsis*, *H. cærulea*, *H. lesueurii*, *H. peronii*, *H. nasuta*, *H. gracilenta*, *Limnodynastes ornatus*, and two other species not determined.

PARIS

Academy of Sciences, October 11.—M. Jurien de la Gravière, President, in the chair.—On a principle in rational mechanics, and on a demonstration used by Daniel Bernoulli in 1757, by M. de Jonquières. The reference is to the author's recently-explained theory of the hydro-extractor, the fundamental principle of which he now finds was already known to Bernoulli, at least so far as concerns the action of the pendulum. His demonstration, analogous to that of M. de Jonquières, is contained in his memoir entitled, "Principes hydrostatiques et méchaniques, &c.," which obtained the prize of the Royal Academy of Sciences.—On the persistence of the instinctive functions and voluntary movements in bony fishes after extraction of the cerebral lobes, by M. Vulpian. In supplement to his previous paper on this subject, the author mentions the case of a carp operated upon on March 18, 1886, and which survived till September 29. During this period it acted in almost every respect like any ordinary fish, noticing and avoiding obstacles, seizing and swallowing its food, rejecting non-alimentary substances, and so on. With the exception of smell, it evidently retained all its senses and instinctive and intellectual faculties. This experiment fully confirms the results already determined by the researches of M. Is. Steiner, and shows that in fishes instinct and will survive the extraction of the cerebral lobes, which in reptiles, birds, and mammals are the seat of those faculties.—Experimental researches on the nature of *rigor mortis*, by M. Brown-Séquard. The object of these studies is to show that the rigidity ensuing after death is due neither altogether nor even to any great extent to the coagulation of the albuminous substances of the muscles, as still maintained by most physiologists on the authority of Brücke, Kuhne, and Wundt.—On the temperature of the bed of oceanic basins compared with that of the continents at the same depth, by M. Faye. In connection with the reference made to this subject in the opening address of the President of the British Association at Birmingham, the author takes the opportunity of generalising the law already established by him respecting the more rapid and deeper cooling of the earth's crust under the seas than under the continents. Not only is this law applicable to the Polar seas, whose lowest depths have a temperature very near zero, but also to those which do not freely communicate with the Poles. In these waters also the temperature decreases with the depth, the difference between them and the continents at the same depths being, within about 15°, as great as for the oceans.—Purification of yttria, by M. Lecoq de Boisbaudran. In the process of purification here described the earth A, differing little from that of M. Clève, yielded a beautiful phosphorescence of a pink auroral tint, due not to the yttria itself, as supposed by Mr. Crookes, but to the presence of a minute trace of bismuth derived either from the primary substance or from the reagents.—Fluorescence of the compounds of bismuth subjected to electric effluvium *in vacuo*, by M. Lecoq de Boisbaudran. In this paper the author sums up his observations on the pink fluorescence referred to in his previous communication. He remarks incidentally that during these studies he detected traces of bismuth in numerous chemical products, several of which were supposed to be quite pure.—Summary of the meteorological observations made during the year 1885 at four stations in the Upper Rhine and Vosges districts, by M. Hirn. The observations here tabulated give the highest and lowest temperatures from month to month at Colmar, Thann, Schlucht, and Munster, the actinometric readings taken at the Colmar Observatory, the atmospheric pressure, rainfall, and other meteorological data at these stations.—On the transformation of surfaces, and on a class of differential equations, by M. E. Picard.—The reciprocal relations of the great forces of Nature, by M. Emile Schwœrer. The author's remarks are in reference to his French translation of M. A. Klein's remarkable analysis of MM. Hirn and Clausius's recent memoirs contributed to *Gaea*.—Saturation of normal arsenic acid with lime-water and with the water of strontian, by M. Ch. Blarez.—Contribution to the study of the alkaloids, by M. Oechsner de Coninck. Two very sensitive reagents are described, which are easily produced, and which are likely to prove very serviceable in the diagnosis of the various alkaloids and of the different bases treated by the author.—On the genus *Entione*, Kossmann, by MM. A. Giard and J. Bonnier. In the *Porcellana longicornis* of Concarneau the authors have discovered an *Entoniscus* closely allied to those met by Fritz Müller in the Porcellanæ of the Brazilian seaboard. The study of this species, here named *Entoniscus mülleri*, justifies the division of the genus proposed by Kossmann. The term *Entoniscus* being reserved for the two species of parasites of the Porcellanæ, the *Entoniscus* of the crabs would then constitute the genus *Entione*.—Diseased grapes in the vineyards of La Vendée, by M. Prillieux. The vineyards of this district have this year been attacked by a species of mildew here fully described.—On some garnet-bearing rocks of Puy-de-Dôme, by M. Ferdinand Gonnard. It is shown that, contrary to the received opinion, the important group of garnets, whether as a mineralogical accident, or as an essential constituent element of the different grenatites, is largely represented in the primitive or plutonic formations of Puy-de-Dôme.—On the phosphated deposits of Beauval (Somme), by M. Stanislas Meunier. From a careful study of the phosphate of lime recently discovered at Beauval, the author infers that the phosphated chalk of Picardy belongs to an older geological epoch than that of Belgium.

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THURSDAY, OCTOBER 28, 1886

HISTORY OF ETHICS

Outlines of the History of Ethics for English Readers. By Henry Sidgwick, Knightbridge Professor of Moral Philosophy. (London: Macmillan and Co., 1886.)

FROM the earliest times of Greek thought the foremost philosophic minds of Europe have restlessly sought for sanctions of the moral code; and the last quarter of the nineteenth century sees but little unanimity of opinion on the subject. The purely intuitional moralist dreads, in the determinist ethics of evolution and of utilitarianism, the deathblow of virtue properly so called. The followers of Bentham and of Spencer foresee the downthrow of morality unless some sanction more solid than the intuitionalist can supply be found for the ethical creed, unless "morality be established on a scientific basis." Neither can bring himself to understand how the creed of the other can really influence conduct; and both believe in their inmost hearts that the conduct of the other is really determined by something that goes deeper than the outward profession of faith.

Amidst the divergencies of ethical opinion, however, practical morality has undergone but little modification. Notwithstanding the momentous change wrought by the introduction of Christianity into Europe the ideal at which the good man aims to-day differs but little from that towards which the ancient Greek directed his endeavours. It is true that the Christian virtue of *humility* takes the place of the Greek *highmindedness;* but it is questionable whether the standard of excellence practically set before himself by the bishop who preaches the one differs materially from that of the philosopher who inculcated the other. This uniformity of practice amid diversities of faith, notwithstanding that the practice is in a high degree the outcome of the faith, receives perhaps sufficient explanation when it is remembered that the conduct of the individual is determined by a triple service—the service of Self, the service of Man, and the service of God. But the service of God—mediæval monkism notwithstanding—takes practical expression in every-day life in the service of Man, while the pure service of Self is rendered impossible by the exigencies of the social life. In this way conduct is practically reduced to a subtle compound of Egoism and Altruism. Whether the welfare of others is sought from motives of self-interest, or the improvement of self is ennobled by the thought that in this way the level of humanity is being raised, matters not practically. The material morality is the same, however wide may be the essential and formal difference.

But man is an inquiring animal, especially in the scientific and philosophical varieties of the genus. He cannot rest content with the mere possession of moral intuitions, he must also endeavour to ascertain their cause and mode of origin. He is also in his higher developments essentially a rational animal. He is not satisfied with the promptings of ethical desires, he must also justify the resulting conduct on rational grounds. Hence the science of ethics, which deals with the questions "What is right?" and "Why?"

In Prof. Sidgwick's "Outlines of the History of Ethics" we have a remarkably clear and succinct account of the answers that have been given to these questions. In dealing with the subject as a separate province of thought there is this peculiar difficulty, that ethical theory is in a very high degree determined by philosophic creed. The ethical theory of the Platonist for whom the Universe has thought itself out from the abstract to the concrete, inevitably differs from that of the modern evolutionist who believes that material and mental groupings have gradually advanced from the simple to the complex until the extraordinary complexity of the human brain and human thought-processes has been reached. Prof. Sidgwick has met this difficulty as fully as the space at his disposal rendered possible, and by not unduly narrowing the limits of his province has presented a tolerably complete bird's-eye view of the history of ethical thought.

His work begins with an *Introduction* intended to assist the reader in grasping and arranging the somewhat compressed historical matter presented to him in the body of the book. The student who comes fresh to the subject will probably find the study of this introduction more valuable at the end of his first perusal of the work than at the outset of his labours. Then follows in Chapter I. a general account of ethics, in which the subject is defined, and its relations to theology, politics, and psychology are clearly indicated. It may be useful to give here the "summary view of ethics" with which this chapter closes:—

"The subject of Ethics, most comprehensively understood, includes (1) an investigation of the constituents and conditions of the Good or Well-being of men considered individually, which chiefly takes the form of an examination into the general nature and particular species of (*a*) Virtue or (*b*) Pleasure, and the chief means of realising these ends; (2) an investigation of the principles and most important details of Duty or the Moral Law (so far as this is distinguished from Virtue); (3) some inquiry into the nature and origin of the Faculty by which duty is recognised, and, more generally, into the part taken by Intellect in human action, and its relation to various kinds of Desire and Aversion; (4) some examination of the question of human Free Will. It is connected with Theology, in so far as a Universal Good is recognised, inclusive of Human Good, or analogous to it; and again, so far as morality is regarded as a Code of Divine appointment. It is connected with Politics, so far as the well-being of any individual man is bound up with the well-being of his society; and again with Jurisprudence (or Politics), so far as morality is identified with Natural Law. Finally almost every branch of ethical discussion belongs at least in part to Psychology; and the inquiries into the origin of any moral faculty and the freedom of the Will are purely psychological."

The three following chapters form the main body of the work. The first deals with Greek and Græco-Roman ethics. In this chapter the author brings out clearly the Socratic paradox, that men's ignorance of justice is the sole cause of their unjust acts, that, in a word, no one is voluntarily bad; and its justification as the outcome of a pair of apparent truisms, (1) that every one wishes for his own good, and would get it if he could, and (2) that those who knew how to do just and righteous acts would prefer nothing else, while those who did not know could not do them if they would. Unfortunately, as a practical fact, men too often desire in their moments of passion what in

their hours of reflection they know to be wrong. And since action takes the line of least resistance at the moment of temptation, there follows remorse in the hour of bitter remembrance. In the moment of trial, knowledge of right is not absent but submerged. The development of ethical doctrine as it passed through the hands of Plato and Aristotle, of Zeno and Epicurus, is treated with marked clearness and ability.

In the chapter on Christianity and mediæval ethics the main characteristics of Christian morality occupy a considerable space, the only writer whose doctrines are expounded at any considerable length being Thomas Aquinas.

The last chapter deals with modern ethics, chiefly English. The author, however, tells us, in his preface, that he has not attempted to deal with contemporary modes of ethical thought—with which he has been engaged controversially—except in a very brief and summary way. The motive is admirable; but the fact is to be regretted. As a "manual for students" the book would have been more complete had contemporary ethics formed the subject of a concluding chapter. There are many who will take up this book as a summary of the subject as a whole from the hands of one of its masters, and who will be disappointed to find so meagre an account of modern transcendental ethics and of the moral theory as "sanctioned" by evolution. The writer who has treated the ethics of twenty centuries with such marked impartiality could safely have been trusted to preserve a due "objectivity" of treatment in dealing with the modes of ethical thought in his own time.

All genuine students of human thought and endeavour will thank Prof. Sidgwick for having presented them with this altered and enlarged edition, in a handy form, of his article in the "Encyclopædia Britannica."

C. Ll. M.

PROFESSOR CHRYSTAL'S "ALGEBRA"

Algebra: an Elementary Text-Book for the Higher Classes of Secondary Schools and for Colleges. By G. Chrystal, M.A. Part I. (Edinburgh: Adam and Charles Black, 1886.)

"THERE are few things where the want of an enlightened scientific public strikes an expert more than the matter of scientific text-books." "For our teaching of algebra, I am afraid, we can claim neither the sanction of antiquity nor the light of modern times. Whether we look at the elementary, or at what is called the higher teaching of this subject, the result is unsatisfactory. . . . In the higher teaching, which interests me most, I have to complain of the utter neglect of the all-important notion of algebraic form." "The logic of the subject, which, both educationally and scientifically speaking, is the most important part of it, is wholly neglected. The whole training consists in example-grinding. What should have been merely the help to attain the end has become the end itself. The result is that algebra, as we teach it, is neither an art nor a science, but an ill-digested farrago of rules, whose object is the solution of examination problems." "The problem for the writer of a text-book has come now, in fact, to be this—to write a book so neatly trimmed and compacted that no coach, on looking through it, can mark a single passage which the candidate for a minimum pass can safely omit. . . . When our system sets such mean ends before the teacher, and encourages such unworthy conceptions of education, is it to be wondered at that the cry arises that pupils degenerate beneath even the contemptible standards of our examinations. . . . The cure for all this evil is simply to give effect to a higher ideal of education in general, and of scientific education in particular. Science cannot live among the people, and scientific education cannot be more than a wordy rehearsal of dead text-books, unless we have living contact with the working minds of living men."

Such being some of the author's weighty utterances in his famous British Association address to Section A (NATURE, vol. xxxii. pp. 446-449), it was with much interest we read the announcement that he was writing a treatise on algebra, and it is with much pleasure we have perused this first instalment of 542 pages. This is no ordinary treatise: school text-books abound, and more are on the way. This bears traces everywhere of a master's genius; those are but clever arrangements of well-known materials.

This is an elementary volume because "it begins at the beginning of the subject"; it is not written, however, for babes. It will have been noticed how the address quoted above insisted upon the "all-important notion of algebraic form": at the commencement Prof. Chrystal lays down generally the three fundamental laws, and thence proceeds deductively. This he does because this idea of algebraic form is "the foundation of all the modern developments of algebra, and the secret of analytical geometry, the most beautiful of all its applications." The following abstract of the interesting preface will best indicate the writer's aim. Outside algebra proper the reader is expected to be familiar with the definition of the trigonometrical functions, and to have a knowledge of their fundamental addition-theorem. The first object is to "develop algebra as a science, and thereby to increase its usefulness as an educational discipline." Sources of information are indicated, and a most admirable feature is the introduction of numerous historical notes. With regard to some of the early chapters, which are specially hard reading for junior students, Prof. Chrystal writes that they were "written as a suggestion to the teacher how to connect the general laws of algebra with the former experience of the pupil. In writing this chapter I had to remember that I was engaged in writing, not a book on the philosophical nature of the first principles of algebra, but the first chapter of a book on their consequences."

The subject is broken up into twenty-two chapters, and, as the arrangement—"the result of some ten years' experience as a University teacher"—deviates somewhat from ordinary usage, we give the headings:—(1) Fundamental laws and processes (association, commutation, and distribution, with historical note); (2) laws of indices, theory of degree; (3) theory of quotients, first principles of theory of numbers; (4) distribution of products (Σ and Π,) principle of substitution, homogeneity, symmetry, principle of indeterminate coefficients; (5) transformation of the quotient of two integral functions; (6) G.C.M. and L.C.M.; (7) factorisation of integral functions;

(8) rational fractions; (9) continuation of theory of numbers (Lambert's theorem); (10) irrational functions; (11) arithmetical theory of surds; (12) complex numbers; (13) ratio, proportion, variation; (14) on conditional equations in general (elimination, transformation); (15) variation of a function; (16) equations and functions of first degree (determinants, contour lines); (17) equations of the second degree; (18) general theory of integral functions (Newton's theorem, Lagrange's interpolation formula, maxima and minima); (19) solution of problems; (20) arithmetic, geometric, and allied series; (21) logarithms; (22) theory of interest and annuities. There is a large collection of exercises: with regard to these, after reading the address, we are prepared to find that the author deprecates the idea of a reader's working through all these at the first reading: they are given for the sake of variety, and to be worked at different times of reading. Answers are given at the end. We have put the writer's own words in the forefront, that our readers may be reminded of what he has said in the past and informed of what he has now attempted to do. The result is a work of singular ability and freshness of treatment. It follows no previous leader, it will give rise to shoals, possibly, of imitators, but it will bear boiling down by the "fifth-rate workmen" whom the Professor lashes. It is not a book for our elementary classes, but it will be an excellent work to put into the hands of some of our sixth-form pupils. It is admirably adapted for thoughtful students at our Universities who have not the dread of examinations before their eyes, but can afford to go deeper into the subject than the ordinary run of our students do. The book is excellently printed and is of a handy size. We hope the second part is well advanced.

THE MAMMALS OF CENTRAL AMERICA

Biologia Centrali-Americana. Mammalia. By Edward R. Alston. With an Introduction by P. L. Sclater, M.A., F.R.S. 4to. (London: R. H. Porter, 1879–82.)

THE progress of various portions of the great work upon the fauna and flora of Central America undertaken by Messrs. Salvin and Godman has been from time to time noted in our columns. Each section of the extensive and almost exhaustive mass of material which the industry and liberality of the projectors and editors of the work have accumulated, has been placed in the hands of some one specially qualified to render them available for the purposes of science. No one could have been found more fitting to undertake the description of the mammals than the late Edward R. Alston, whose lamented and untimely death deprived zoology of one whose careful and conscientious method of work gave promise of a career of great benefit to the progress of the special branch to which he had devoted himself. He was unfortunately unable even to complete the work under review, which owes its finishing touches to the pen of Mr. Sclater.

Compared with the general mammalian fauna of the world, that of the region treated of by Mr. Alston is rather limited. 181 species are enumerated, of which 52 are Bats, and 60 Rodents. Of the *Primates*, 10 species of *Cebidæ* and 1 of *Hapalidæ* are described, all forms proper to the Neotropical region. Their extension into Central America is a subject of much interest which has been particularly investigated by Mr Sclater, whose observations are extended or confirmed by Mr. Alston. One species only (*Ateles vellerosus*) is known to inhabit Mexico, reaching as far north as the 23rd parallel. The *Insectivora* are represented only by 5 small species of *Soricidæ*. The *Carnivora* are more numerous. The *Felidæ* comprise the southern Jaguar, Ocelot, Margay, Eyra, and Jaguarondi, the widely distributed Puma, and the northern Bay Lynx. The dogs are all northern forms, viz., *Canis lupus*, *C. latrans*, and *Vulpes virginianus*. The *Mustelidæ* are well represented by both Neotropical and Nearctic forms. The two North American Bears, *Ursus horribilis* and *U. americanus*, both extend as far as Northern Mexico, and are therefore included within the scope of the work. But the most interesting of the *Carnivora* are the curiously generalised group of *Procyonidæ*. Of 8 recognised species of this family, 7 are included in the limits of Central America, the Brazilian *Nasua rufa* being the one exception. Of special interest are the rare and little known *Bassaris astuta* and *B. sumachristi* (of which a new figure is given), and *Bassaricyon gabbi*. The *Ungulata* are, as is well known, very poorly represented in the actual fauna of the American continent, though so abundant and varied in former ages. Four deer of the genus *Cariacus*, the northern Big-Horn and Prong-buck, two Peccaries and two Tapirs are all that can be mustered as denizens of the Central American region. It should be mentioned that the distinguishing cranial characters of Dow's Tapir are carefully worked out and figured. A fair proportion of the essentially Neotropical Edentates and Marsupials extend beyond the Isthmus of Panama, including the three modifications of the Anteater type, an Armadillo, three Sloths, and seven Opossums.

The Cetacea of the coast are not included in the work, but there is a full notice of the Manatee, containing copious extracts from Dampier's quaint but graphic description of the habits and distribution of the animal in his time. As in so many other cases, the correct scientific designation of this creature is a matter of considerable perplexity. We quite agree with Mr. Alston in keeping *Manatus* for the generic name, but *australis* can hardly be accepted for any of the species at present discriminated. It was originally applied to a combination of the African and American forms, as opposed to *borealis*, the northern Manatee or Rhytina, and if retained should belong to the former, as the African habitat is mentioned first by Gmelin (1788) and Tilesius (1812), and is the only one given by Shaw (1800). Cuvier (1809) first distinguished the African from the American species by their osteological characters, calling them respectively "Lamantin du Senegal" and "Lamantin d'Amérique," which names were subsequently Latinised by Desmarest (1817) into *M. senegalensis* and *M. americanus*. This last name is therefore certainly preferable to *M. australis* for the West Indian animal. In a recent monograph of the genus, Dr. C. Hartlaub (*Zoologische Jahrbuch*, Bd. I.) has carefully investigated the synonymy, and admits two species as inhabitants of the New World, *M. latirostris* (Harlan) and *M. inunguis* (Natterer), *M. americanus* being suppressed as a compound of the two. The Central American form is referred by Hartlaub to *M. latirostris*,

M. inunguis being apparently confined to the upper waters of the Amazon and Orinoco; but we cannot say that we are quite satisfied with the supercession of Cuvier's name for that of Harlan.

The work is illustrated by twenty excellent coloured plates by Wolf, Keulemans, and Smit, representing new or little-known species. We cannot conclude our notice without again expressing our admiration for the scientific enthusiasm and public spirit shown by Messrs. Godman and Salvin in the manner in which they are carrying out their great undertaking. W. H. F.

PACKARD'S "FIRST LESSONS IN ZOOLOGY"

First Lessons in Zoology, adapted for Use in Schools. By A. S. Packard, M.D., Ph.D., Professor of Zoology and Geology in Brown University. American Science Series, Elementary Course. (New York: Henry Holt and Company, 1886.)

ONE of the principal objects of the American Science Series, we are told, is to supply "authoritative books the principles of which are, so far as is practical, illustrated by familiar American facts." Another "lack" intended to be supplied by the series is that of text-books which "do not *at least contradict* the latest generalisations." Whatever success Dr. Packard may have attained in the first of these objects, we fear he has not always kept clear of the many pitfalls into which writers of compilations in any branch of science are in these days nearly sure to stumble. Some of his statements are certainly in contradiction to the latest generalisations of zoological science.

On p. 28 we find, in the account of *Millepora*, two forms of zooids, distinguished as "nutritive" and "reproductive." The so-called "reproductive" zooid is nothing of the kind, but simply a tentacle-bearing zooid unprovided with mouth and stomach. Its function is to assist the nutritive or gastro-zooids in obtaining nutrition, by directing small particles of food towards the latter Of the reproduction of *Millepora* nothing is yet certainly known, but Prof. Moseley has suggested that it may probably give off a free-swimming *Medusa*.

Again our author, in enunciating the differences of animals and plants, states (p. 6) that plants "inhale carbonic acid gas, and exhale oxygen," and that animals do just the reverse. This seductive and oft-repeated antithesis is unfortunately not strictly accurate. Both plants and animals inhale oxygen and exhale carbonic acid gas. But in the case of the chlorophyll-bearing plants this process is obscured by an opposite process, by means of which the carbonic acid gas (CO_2) is broken up into its constituent elements, the carbon (C) is absorbed into the plant, and the oxygen (O_2) is set free. This process is, however, rather a nutritive than a respiratory process.

Speaking of *Amphioxus* (p. 139) Dr. Packard states that the water after passing through the gill-slits "enters the general body-cavity." This is an error: the water enters the *peribranchial* cavity—a perfectly distinct structure of quite different origin. Nor has *Amphioxus* "two eye-spots," but only one.

In the chapter (XXIII.) upon the "Lung-fish" (*scr.* Lung-*fishes*), the African *Protopterus annectens* seems to have been mixed up with *Polypterus bichir*, which does not belong to this order at all. The former fish is correctly figured (p. 168), but is named *Polypterus* just above, and is stated to be found in the "Nile," which is the case with *Polypterus*, but not with *Protopterus*.

These inaccuracies occur to us as we turn over the leaves of the "First Lessons in Zoology": we fear it would not be difficult to find others. We must also say that the woodcuts are mostly of coarse execution, and not always well drawn. On the other hand, it may be allowed that as great, or greater, faults might be found with every other attempt that has yet been made to supply a school-book of zoology. We are not acquainted with a really satisfactory work of this kind. A good text-book of zoology for beginners has still to be written. In the meanwhile, Dr. Packard's "first lessons," although going rather too deeply into certain portions of the subject, may be use fully employed for this purpose, without fear of teaching much that will have to be unlearnt.

OUR BOOK SHELF

Russland: Einrichtungen, Sitten, und Gebräuche. Geschildert von Friedrich Meyer von Waldeck.
Die Schweiz. Von Prof. Dr. J. J. Egli. (Leipzig: G. Freytag, 1886.)

THESE volumes are amongst the latest of that encyclopædic work, "Das Wissen der Gegenwart," which has now passed its fiftieth volume. Although, so far as the publication has at present gone, there are more volumes devoted to popular descriptions of countries than to any other, yet general scientific subjects are by no means unrepresented. Thus, volumes have appeared on meteorology; insects, useful and injurious; the sun and planets; light and heat; the fixed stars; the earth and the moon; comets and meteors; electricity and its applications; the nourishment of plants; sound; the ocean, &c., &c. The series is progressing rapidly, we are glad to see, with very short intervals between the successive volumes, from which it is to be presumed that the undertaking is meeting with the success which it deserves amongst the German people, although, we regret to believe, it would ruin any publisher who projected and attempted to carry out a series of this scope and magnitude in this country. In both of the volumes before us the work appears to be done as thoroughly as the space admits Herr Meyer von Waldeck's book is the second part of a work on Russian laws, customs, and manners, and specially deals with the system of administration, and national defence, the church and clergy, and the grades of society. Prof. Egli's account of Switzerland contains a large amount of information compressed with much skill into a very small space. It is not merely a tourist's book, although the tourist who would not take a more intelligent interest in Switzerland after having read it must know a good deal about the country; it is an excellent account of Switzerland which might be read with instruction even by persons who never look forward to seeing that country. The first paragraph of the first chapter deals with the *Urzeit*, or prehistoric period; the last chapter in the book sketches the history of the St. Gothard railway. The numerous excellent illustrations must add largely to the attractiveness and popularity of the series, which, however, the books well deserve on more substantial grounds.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

On the Connection between Chemical Constitution and Physiological Action

IN his letter to NATURE last week (p. 594), Dr. Blake considers that I have not only misunderstood the scope of his experiments, but have been led into error on account of my having no definite idea of the meaning of the term chemical constitution, which he thinks I have evidently confounded with that of chemical composition.

In regard to the first of these points, I shall be very sorry if, by mishap, I have not rightly understood, or have failed to appreciate at their true value, Dr. Blake's experiments (most of which were published before I was born), for I regard him as a true pioneer in the field of pharmacology.

The scope of Dr. Blake's researches, as defined by himself in the Report of the British Association for the Advancement of Science for 1846, was "fully to establish the law of the analogous action of isomorphous substances."

I should no doubt have described Dr. Blake's researches more correctly had I used the word "isomorphous" instead of translating it into popular language, for my translation undoubtedly does not give the full meaning of the word; but my whole address was an attempt to make a difficult subject as popular as I could, and I thought that I had sufficiently acknowledged Dr. Blake's priority by observing that the present epoch of pharmacology might be dated from his researches, although it was those of Crum Brown and Fraser which fairly started pharmacological investigation in a new direction. Perhaps Dr. Blake will be inclined to regard my shortcomings in regard to him more leniently if he will read over my address again, for, if he does so, I think he will see that if on my part I have failed to give him due credit, he on his part has completely misunderstood the whole drift of my address, which was to show the importance of chemical constitution as distinguished from chemical composition. T. LAUDER BRUNTON

The Origin of Species

IT has already been pointed out by Mr. Evershed that the "physiological selection" of Dr. Romanes is identical with the theory outlined by me nearly two years ago in these pages (vol. xxxi. p. 4). As all the objections which have been raised apply equally to my theory, I may perhaps be allowed to give my answer to some of them; it will probably differ in some points from that promised by Dr. Romanes in the *Fortnightly*.

I quite agree with Mr. Wallace (in the *Fortnightly*) that it is only among the group of animals which have at least one common parent that the corresponding variations of the sexual organs which are required for physiological insularity would be likely to occur. But when he maintains that not more than two or three of such a group would reach maturity, and that therefore they would soon die out, he seems to me to forget that it is only on the average that the number would be so small. Many groups would be small, and would die out; exceptional families would be more numerous and more lucky; just as we can all point to human families where twelve or more children have reached maturity, though the average number of those who do so is under three in a family.

The survivors, more or less numerous, would generally not be scattered far from their common birthplace, so that their chance of finding one another would not be very small, especially if the sexual instinct was correspondingly modified, and this might well be the case from what we know of the connection between the psychical and physiological parts of the reproductive function. This presupposes some difference of smell, form, colour, &c., to enable an animal to distinguish those of its own family from the rest of the species, but this probably exists between any two animals.

They might thus be under no great disadvantage compared with the parent species, and they would have a counterbalancing advantage in the much greater adaptability to circumstances which a small group possesses. Any useful variation occurring in a large group, if not swamped by the effect of interbreeding with a large number of unimproved forms, must take many generations to modify the whole mass; while a similar useful variation occurring in one member of a small and physiologically isolated group could modify the whole group in a few generations. The existence of a six-fingered man in England would not appreciably modify the inhabitants in a thousand years, even if it was a slight advantage to have six fingers; while if a six-fingered man was introduced into an island with five other inhabitants, a fair proportion of the population would probably be six-fingered in three generations.

It is perhaps worth pointing out that the curious connection between colour and fertility, in which Mr. Wallace seeks for the explanation of the sterility of species, follows at once as a corollary from the doctrine of physiological selection. For, apart from any special modification of the sexual instinct, all animals seem to prefer to breed with those of their own colour, and hence *any* change of colour in the isolated family would be an advantage, and would indeed remove the one disadvantage under which such a family lies. So a change of colour, otherwise useless, would in such cases be preserved, and be found accompanying sterility with the parent species.

Another of Mr. Wallace's objections seems to me a strong argument in favour of my (and Dr. Romanes's) theory. He says that some animals, not only of different species but of different genera, can produce hybrids, and he instances the pheasant and black grouse. Now this is just what we ought to find on our theory, and ought not to find on any other. If either structural divergence or divergence in colour produces infertility, then the pheasant and the black grouse should be sterile, since they differ more, both in structure and colour, than many sterile pairs. But if species are produced sometimes by physiological isolation, but sometimes by other causes, such as geographical isolation, spontaneous distaste (not disability) for pairing, or even unaided natural selection, then those species which have been produced by aid of any of these latter processes will be fertile in spite of any ordinary amount of divergence, since nothing has occurred to render them otherwise; while those which have been formed by physiological isolation will be sterile even though they have hardly diverged at all. We cannot tell, without assuming what I am trying to prove, what form of isolation has been at work, except in the case of island species; but we can tell that there ought to be both very divergent fertile forms and slightly divergent non-fertile forms, and this is the case.

It has also been objected that the gradual increase of sterility, as we pass from different species to different genera and families, proves that divergence produces sterility. But it would exist on my theory; for if physiological isolation, more or less complete, occurs before each species is formed, it will have occurred at least twice between the members of two genera, and more often between those of two families. If B is separated from A by being nearly infertile, and C from B in the same way, C is likely to be still more infertile with A. But in some cases geographical or other isolation takes the place of physiological isolation, and then any number of successive divergences may occur without any accompanying infertility.

It has been said (I have lost the reference) that a certain amount of sterility has resulted in some cases from the divergence produced by artificial selection. It may be so. But on my theory, physiological isolation, the spontaneous occurrence of a fertility circumscribed by the boundary of common parentage, must be of very common occurrence, since it must have occurred not only once for each of most of the recognised species, but many more times when the resulting species has died out, and in some cases where the two species, though still existing, have not diverged in any way so as to suggest to observers that they are not one, (just as many island species do not differ perceptibly from those on the mainland). If spontaneous physiological isolation is so common, it would be certain to occur, at any rate in its commoner partial form, among the great variety of our domesticated animals, even if, as I believe, ordinary variation has no tendency to produce it.

EDMUND CATCHPOOL

Friends' Institute, E.C., October 13

Note upon the Habits of Testacella

BETWEEN four and five months ago I found eleven specimens of this slug upon a low wall surrounding the garden of a house

near the Oxford University Parks, and on the following day I captured eleven more in the same place. There had been exceptionally heavy rain, extending over some days, immediately previous to those on which I found the specimens, and it therefore seems probable that these animals are driven out of the earth when it becomes sodden with moisture. Thus it is possible to account for the capture of a very unusual number of specimens, for, as far as I can learn, the species has hitherto only been met with singly in this locality.

I have also ascertained what happens to the animals when the earth in which they are contained becomes hard and dry from the loss of water. A few of the twenty-two specimens were killed and hardened, and the remainder were put in a box containing earth, in which they buried themselves. In the press of other work the box was neglected, and remained untouched in my laboratory until to day, the earth having quickly dried into a hard cake. To-day I emptied the box, and fully expected to find the slugs dried up dead, but to my surprise I found twelve specimens alive, each encysted in a thin transparent capsule formed of the hardened mucous secretion of the animal's skin. The body was contracted, and oval in shape, but it had been so completely protected from evaporation that there was no noticeable reduction in bulk after these hottest months of the year, during which water had been entirely withheld. One or two specimens had died almost immediately after capture, and a few escaped, so that all those which bad been exposed to the heat and dryness in the box had become encysted, and survived in apparent bealth. EDWARD B. POULTON

Wykeham House, Oxford, October 19

Lepidoptera and Migration

THE subject of migration in connection with Lepidoptera is beginning to receive some attention at the hands of our best lepidopterologists. I am decidedly of opinion that the abundance or scarcity of many species of Lepidoptera is largely regulated by migrations from abroad. Last year our southern shores were visited by an abnormal number of rare *Sphingidæ*, but this year there have scarcely been any records of captures published concerning them. It would be interesting to know what are all the influences which cause these migrations, and if there is a periodicity to the phenomenal occurrences.

Birmingham, October 12 W. HARCOURT BATH

The Earthquake of October 16 in the Vosges, &c.

MAY I be allowed to call attention to the fact that Alsace lies on the direction of the great circle, "boundary of Tertiary formation of the United States," mentioned in my letter which appeared in your number of the 14th inst. (p. 570), and furthermore that Strasburg has been repeatedly shaken since 1355, the first date which I found recorded as having been marked by a shock. It is quite true that the interval between that shock and the next recorded (1556) was 201 years; but the greatest subsequent interval, that between the shocks of 1577 and 1655, was only of 78 years. This interval represents a multiple of 13, being $= 13 \times 6$. The interval of 13 is of frequent recurrence, as I purpose to show in a paper which I have about terminated on this question of intervals and periods of earthquakes.

J. P. O'REILLY

Royal College of Science for Ireland, Stephen's Green, Dublin, October 23

RECENT ORNITHOLOGICAL WORKS

THE future student of British birds ought to have little difficulty in working out the distribution of species within the shores of Great Britain, so much excellent work having been done in the way of local lists during the last few years, and certainly one of the most useful will be the little work on "The Birds of Cumberland and Westmoreland," just issued by the Rev. H. A. Macpherson and Mr. W. Duckworth.[1] The situation of these two counties is interesting, especially to the student of migration, and the notes on the passage of water-birds and sea-birds are particularly good. The completeness of the information, and the concise and simple form in which it is conveyed, render this small book a model of what a faunistic work should be, and it forms a worthy accompaniment to the many excellent county lists of birds which have appeared in England during the last twenty years. It would be well if every expiring species in Great Britain had had its death-song as well sung as is the case with the Dotterell, by Mr. F. Nicholson, in the present work. While Protection Acts are spreading their ægis over many birds in the breeding season, so that the numbers are visibly increasing, and the enlightened care of a few landed proprietors aids the work of bird-preservation, there are still a certain number of species whose nesting days in this country are numbered, and which, like the Great Bustard and the Bittern, are doomed by the inexorable advance of civilisation to seek less over-crowded countries in which to breed. The (too probably final) breeding of the Dotterell in Cumberland is therefore appropriately described by Mr. Nicholson, who has himself taken the eggs in the county. An excellent account is likewise given of the breeding of the Pied Flycatchers.

We learn with some surprise that the White-headed Long-tailed Titmouse of Scandinavia, the true *Acredula caudata* (Linn.), "may be detected in Cumberland in mid-winter," when "the appearance of a flock of adults in their snow-white caps is refreshing to an insular observer.' We should like to see some of these Titmice, and may state that an example is a desideratum to the national collection, where we should be glad to receive a specimen. Our experience in France, where we have shot *all three* races of the *Acredula caudata*, is that it is impossible to detect the difference of the forms when in the open, though a difference in note led to our recognising *A. irbii*. We can only consider the true *A. caudata* to be a very occasional, though not impossible, migrant to our shores, and we by no means sympathise with the authors of the "Birds of Cumberland" in their suppression of the name of *Acredula rosea* for the British Long-tailed Titmouse, from a dislike to the "needless multiplication of species." As regards Great Britain the facts are perfectly plain. The resident *Acredula* is always recognisable, and the white-headed form is only a very occasional winter visitant, and however much they may interbreed in the Rhine Provinces or elsewhere on the Continent, there is nothing of the kind in England, where perfect differentiation exists; and therefore to say that our English Long-tailed Titmouse should be called *Acredula caudata* of Linnæus, is a mistake and nothing else, for that name belongs to the Swedish form. Those ornithologists who continue to do this suppress a most interesting fact in nature, viz. that the isolation of the British Islands from the rest of Europe has produced a well-marked modification in the colour of some of our birds, amounting in certain instances to a subspecific value. The same reasoning applies to the Coal Titmouse, where our authors state that "British specimens have *generally* olive backs, as contrasted with the slate-gray backs of typical German specimens, but intermediate forms occur." This is not our experience. In summer plumage, when the olive-brown tips and the feathers become shed, the back of the English Coal Titmouse is gray, and then it is difficult to tell it from a summer-plumaged *P. ater* from the Continent. But if two winter-killed birds are compared, the difference between the British and Continental specimens is very strongly marked, for the back in the latter remains gray, whereas in the British form it is olive-brown. Having been the first, as we believe, to detect this modification in the British form, we have, ever since we first gave it the name of *Parus britannicus*, assiduously collected a series of specimens in the British Museum, and we have never seen reason to modify our original opinion, nor have we yet seen the intermediate forms for which our authors vouch.

[1] "The Birds of Cumberland critically studied, including some Notes on the Birds of Westmoreland." By the Rev. H. A. Macpherson, M.A., and William Duckworth. (Carlisle, 1886.)

We must not conclude our notice of this interesting volume without calling attention to the excellent plate (by Keulemans) of the Dotterell in summer plumage, and of the useful coloured map, an indispensable adjunct for the proper comprehension of a well-written local avifauna such as the present.

We have recently received from America two publications of the highest value, dealing with the expeditions prosecuted under the auspices of the United States Government in the high north. In the *Bulletin* of the United States National Museum, Dr. Leonard Stejneger gives his "Results of Ornithological Explorations in the Commander Islands and Kamchatka." Dr. Stejneger has worked hard for several years at Palæarctic and Nearctic ornithology, and of the many important contributions from bis pen the present *Bulletin* is perhaps the most noteworthy. Although Kamchatka has been visited by several of the Russian naturalists, the published information respecting its ornithology has been meagre enough, if we except the labours of Dr. Dybowski, whose collections have been described by himself and Dr. Taczanowski. Dr. Stejneger divides his work into three parts: (1) a review of the species of birds collected or observed by himself on the Commander Islands and at Petropaulowski in Kamchatka; (2) a synopsis of the birds reported to inhabit Kamchatka; and (3) conclusions. One hundred and forty species were obtained by Dr. Stejneger himself, and of all of these he gives a full account, with synonymy and measurements. Naturally, the Auks, which abound in the Japanese and Kamchatkan seas, come in for a large share of attention, and Dr. Stejneger's description of these difficult species is very complete, especially as regards the shedding of the lamellæ of the bill, on which Dr. Bureau and Dr. Dybowski have had much to say in their writings. As these observations are accompanied by illustrations coloured on the spot by the author from freshly killed birds, there can be no doubt that they will be of inestimable value to the future student. Several new species are described, most of them, as might be expected, being extreme forms of Siberian birds. *Cuculus peninsulæ* represents *C. kelungensis* of Japan, *Dryobates purus* and *D. immaculatus* take the place of the Palæarctic Woodpeckers, *D. major* and *D. minor*; *Pica Kamchatica* that of the Magpie, *P. caudata*. The synonymy of the Redpolls is well treated, and a series of exhaustive measurements given. We can scarcely believe in the new race of the Scarlet Grosbeak, *Carpodacus erythrinus grebnitzkii*, seeing the variation which takes place in a series of *C. erythrinus* from other localities; nor can we distinguish the Kamchatkan race of the Yellow Wagtail, *Budytes flava leucostriatus*, which seems to us to be inseparable from *B. flava* of Europe, of which species it is an Eastern colony. If the differences pointed out by Dr. Stejneger hold good, it will solve an interesting problem in geographical distribution, because it will be much more simple to recognise three forms of *B. flava*: the ordinary one breeding in Europe and wintering in Africa; an Eastern form, *B. leucostriatus*, breeding in Eastern Siberia and wintering in the Malay Archipelago; and a central one, *B. beema*, breeding in Siberia and wintering in India. At present we have fully recognised the latter, whereas the European and East Siberian birds appear to be identical, though possessing well-defined breeding and wintering habitats, with the Central Siberian bird interposed as regards its breeding and winter range. Our synonymy of the Pied Wagtails in the "Catalogue of Birds" does not appear to be quite the same as Dr. Stejneger's, but the ground has now been cleared for Mr. Nicholson or the next bold man who makes the *Motacillidæ* a special study. The problems concerning the Wrens of the East which our author presents us with require careful consideration, and show that these representative forms cannot be disposed of in the off-hand way recently attempted by Mr. Dresser. Let no ornithologist neglect to study the "Conclusions" with which Dr. Stejneger finishes his work, as there are some admirable critical notes; and in conclusion we congratulate America on having gained from Europe such a thorough worker as the author of this volume shows himself to be. We have perused every page of his "Review" with interest, only regretting that our series of Kamchatkan birds is so poor in the British Museum that we have not been able to study, as we should have liked to do, with the specimens in our hands.

The bulky volume entitled a "Report of the International Polar Expedition to Point Barrow, Alaska" contains 24 pages of ornithology, by Dr. Murdoch, and many excellent notes on the birds are given. The chief interest centres round Ross's Gull, of which the Expedition "succeeded in obtaining a large series—more, in fact, than there were before in all the museums of the world put together." Coloured figures of the adult and young are given. So, slowly but surely, are the great desiderata of bird collections being rendered available for science. Only a few months ago this Gull was reckoned one of the greatest prizes to be obtained, and now we not only know the immature plumages, but something of its migrations and habits. Its previous rarity may be imagined from the fact that until the present year no specimen existed in the British Museum, where now, thanks to the liberality of Mr. Henry Seebohm, a fine adult bird is exhibited.

Our colleague, Mr. A. G. Butler, has varied his entomological pursuits by publishing a little work on British Birds' eggs,[1] the figures of which he has drawn himself; and he has shown himself as capable a draughtsman of eggs as he is admitted to be of Lepidoptera. The plates bear evidence of the difficulty which the author has experienced in mastering the shadows of the objects, as several plates are differently treated in this respect; and perhaps it would have been better if Mr. Butler had redrawn some of the earlier plates which did not satisfy him. The chromolithography, for which Messrs. Mintern are responsible, is certainly on the whole satisfactory, and may be found available for this kind of illustration, though we notice a tendency to heighten the colour, which in our opinion has hitherto proved an obstacle for the employment of the process as regards birds themselves. The figures of the eggs are, however, undoubtedly good, and no book yet published in this country shows in a better degree the variation in form and colour which eggs are subject to. In this respect Mr. Butler's little work will be useful to all oologists, and it can confidently be recommended to the young beginner as a hand-book. The letterpress is too short for a complete account of the nidifications of the birds, but contains a good deal of information in a narrow compass.

An "Oxford Tutor" in his little work, "A Year with the Birds," discourses pleasantly enough about his feathered favourites; and whether in the classic grounds of a college garden or in the mountains of Switzerland shows himself an observant student of Nature. We have read this book with considerable pleasure, and it has carried us back in memory to many such scenes as the author describes, though we have lacked his easy facility of recording his experiences. As he is interested in migration, we recommend his spending an autumn holiday on the milder parts of the south coast, where he may really meet with a "flock" of Blackbirds, and even of Robins and Hedge-sparrows. So keen an observer will find scope for his energy in the unravelling of some of the difficult problems which yet surround the study of some of our British species, and we may venture to point out to him one or two facts which have occurred to us during the perusal of his book. Thus, when he speaks of the Wheatear in Switzerland (p. 61) as "an English species,"

[1] "British Birds' Eggs: A Hand-book of British Oology." By A. G. Butler, F.L.S. 8vo. (London, 1886.)

we want to know whether it is the large or the small form which he met with. Again, on p. 73, when he talks of the "sheet of water or marshy ground which might attract the waders and sea-birds so commonly found near Oxford," is it not the east wind which drives the latter along the course of the Thames, rather than any peculiar attraction of the country near Oxford? The fact of the Cuckoo carrying her egg in her bill for deposit in the nest of her victims is now universally admitted; but what concerns ornithologists is the greater or less resemblance of the egg deposited by the female Cuckoo to the eggs of the foster-parents which she selects to bring up her young. The Spotted Flycatcher has undoubtedly a song (p. 83), but it is a poor affair, and is heard only, according to our experience, at daybreak. It is true that the Green Sandpiper (p. 86) has really only the legs greenish, but the reason of the perpetuity of the name to which our author objects, is because Linnæus called the species *ochropus*, and the name adopted by the older English writers was the *Green* Sandpiper, which has been handed down to the present generation, as is also the case with the Grey Wagtail. The author knows his "Dresser" and his "Harting," but he can learn something of the affinity of Robins and Redstarts (pp. 88, 101) from Mr. Seebohm in the British Museum "Catalogue" (vol. v.), or in his "History of British Birds." The little essay on the "Birds of Virgil" is most interesting, especially with regard to the *Alcyon*, which, we agree with the author and his authorities, was probably *not* our Kingfisher, though the presence of the latter on the sea-shore in some numbers is a fact at the season of the autumn migration. We offer these few remarks to the author as points of further study on which we should be glad to have the observations of a true naturalist, such as he evidently is.

The Rev. Gregory Smart has recently published a little book on the "Birds on the British List," which is a *critique* on the list issued by the British Ornithologists' Union, which he collates with the works of Mr. Dresser, Mr. Seebohm, and the fourth edition of "Yarrell." The book is disfigured by a slovenly style of writing, for which the printers' errors can scarcely account, and this is the greater pity, as the author's intentions are good, and he scores distinctly on several occasions when dissecting the evidence on which some birds are admitted to the British List while others are rejected; but the English in which he endeavours to record his conclusions is, to say the least of it, a little mixed. The author confesses to "having had but little experience," and it would therefore have been better to have restricted himself to the main object of his book, viz. the criticism of the evidence on which some species are retained or rejected in the works above alluded to. When he gets outside the boundary of his *critique*, he talks in some instances simple nonsense, as in his remarks on the Gold-vented Bulbul (p. 18). Mr. Dresser and Mr. Bidwell, in aiding and abetting Mr. Smart in his hap-hazard identification of his Bulbul's egg, could scarcely have expected the punishment of having their rash opinion published to the world. On p. 41 some more nonsense appears about *Anthus ludovicianus* and *A. campestris* being conspecific! The author would seem to be unaware that "Ungaru" is generally spoken of as "Hungary" by English writers, and that "Los Angelos Cala" is a locality which will puzzle many an "inexperienced" collector for whose benefit Mr. Smart professes to write. We would advise the author, before publishing another book, to get some friend to look over his manuscript for him, as a good deal of the difficulty of unravelling the meaning of his sentences would have been avoided by a simple attention to stops and commas, which is not too much to expect from a "late Scholar of Trinity College, Cambridge."

R. BOWDLER SHARPE

SOLAR PHYSICS[1]

MOST of our readers are aware that the sun, as constructed by Zöllner, was a white-hot, liquid body, that its spots were scoriaceous products of local cooling, and that its atmospheric circulation was closely modelled upon the terrestrial, with trades and anti-trades, an equatorial belt of calms, land- and sea-breezes, the last due to the contrast of temperature between the slag-islands constituting spot-nuclei, and the incandescent ocean in which they floated. On these lines M. Schulz has reared a solar edifice out of materials to a large extent new. Sixteen additional years of results in one of the most rapidly progressive branches of modern physical astronomy, give him an advantage over his predecessor, utilised to the utmost in modifying, extending, and generalising views of which he is the intrepid, though not blind, partisan. The upshot, we venture to assert, is to prove them wholly untenable. If M. Schulz's ingenious advocacy fail to recommend them, their inherent weakness must be great. Our readers shall judge for themselves of its success.

In the work before us it is undertaken to account for the whole array of solar phenomena, from the conservation, through long geological ages, of the solar activity, and its cyclical fluctuations, to the production of a pore or a facula. With this alluring prospect in view we are invited to regard the sun as a liquid globe composed of unknown substances, glowing at a temperature somewhere between 10,000° and 20,000° C. Although the heat rises towards the centre by a very low gradient, the inequality suffices to insure the distribution of the loss by radiation throughout the bulk of the globe, vertical convection-currents carrying down the cooler and heavier outer layers, and replacing them with hotter and more buoyant materials from the interior. Thus the danger is averted of the light-and-heat-giving career of our luminary being brought to a premature close by the untimely formation of a crust. The relative permanence of that career is further secured by the application to a liquid sun of Helmholtz's gravitational principle of the maintenance of solar heat.

The extensive atmosphere surrounding this molten mass is composed mainly of the unknown gas emitting Kirchhoff's "1474" line. This, in M. Schulz's opinion, is the primitive and simplest form of matter. Its atoms, many times lighter than those of hydrogen, are the fundamental units by the various aggregation of which the atoms of all other substances whatsoever are constructed. It plays, as we shall presently see, a very important part in the solar economy devised by our author. The solar supply of it is on a prodigious scale, since it fills, mixed with small percentages of hydrogen, helium, and metallic vapours, the vast spherical shell visible during total eclipses in the form of a "corona."

In the lowest strata of this gaseous envelope the photosphere hangs suspended at a height of a few thousand miles above the real surface of the sun. Its structure resembles that of our cirrus-clouds, only that metallic and incandescent condensing particles are substituted for aqueous frozen ones. The Fraunhofer absorption M. Schulz brings about by the customary machinery of the "reversing layer," regardless of the growing objections on the part of leading solar physicists to its exclusive employment in that capacity. Indeed the details of spectroscopic evidence scarcely receive from him the minute attention they deserve. Mr. Lockyer's researches on "lines widened in spots" give an example of the kind of work that has henceforth to be done on the solar spectrum. Summary explanations of its phenomena no longer suffice. Each one of its thousands of dusky rays has an individual story to tell, well worth the trouble of

[1] "Zur Sonnen-Physik." Von J. F. Hermann Schulz. Separatabdruck aus der *Gaea*, Bände xxi. und xxii., 1885-86. (Leipzig: W. Drugulin, 1886.)

inquiring into. Each has its own significance, and might be made the subject of a separate, and not unfruitful, study.

Prominences and spots are, by our author, connected together as cause and effect, but in the inverse order of their probable occurrence. There are strong grounds for the belief that the initial disturbance is that which occasions a spot, eruptive appearances ensuing consequentially. But, if M. Schulz's account of the matter were correct, no spot could arise without an introductory display of spontaneous and preliminary flames. Prominences, in his scheme, are composed exclusively of the green coronal gas "1474." It is true that in the spectroscope lines of hydrogen and helium are visible, but their meaning, we are told, has been misinterpreted. They take their origin, not from the body of the prominence, but from the glowing sheath with which the resistance of the solar atmosphere to its upspringing encompasses it. This surprising contention refutes itself. The implied resistance would, in a few seconds, shatter into inconspicuousness the rushing volumes evoking it. "Quiescent prominences," moreover, would on this theory be impossible; yet they are often plainly visible, for weeks together, in virtually unchanging forms; to say nothing of spectroscopic incompatibilities, too obvious to need dwelling upon.

The mechanical power consumed in the projection upwards of these bodies is derived from the expansive force of gas escaping from tremendous pressure. In dilating, however, it loses heat, and at such a rate that by the time the pressure upon it is reduced from ten million terrestrial atmospheres to one tenth of an atmosphere, its temperature has fallen from 12,000° to – 216° Centigrade. The ensuing condensation to the liquid and thence to the solid state brings about a fall of "1474 snow" upon the photosphere. When the shower is a light one, a "pore" is the consequence; when it is heavy and long-continued, the cold falling matter reaches the liquid sheet beneath, a group of "slag-islands" is formed from the chilly contact, and a spot becomes apparent to distant onlookers. The overlying photospheric clouds then arrange themselves, under the influence of atmospheric currents, into the characteristic funnel-shape of the penumbra, at the bottom of which lies the obscure solid nucleus, more or less veiled in dense absorbing vapours.

Improbability raised to an infinite degree becomes impossibility; and we may safely assert that that degree has here been reached. Criticism is silent in the presence of a supposition so fantastic as that of a substance presumably far less condensable than hydrogen existing frozen in the very depths of the thrice-heated furnace of the sun.

So much for prominences and spots: we now come to faculæ. They are regarded by M. Schulz as mere optical effects of irregular refraction in the agitated vicinity of spots. Yet the plainest ocular proof of their being real elevations above the general level of the photosphere is afforded by their not unfrequent visibility as projections from the smooth border of the limb, as well as by Dr. De la Rue's relief-pictures obtained by the stereoscopic combination of photographs.

The (not undisputed) higher equatorial temperature of the sun supplies M. Schulz, as it supplied Zöllner, with a "trade-wind" circulation, by means of which the retarded transport of spots remote from the equator are, with some difficulty, accounted for. Their slight displacements in latitude prescribe the mode of the sun's bodily circulation. A stupendous system of vorticose currents—set going by differences of specific gravity through surface-cooling—is disposed so as to impel such objects slowly towards the poles from about 15° of north and south latitude, while minor equatorial whirlpools give the observed opposite drift within those limits. But such an arrangement, even were it otherwise possible, would reverse Carrington's noted law of solar rotation, the angular rate of which would, under the supposed circumstances, *quicken* with advance poleward, while the maximum retardation would occur somewhere between ten and twenty degrees of latitude.

This rolling movement from within outward of the entire substance of the liquid solar globe, save a small dense nucleus, serves, however, a further purpose. It explains the spot-period.

The occurrence of spots, it must be remembered, depends primarily upon the escape of "1474" gas (which we may designate "coronium") from the interior. But how does it get there? M. Schulz's reply is to the following effect.

Coronium has a powerful affinity for a certain hypothetical solar constituent described as "spot-stuff." In middle and high latitudes a temperature as low perhaps as 9000° C. permits combination which accordingly takes place freely over a vast area. Huge masses of a compound of coronium with "spot-stuff" thus enter into the general circulation, and are gradually carried down to depths where a temperature enforcing dissociation is encountered, giving rise to the formation, under enormous pressure, of gigantic bubbles of pure coronal gas. By their variously-conditioned outbursts these finally occasion prominences and spots, which are more or less numerous according as the distribution of "spot-stuff" is more or less plentiful. Admitting some degree of permanence in its localisation, and assuming that the great vortices whirl once completely round in eleven years, the spot-cycle is established. The equatorial spot-and-prominence minimum finds its *rationale* in the higher temperature by which the occurrence of chemical association in any part of the separate equatorial vortices is prohibited.

We have endeavoured, while omitting details which it would be waste of time to dwell upon, to do no injustice to M. Schulz's ideas in our brief sketch of them. Yet it is difficult to treat them quite seriously; and we confess to a feeling of regret in seeing a writer of M. Schulz's ability and acquirements apply them to the elaboration of so baseless a series of hypotheses—baseless in this sense, that they rest upon a number of postulates which few will be disposed to allow. With sufficient liberality of assumption almost anything can be explained on any desired principles. But this is just the kind of supply which a prudent investigator is most chary of granting either to himself or others. For its misuse undermines the foundations of science, and involves in common discredit illusory theories and legitimate schemes of inductive reasoning.

It is not without cause that solar physicists have adopted what M Schulz calls the "gas-ball theory" of the solar constitution. A mainly liquid sun is for many reasons inadmissible. At a temperature of 10,000° C. and upwards, to begin with, no substance known to us upon the earth can exist otherwise than in a state of vapour. Hence the necessity for having recourse to unknown elements with preternaturally high boiling-points. But a theory of Nature built upon the unknown has, it must be admitted, no very secure basis. Further, the internal stores of heat of a liquid sun could not be made available at the surface. The heterogeneous materials presumably composing it would necessarily arrange themselves, in the order of their specific gravities, into a succession of shells growing in density towards the centre, which no possible convection-currents would have power to disturb. The result would be the formation of a crust, and—so far as we can see—the speedy and final cessation of the radiative function of our luminary.

Some of the points touched upon by M. Schulz are of great interest, and we cannot but feel grateful to him for emphasising them, however little we agree with his methods of elucidation. He shows, for instance, that an atmosphere of hydrogen could not, on any probable assumption as to temperature, extend much more than

3000 kilometres from the photosphere without producing an amount of pressure at its base which certainly does not exist. Yet the spectroscope tells us that incandescent hydrogen is actually present at a couple of hundred times that height. Nor can the anomaly be reconciled by supposing, with our author, the solar atmosphere to be chiefly composed of a very much lighter gas ("1474"), merely, as it were, adulterated with hydrogen. Even if we were satisfied to ascribe to "coronium" an almost impossible degree of elasticity, it would not avail to lift the mingled hydrogen one inch above its natural level. The law of diffusion does not abrogate the law of gravity. Each gaseous ingredient of a mixed atmosphere obeys its own law of equilibrium, as if it existed alone. It is true that the anticipated thinning out of oxygen at great heights in our own atmosphere has not been experimentally verified; but the incessant agitation of the air is believed to mask an effect which should otherwise be perceptible.

The action of a repulsive force, such as is visibly exerted on comets, has been invoked as a means of escape from this difficulty. The supposition has much to recommend it, and would remove a good deal of perplexity; it is, besides, countenanced by the authority of Dr Huggins. But the more tempting it appears, the more severely it should be tested previous to its admission, on other than a provisional footing, among the theories of science.

The slightness of resistance to motion in the solar neighbourhood is one among many indications of the extreme tenuity of matter there. Comets well-nigh graze the sun's surface without experiencing perceptible retardation; and millions of cubic miles of hydrogen sweep onwards or upwards at rates up to 250 miles a second, almost as if *in vacuo*. Since both the moving substance and the medium are incandescent, the varying viscosity of gases at high temperatures would claim attentive consideration in the matter, were it not that reliable data are unfortunately deficient.

The rotational peculiarity of the sun may, however, be said to dominate the problem of its constitution. Three classes of explanation are possible, and have found various degrees of favour. It may be produced by the fall of matter upon the photosphere, by the ascent of matter from beneath it, or by surface-currents. The last was the theory of Zöllner, and has been inherited from him by M. Schulz, but may be dismissed without hesitation as contradictory of known facts. M. Faye's hypothesis of vertical currents bringing up with them a smaller linear velocity is more plausible, but needs peremptory treatment to fit it to the required shape. It is remarkable that M. Belopolsky has lately deduced theoretically, on some not improbable assumptions, Spörer's empirical formula of the diminishing rate of the sun's rotation north and south from his equator (see NATURE, vol. xxxiv. p. 54). The coincidence is striking; but it must not mislead us. It is not enough that a cause be true; it must also be sufficient. Is there any likelihood of its being such in this case? We apprehend that the effects, even supposing them realised to the full, would be microscopic compared with those actually observed. If we take the mean density of the sun at double its superficial density (an outside admission), the maximum of gravity will occur below the surface, at a depth of one-sixth of the radius, and there the *theoretical* rotation-period comes out, by a rough calculation, about twenty-two days. But this period is not in reality complied with. The tremendous hindering power of friction intervenes. It occurs on paper only, and belongs even there but to a single stratum. The effects in superficial acceleration must be quite inconsiderable.

Acceleration from below failing, we turn to acceleration from above. And it has to be borne in mind that the mode of the sun's rotation is inferred from the movements of spots, and from them alone. But if spots be due, as generally supposed, to vaporous down-rushes, they must share in the augmented velocity brought by the materials forming them from regions of wider circumference; and this theoretical necessity is confirmed by the characteristic plunge forward attending the sudden development of these objects. If we assume further that the height of fall, consequently the added linear velocity, diminishes progressively with distance from the equator, the phenomena of spot-transport in longitude are satisfactorily accounted for. Just such a graduated elevation of the sources of spot-supply forms an integral part of Mr. Lockyer's "meteoric-ring theory" of sunspots, communicated to the Royal Society in May last (NATURE, vol. xxxiv. p. 251). The line thus struck out, however it may be modified by future experience, seems to lead, more naturally and easily than any other yet tried, to the solution of the problem of the sun's apparent rotation.

A. M. CLERKE

THE NEW OPTICAL GLASS

EVERYTHING that falls from the pen of Prof. Abbé of Jena relating to optical matters commands respect. His announcement therefore of the discovery of new kinds of glass specially adapted for the manufacture of lenses has been received with peculiar interest; and although details of information on various points are still wanting, enough has been published respecting the new optical glass to be worthy of more than passing notice.

All who have had anything to do with optical instrument-making know only too well the existence of the defect termed "irrationality of dispersion." When Hall and Dollond had independently shown that the chromatic dispersion of a crown glass lens might be corrected by combining it with a second lens of flint glass, a new impetus was given to optical research. The so-called "achromatic" lens in the hands of successive generations of opticians constituted the basis both of the modern microscope and of the modern telescope. But as greater and greater perfection in the construction of the "achromatic" lens was attained, it became apparent that perfect achromaticity was very far from being realised; for though two lenses might be found which should perfectly bring together two widely differing rays, such, for example, as the red of the line C of the spectrum and the blue of line G of the spectrum, it by no means followed that this pair of lenses would bring together to the same focus all other rays. On the contrary, owing to the "irrationality" of dispersion a "secondary spectrum" would always remain uncorrected

The relation between the chemical constitution of a medium and its action on waves of light of different periods is one of those matters about which we are still profoundly ignorant. We know that a prism of glass does not spread out the waves in proportion to their wave-length, or to the frequency of their periods. A simple case of irrationality is afforded in the fact that a green ray which when viewed through one prism may lie exactly half-way between C and G in the spectrum will not lie exactly half-way when viewed through a prism of a different kind of glass. All that a combination of two lenses can do is to achromatise for two rays of the spectrum: it may very nearly achromatise for the neighbouring rays, but strictly speaking it only achromatises for two. For ordinary optical purposes we seek to achromatise for the red and the blue, so reconciling the end regions of the visible spectrum. For photographic purposes we achromatise for green and violet (or even ultra-violet) rays, reconciling the end regions of the photographically active spectrum.

To Dr. Blair, whose observations were published in the *Transactions* of the Royal Society of Edinburgh for 1791, we owe the suggestion to achromatise for three rays by using compound lenses of three different media. Blair, indeed made a most extensive examination into the dispersive powers of various media, and in particular of

liquid media, which he proposed to use in his compound lenses; a fluid lens being formed in a cavity between two lenses of glass. He also suggested combinations of two or more fluid lenses. He found that in the spectrum of hydrochloric acid the green rays lay much nearer the violet than in the spectra of most metallic solutions; and he proposed to use the chlorides of antimony and of mercury in various proportions along with hydrochloric acid, or with sal-ammoniac, in order to obtain a fluid which, while having a different absolute dispersion from crown glass, should have its relative dispersion or relative distribution of the rays of different colours proportionately identical throughout the spectrum. Blair's object-glasses for telescopes, though regarded at the time as of singular merit, never came into use. The only recent serious attempt to revive a triple lens for approximating to a correction of the secondary spectrum by achromatising for three rays has been that of Prof. C. S. Hastings, of Yale, who has used three kinds of glass.

What Blair proposed to effect with his liquid combinations Prof. Abbé claims to have now achieved by his discovery of new kinds of optical glass. To Abbé we owe the remark that, in addition to the secondary chromatic aberration of our so-called achromatic lenses, there exists a second defect, termed by him the chromatic difference of the spherical aberrations. This term he uses to denote the fact that with the crown and flint glasses used by opticians, though the curves of the lenses be calculated to correct the spherical aberration, taking in each case the mean refractive power, there will be a slight residual spherical aberration for all rays not of mean refractive index; the lens being spherically under corrected for red rays and spherically over-corrected for blue.

Having realised so far back as 1880 that these defects were inherent in the use of such glass as opticians had at their disposal, Abbé determined to make a resolute attempt to discover new kinds of glass which should be free from these vices. The research involved no less a field of work than the examination of the optical properties of all known chemical substances which undergo vitreous fusion and solidify in non crystalline transparent masses, together with a detailed comparison of their physical and chemical properties. The work was begun so far back as January of the year 1881 by Prof. Abbé and his coadjutor, Dr. Schott, then of Witten in Westphalia, now of Jena. Dr. Schott undertook the chemical portion and the melting processes; Prof. Abbé and his assistant, Dr. Riedel, conducted the optical examinations of the products. At first only small quantities, from 20 to 60 grammes in weight, were melted at once; all kinds of chemical elements being tried with the view of ascertaining their influence on the refractive and dispersive powers. Not only were glasses of ordinary kinds having silicic acid for their chief constituent tried, but also glasses, resembling Faraday's famous "heavy-glass," made from boric acid and also phosphoric acid. So, by the end of 1881, a series of fundamental facts in chemical optics were gathered together for future use. The next point was to use these chemical researches as the basis for the production of real glass possessing the necessary qualities of hardness, colourlessness, and unalterability. To carry out this work, Dr. Schott moved to Jena early in 1882, and set up a special laboratory for scientific glass-making with proper blast-furnaces, and smelting-pots in which quantities of 10 kilogrammes could be melted at once. Until the end of the year 1883 this research-laboratory was occupied almost exclusively in working toward the solution of two practical problems. The first of these was the production of pairs of kinds of flint and crown glass, such that the dispersion in the various regions of the spectrum should be, for each pair, as nearly as possible proportional. The object of this was to permit of a higher degree of achromaticity than hitherto possible, by diminishing the secondary colouring effects inseparable from the irrationality of the ordinary silicate flint and crown glasses. The second problem was the production of a greater multiplicity in the gradations of optical glass in respect of the two chief optical constants, the index of refraction and the mean dispersion. Though this has not generally been regarded as an important need in optics, it was considered by Prof. Abbé and Dr. Schott as of quite equal importance to the first. For the silica glasses hitherto used constitute, according to their composition, a simple series, from the lightest crown to the densest flint, in which, with an increase in refractive index, there is practically always an increase in the mean dispersion. But there is no doubt that for many purposes of practical optical instrument-making, particularly in the designing of optical combinations for special purposes, it would be a great benefit for the optician to have at his command other kinds of glass in which the refraction and dispersion are not related in the way in which they are in the silica glasses; for example, a glass having great refraction and small dispersion. Hence the multiplication in the available kinds of optical glass opens out new possibilities of great practical moment. Pending the publication of these very valuable scientific investigations, only a very brief account can be given of the actual results. The first problem has been satisfactorily solved, with the result that achromatic lenses of a much more perfect kind than has ever been attainable are now in the market; and the second has also been successfully carried out, a whole series of new glasses of graduated properties having been introduced into the optical trade.

Down to the autumn of 1883 the matter was one of systematic scientific research; but at that date, encouraged by the substantial fruit borne by the investigations, a further step was taken. In conjunction with the brothers Dr. Carl Zeiss and Dr. Rod. Zeiss, of Jena, whose names are household words to every microscopist, a commercial enterprise was set on foot for establishing a new glass-foundry. This establishment, which, under the style of Schott and Company, has been at work since the autumn of 1884, produces glass of all kinds, old and new, on a large scale. The first application of the new glass to the microscope comes naturally from the famous firm of Zeiss. To his new microscope lenses Prof. Abbé gives the distinctive name of *apochromatic objectives*. He claims for them great superiority in the finer qualities of definition, the new dry apochromatic lens giving an image equal to that of an ordinary achromatic water-immersion objective. He also claims that the more perfect corrections permit equal magnification to be obtained by using a longer-focus objective with an eye-piece of higher power than hitherto has been usual, thus obviating some of the difficulties of very short-focus objectives. Moreover the foci for visible and for photographic purposes are identical. Special compensating eye-pieces have been also devised for use with the new apochromatic objectives.

Whether these new appliances are found to fulfil under the test of experience all that their inventor claims for them remains yet to be seen; but it cannot be doubted that a great step has been taken. It ought also to be recorded to the credit of all concerned that no attempt is being made to secure to one firm a monopoly of the new materials, but that the new optical glass is offered to the optical trade without any restriction or patent to stand in the way of further development. Nor less honourable or significant is it that the researches of the "Glass-technical Laboratory" of Jena should have been supported by a very liberal and several times repeated subvention from the Prussian Government. Such a result arising from the endowment of research makes ample answer to the easily-uttered assertion that such endowments, if given, would be wasted on useless fancies and trifling schemes. We sincerely congratulate Prof. Abbé and Dr. Schott on the completion of their most meritorious labours.

THE GERMAN NAVAL OBSERVATORY[1]

IT will be remembered by our readers that in the spring of the present year a review of the sixth yearly Report of the German Naval Observatory at Hamburg was given (NATURE, vol. xxxiii. p. 411), in which the objects and the general system of working in the several departments, as well as some special papers on subjects connected with the Observatory, were noticed.

In this, the seventh yearly Report, there is the same evidence of the progress of useful work in all departments described in the first of the four papers which it contains, but not requiring special remark. In Paper No. 2, however, there is a detailed account of the building in which this work is carried on, and a general description of the instruments employed, which can hardly fail to be of interest even to those who may have personally visited the Observatory.

The traveller approaching the docks of Hamburg by the Elbe, will see a square sandstone building in the Renaissance style situated on an eminence which rises abruptly 100 feet above the river, between Altona and Hamburg. This is the Naval Observatory, in an excellent position for observations, and commanding an extensive view of surrounding objects for many miles, close to the shipping for the welfare of which it was chiefly established, and as it were inviting the commanders to come and partake of the advantages held out to them. The main building—which in plan forms a hollow square, and consists of a basement, ground floor, and two stories above, with ample internal galleries and staircases for communication with the various rooms—was commenced in 1879 and completed in October 1881, a Naval Exhibition in the lower rooms having been opened in the previous month by the Emperor of Germany in person.

Over the principal entrance, which faces towards the south-west, are three busts of well-known scientific worthies, that of Dove being in the middle, with Maury and Rümker right and left. The square inside the building is roofed with glass, giving protection to the galleries and the Combe apparatus which occupies the floor, whilst it proves but a small obstruction to the light.

A view from the north-west side of the building will be found on Plate 1, and on Plate 2 a plan showing the general disposition of the adjoining structures.

Preceded by some historical references to the site now occupied by the Observatory—where fortifications formerly existed—in pp. 5-12 will be found a description of the uses to which the various rooms are devoted, with references to the twenty-nine excellent plates, showing their size, and the mounting of the various instruments in them, both in elevation and plan. At each angle of the building there is a low square tower. On the western of these the anemometers and wind-vane are mounted, with electrical communications to the registering apparatus. In the south tower is the apparatus for proving sextants, for which the known angles between well-defined distant objects are used, it being a rare occurrence for the latter to be obscured by fogs. A transit instrument occupies the eastern tower, and an alt-azimuth instrument the northern.

From pp. 12-26 detailed accounts are given of the principal normal and self-registering instruments, the laboratory, the compass observatory, and the museum with its contents. Amongst the special apparatus a registering rain-gauge is described at p. 27, with an illustration; this, with the sliding-weight barograph described on p. 29, was designed by Dr. A. Sprung.

The magnetic pavilion in the garden is chiefly devoted to experimental determinations of the induction-coefficients for various kinds of iron, and instruction to students in magnetism.

[1] "Aus dem Archiv der Deutschen Seewarte." VII. Jahrgang, 1884. Herausgegeben von der Direcktion der Seewarte. (Hamburg, 1886.)

Some remarks on the uses of the Combe apparatus, founded on information of a later date than that of the present Report, may possibly be of interest. A doubt has already been expressed (NATURE, vol. xxxiii. p. 411) as to the value of the effects produced on chronometers by machinery for simulating the rolling and pitching motion of a ship at sea. This doubt has been confirmed by experience, and some additions have been made to the apparatus by which the effects of shaking such as might be caused by the racing of the engines on board ship or the blow of a heavy sea are introduced. These effects of shaking are clearly shown in the rates of the chronometers tried.

As the Combe apparatus can be rapidly revolved by means of a gas engine, its uses have been extended to the trial of anemometers and the measurement of wind-pressure, the arrangements for which may be seen on Plate 18, and a description of the same on pp. 12-15. It is reported that the results obtained are satisfactory with the exception of minor effects on the anemometers caused by draughts passing through the arched openings to the adjoining corridor, but this is in a fair way to be accurately accounted for.

In Paper No. 3, the course of instruction used by students at the Observatory on the mathematical treatment of the deviation of the compass, with examples, is shown in detail. A certain knowledge of mathematics and mechanics is required of the students preliminary to this instruction, but they have the assistance of Dr. Neumayer's deviation model (NATURE, vol. xxxiii. p. 587) for experimental illustration.

The formulæ used are, with one exception, those of the Admiralty Manual for Deviations of the Compass, published in London in 1869.

The exception will be found on pp. 29 and 30, where it may be seen that a new term is introduced into each of the equations representing the fore-and-aft and transverse magnetic forces of an iron ship. The object is to give a mathematical expression for the temporary changes which are produced in an iron ship's magnetism when her course lies in a given direction for a few days, or even hours, under certain conditions—the change only becoming apparent on alteration of the course. The amount of change experienced depends upon the quality of the ship's iron, the position of the compass, the length of time she is kept on the given course, the degree of shaking she is subjected to during that time, and is proportional to the earth's magnetic force at her mean geographical position. The question is more fully discussed in the *Archiv der Deutschen Seewarte* for 1879, No. 4, where some values of the changes denoted by the constants V and V' for certain ships are tabulated, but these values are dependent on so many contingencies, that nothing but carefully-conducted observations systematically made for each ship can give exact results. It may be remarked, however, that from results of the kind just described properly analysed, much useful information might be deduced and furnished to iron vessels proceeding on a voyage, as to the probable changes in their magnetism under various conditions.

The general rule at present is to depend entirely on observations of the deviations of the compass and their registration for future guidance when observations cannot be obtained. There is much to be said in favour of this rule, but there is also much to be hoped for from the more scientific treatment. It may be added that as similar results are often obtained in vessels of like types of construction, the analysis of observations from many vessels is much to be desired for the guidance of commanders of vessels starting on their first voyage, possibly, in weather when objects on land or in the sky are invisible.

In the fourth and concluding paper an account is given of the observations made by Dr. Richard Neuhauss during a voyage from Germany to Australia by the Suez Canal

route, returning by New Zealand, Samoa, the Sandwich Islands, San Francisco, and New York. The observations relate principally to barometrical heights, temperature of air and sea, moisture of the atmosphere, and twilight phenomena, including zodiacal light, whilst passing through the Red Sea and tropical portions of the Indian and Pacific Oceans.

Although in previous yearly Reports Dr. Neumayer has published much information respecting the internal working, aims, and results of the Observatory he so ably directs, it is only from the perusal of this seventh Report that a full idea of the pains which Germany has taken on behalf of her sea-going population, in building and equipping at considerable expense the noble Naval Observatory at Hamburg, can be obtained. It should also be remembered that there are several minor affiliated institutions in Germany of like intent.

THE INSTITUTION OF MECHANICAL ENGINEERS

AT the recent meeting of the Institution of Mechanical Engineers, held in Leeds under the presidency of Mr. Jeremiah Head, a paper was read "On Triple-Expansion Marine Engines," by the late Mr. Robert Wyllie of Hartlepool. During the last few years the high-pressure triple-expansion engine has proved the successful rival of the double-expansion compound, and the object of the paper was to bring forward the results of recent experience with this new type of engine, and to consider briefly the various points which have a direct bearing on its efficiency, as well as the most suitable design for marine purposes. The general conditions of efficiency were stated to depend on the approximate equality in the range of temperature in each cylinder, in the initial stress on each crank, and in the indicated horse-power of each engine. As complementary to these are steam-jacketed cylinders and other matters which are first treated of. As regards steam-jackets, when in triple-expansion engines attention is paid to the equal division of the total range of temperature amongst the cylinders in which the successive stages of expansion take place, the benefits arising from the use of steam-jackets are naturally not so great as in single-cylinder engines with a high rate of expansion; but however carefully the triple engine may be designed, the jacketing of at least the intermediate and low-pressure cylinders is essential to maximum efficiency. The ratio of the cylinder capacities depends on the pressure of the steam and type of engine, the high-pressure cylinder being larger in proportion where large range of power and economy of fuel is not so important, as in war-ships as compared with cargo steamers. To obtain even approximate equality in powers, temperatures, and stresses requires the greatest care in designing the steam-passages throughout the engine, so that the velocities of the steam at the various points and the degrees of cut-off by the valves may be carefully proportioned. Too much care cannot be taken in the design of the steam-ports and exhaust-passages of the low-pressure cylinder. The ports should be as small as possible to reduce the clearance to a minimum, and the speed of the entering steam should not be so high as to cause excessive frictional resistance, nor that of the exhaust so high as to augment the back-pressure, and consequently the greatest efficiency is obtained when the revolutions and indicated horse-power are not required to vary to any great extent. Contracted or indirect exhaust-passages in the high-pressure and intermediate cylinders have the effect of causing a larger difference between the back-pressure on one piston and the initial pressure on the next, thus diminishing the efficiency of the steam. The cut-off necessary for the highest efficiency is governed to a great extent by the speed of the entering steam and the nature of the passages. In the intermediate and low-pressure cylinders, too high a velocity of the entering steam will produce excessive frictional resistance, causing a drop in the expansion-curve, as well as unduly high receiver-pressure, thus disturbing the equality of temperatures and of initial stresses. Some diversity of opinion has existed as to the order of sequence for the three cranks. The author considered the best sequence to be the high-pressure leading, low-pressure following, and intermediate last. As regards the number of cranks, the best design is to have two cylinders on each crank for a two crank engine on the triple-expansion system, as it is then possible to get an approximately equal initial stress on each crank, the arrangement necessitating one of the three stages of expansion taking place in two cylinders instead of in one. A marine engine should be so designed that any working part can be easily examined or removed, the arrangement of cylinders on three cranks fulfilling the required conditions more nearly than any other design. The requirements of a good valve-gear are, that it shall give at both ends of the cylinder an equal distribution of steam at all grades of expansion, with a minimum of working parts and no undue strains. The four principal methods are: by the single eccentric, by the double eccentric, by taking the motion from the connecting-rod, and by a compound motion derived from both the piston-rod and the connecting-rod; all have their advantages and defects, and vary considerably in complexity and multiplicity of parts. A comparison of practical results with compound- and triple-expansion engines is in favour of the latter, as regards dead-weight carried, speed, indicated horse-power, and coal-consumption, the latter being so low as 1·41 lbs. per h.p. per hour, proving this class of engine to be most efficient. The paper concluded with a reference to artificial draught for boilers, in the special arrangement described the air being heated both inside and outside the uptake, balanced fire doors being applied, which on being opened shut off automatically the hot air supplied by the fan both above and below the fire-bars.

An important discussion followed the reading of the paper. Mr. Wm. Parker, of Lloyd's, looked upon the triple-expansion engine as the engine of the future. Profs. Kennedy and Smith drew attention to the high initial pressures employed in the triple engine, 150 lbs. per square inch as compared with 75 lbs. per square inch in the compound engines being the cause of their higher efficiency; and other speakers having drawn attention to special features in the designs, the further discussion of the paper was put off to the spring meeting of the Institute.

Afterwards the President, by request, declared the Leeds Engineering School of the Yorkshire College open, and spoke in hopeful terms of the useful work it had doubtless before it.

NOTES

WE regret to have to announce the death last week of Dr. Guthrie, Professor of Physics at the Normal School of Science.

WE understand that the Jardin des Plantes has acquired for its botanical collections the herbarium of Lamarck. We do not know under what circumstances this had travelled so far from France. But until recently it was the property of Dr. Roeper, Professor of Botany in the University of Rostock. He incorporated it with his own herbarium, and the whole was purchased at his death by the University for 21,000 marks (1050*l.*). Lamarck's plants have however been separated, and as we understand purchased by the French Government.

THE front and sides of the new building for the galleries of natural history in the Jardin des Plantes are now nearly complete. But it will be two years or nearly so before the interior and the fittings can be finished. The collections will then be moved into the new portion, and the present galleries rebuilt to form the back of the square. The hollow interior will be covered

with a glass roof, and will thus be available for the reception of large objects. In the new front the mammals will occupy the ground-floor and the birds the gallery above.

The French National Museum has received a valuable collection of mammals obtained during M. de Brazza's recent expedition in the Congo district. In it are examples of two new and well-marked species of monkeys of the genus *Colobus*, and specimens of a very fine new *Cercopithecus*, allied to *C. diana*, which M. Milne-Edwards has named *C. brazzæ*. There are also examples of several other mammals of considerable interest. Looking at these discoveries and others recently made in Somaliland, it is evident that the mammal-fauna of Africa is by no means yet exhausted.

The experiment has been tried at the Finsbury Technical College of giving free Saturday evening popular lectures and of afterwards allowing the visitors to see over the laboratories and workshops. The lectures were given by the Professors of the College on the following dates:—October 2, Prof. S. P. Thompson, D.Sc., on "Waves of Light"; October 9, Prof. J. Perry, F.R.S., on "Spinning Tops"; October 16, Prof. R. Meldola, F.R.S., on "Coal, and what we get out of it." The concluding lecture was given on Saturday last, October 23, by Prof. Thompson, on "Magnets and Electro-magnets," the Lord Mayor taking the chair for the occasion. The numbers of visitors filling the lecture theatres on these occasions show that the movement has been appreciated by the public.

Prof. Frederick McCoy, of the University of Melbourne, has been appointed a Companion of the Order of St. Michael and St. George.

We are glad to hear that the completed volume of the "Zoological Record" may be expected before the end of the year; the reports on mammals, birds, and reptiles were issued to subscribers six weeks ago, and those on insects are now ready.

Two new botanical journals have recently made their appearance in Italy, named—according to the fashion of *Linnæa*, *Grevillea*, and *Hedwigia*—after the two distinguished botanists De Notaris and Malpighi. Three quarterly numbers have now been published of *Notarisia*, a journal devoted to the interests of phycology, issuing from Venice, and edited by Sigg. De Toni and Levi. A very useful feature in this publication is the list, in each number, of the phycological literature, and the descriptions of all new species published during the quarter. *Malpighia*, of which the first monthly number is issued, edited by Sigg. Borzi, Penzig, and Pirotta, and published at Messina, is of a more general character. Besides reviews, short notices, and a bibliography, it contains articles "On the Atomic Weights of Living Things," by L. Errera; "On the Structure of the Nectaries of *Erythronium dens canis*," by S. Calloni; "On Soredial Sporidia of *Amphiloma murorum*," by A. Borzi; and "Researches on a Species of *Aspergillus*," by F. Morini.

A curious custom of the natives of Java in the neighbourhood of the Bromo volcano is recorded in the *Straits Times* of Singapore. It is said that whenever an eruption takes place, the natives, as soon as the fire (the molten lava no doubt is meant) comes down the mountain, kindle at it the wood they use as fuel for cooking. They keep in the fire thus made for years, and whenever it goes out through neglect, or for any other reason, they never kindle it anew from matches, but they get a light from their nearest neighbours, whose fire was originally obtained from the volcano. The fires in use up to the late outburst in the native cooking-places were all obtained from the Bromo eruption of 1832.

The issue for last year (No. 16) of the *Journal* of the Straits Branch of the Royal Asiatic Society does not contain much of specially scientific interest, although there are interesting papers on various subjects connected with the Malay Peninsula. Mr. Perham translates a very old and popular Dyak myth, and Mr. Hale, Inspector of Mines at Kinta in Perak, writes on mines and miners there. Some of the customs described are very curious. The Malay miner, the writer says, has peculiar ideas about tin and its properties. He believes that it is under the protection and command of certain spirits whom he considers it necessary to propitiate. He thinks the tin itself is alive and has many of the properties of living matter, that of its own volition it can move from place to place, that it can reproduce itself, and that it has special likes and dislikes towards certain persons and things. Hence he thinks it advisable to treat tin-ore with a certain amount of respect, to consult its convenience, and to conduct the business of mining in such a way that the tin-ore may, as it were, be obtained without its own knowledge. There is also an interesting vocabulary of the language of the Sulu archipelago, which is said to be a variety of the Bisaya of the Philippines.

On Saturday M. Miclucho Maclay opened at St. Petersburg his small exhibition of ethnological objects from New Guinea and the Malay Archipelago in one of the halls of the Academy of Sciences, and delivered a lecture on Russian colonisation in New Guinea to the assembled visitors.

The International Congress of Hydrology and Climatology met at Biarritz in the first week of October under the presidency of M. Durand Fardel. The number of members reached from 800 to 900.

The National Fish Culture Association are constructing a new hatchery, and making other improvements at their establishment at Delaford Park, towards the expense of which they have received 200 guineas from the Fishmongers' Company. Donations have also been received from the Duke of Bedford, the Marquess of Exeter, Sir Albert K. Rollit, Mr. Mann, and others, towards the same object.

We have received the report of the West Kent Natural History, Microscopical, and Photographic Society for the past year. The Presidential Address by the Rev. Andrew Johnson dealt mainly with the progress made in one branch of mycological science, the *Agaricini*, during the last twenty years, starting from the publication of Berkeley's "Outlines of British Fungology," local Societies such as this are, we think, best judged by the local work they do, not by the lecturers they may succeed in getting to address them. Good-natured scientific men of eminence who will consent to address local Societies can be obtained without very great difficulty if approached in the proper way, but to have good local papers a Society must have local members capable of good steady scientific work. In this respect the West Kent Society is not wanting, for the principal paper in this report is one by Mr. Spurrell, entitled "A Sketch of the History of Rivers and Denudation of West Kent," which is an exhaustive account of the geology of the neighbourhood, which may be presumed to be specially within the scope of the Society's work. It occupies about fifty pages, and has a considerable number of plates and illustrations.

From the Report for 1885 of the Australian Museum, Sydney, we learn that the Museum is open to the public from 10 until 5 o'clock (or in summer till 6 o'clock) on week-days, on Sundays from 2 o'clock to 5. The largest attendance on any one day was 1686. The greatest Sunday attendance was 1230. The average daily number of visitors throughout the year was 264 on week-days and 844 on Sundays. The total for the year was 126,512. The collections are still being increased, by means of purchases, exchanges, and donations; also by collecting and dredging expeditions. A list of these additions, under separate heads,

is given in appendixes. Among these may be specially mentioned presentations in exchange from the Royal Museum at Florence; about 2000 Indian fishes from Dr. Day; three whales, caught on the coast of New South Wales; and sundry specimens of ethnology from the South Sea Islands, casts of natives of Micronesia and Polynesia, from Dr. Finsch, &c. A compass and collecting-jar belonging to Capt. Cook, purchased by the Agent-General and presented by the Colonial Secretary, are not without interest. Great alterations and improvements are still in progress, by the erection of additional glass cases. It is contemplated shortly to re fit the old wing of the Museum with cases suitable for the mineralogical collections, which will then be exclusively placed there. Rooms have been fitted up for the osteological collections, which are now being removed thither from other parts of the building. The want of sufficient space in the present building for the constantly-increasing number of exhibits is still felt as a serious drawback to the usefulness of the Institution. The trustees are preparing a proposal for consideration by the Government, which, if adopted, will provide a useful and permanent extension now urgently required, at a comparatively small cost. Catalogues, not only of the various collections in the Museum, but also of all branches of Australian zoology, are still in course of preparation. The following new publications have been issued during the past year:—"Catalogue of Hydroid Zoophytes"; "Catalogue of Echinodermata, Part I. Echini"; "Catalogue of Minerals." Catalogues of Sponges and Medusæ are in the printer's hands; and it is hoped that, by the liberality of the Government, the continuation of "Scott's Lepidoptera" will soon be commenced.

A TELEGRAM from Srinagar, Cashmere, states that a severe and prolonged earthquake was felt there early on the morning of the 20th inst. The damage caused is not yet known. Sharp shocks of earthquake were felt early on the morning of the 22nd at Charleston, Savannah, Augusta, Columbia, Orangeburg, Wilmington, North Carolina, and several other places. The first shock of earthquake was felt at 5 o'clock in the morning, and some sharp shocks followed at 3 o'clock in the afternoon. No damage is reported to have been done. A shock was felt at Louisville in the afternoon. The shocks of earthquake felt in the evening were more severe than any that have been experienced since the great earthquake of August 31. The shock was severely felt at Summerville, where some persons were thrown down and slightly injured. Several geysers have appeared in the neighbourhood of that town.

A CORRESPONDENT of the *Times* (October 21) who appears to have made a special study of Burmah, referring to the ethnology of that country, says that tradition points to three main streams of colonisation into Burmah. The northern sea-board, now known as Aracan, is said to have received its earliest population and polity from the Buddhist kingdoms of Bengal. The southern sea-board, or Pegu, with Rangoon as its modern capital, is supposed to owe its civilisation to settlers who crossed the sea from the Madras coast. The ancient kings of the inner country, which we call Upper Burmah, also claimed an Indian affinity. But, as a matter of fact, they and their people poured across the mountains and down the river valleys from the confines of China and Mongolia. These three prehistoric divisions have left their mark on the political geography of Burmah at the present day. Each of them has in turn advanced upon and crushed its neighbours, while the whole has been from time to time submerged by fresh avalanches of wild races from the north and east.

NEXT March the railway from Oran to Tunis *viâ* Algiers and Constantine, will be completed, and will be between 800 and 900 miles long.

WE have received from Messrs. Goolden and Trotter a small illustrated pamphlet descriptive of their dynamos, which appear to be well-designed modifications of the familiar Gramme machine, lamps, and other appliances for electric light. The pamphlet is also a price-list, and though with the latter feature we have nothing to do, we may remark on a curious and scientific innovation in stating the price of dynamos in a mathematical relation to their output, being, in fact, stated as 8*l*. 10*s*. per 1000 watts plus a constant of 25*l*. Trotter's dioptric shades, which are another speciality of this house, are an example of a great improvement effected in a common manufacture by the application of good geometrical and optical principles, and we have pleasure in noticing them on this account.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Mrs. H. Reader; two Green Monkeys (*Cercopithecus callitrichus* ♂ ♀) from West Africa, presented respectively by Mr. J. W. Bacon and Mr. G. D. W. Ingham; a Canadian Beaver (*Castor canadensis* ♀) from Canada, presented by the Earl of Carnarvon; a Crested Porcupine (*Hystrix cristata* ♀) from Ceylon, presented by Mrs. E. Dunn; a Grey Seal (*Halichœrus grypus* ♀) from the North Atlantic, presented by Mr. H. Overton; a Quail (*Coturnix communis*), an Asiatic Quail (*Perdicula asiatica*) from India, presented by Dr. A. Günther, F.Z.S.; a Pig-tailed Monkey (*Macacus nemestrinus* ♂) from Java, six Mute Swans (*Cygnus olor*), a Cormorant (*Phalacrocorax carbo*), British, deposited; a Scarlet Ibis (*Eudocimus ruber*), a Common Boa (*Boa constrictor*) from South America, received in exchange; a Vulpine Phalanger (*Phalangista vulpina*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE DISTRIBUTION OF THE STARS IN SCHÖNFELD'S "DURCHMUSTERUNG."—The extension of Argelander's survey of the heavens from the North Pole to 2° of south declination undertaken by his successor, Prof. Schönfeld, has recently been published, and contains the places of stars down to mag. 10, situated between 2° and 23° of south declination. Prof. Seeliger, in a paper entitled "Ueber die Vertheilung der Sterne auf der Südlichen Halbkugel nach Schönfeld's 'Durchmusterung,'" published in the *Proceedings* of the Mathematico-Physical Section of the Bavarian Academy of Sciences, has discussed the question of the distribution of these stars. He divides the stars into eight classes. Class I. contains those of mag. 1–6·5, Class II. those of mag. 6·6 7·0, and so on down to Class VIII., which contains stars of mag. 9·6–10·0. Prof. Seeliger then gives the number of stars in each class arranged in zones embracing 1° in declination, and grouped by intervals of 40m. in R.A. The totals for each class are: Class I. 1265, Class II. 1276, Class III. 1828, Class IV. 3516, Class V. 7601, Class VI. 18,633, Class VII. 55,565, and Class VIII. 43,896. The total number of stars thus counted is 133,580, and adding to this 79 objects which are classified as nebulæ or variables, there results the grand total 133,659. This total agrees with the summation of the numbers given by Prof. Schönfeld. Comparing the results contained in this paper with those in a similar paper on the stars in Argelander's "Durchmusterung," Prof. Seeliger finds that, as far as Schönfeld's work can be considered typical of the southern hemisphere as a whole (it must be remembered, however, that it only embraces one-third thereof), the influence of the Milky Way on stellar distribution, at least for stars down to mag. 8, appears to be less marked for the southern than for the northern hemisphere. With regard to the question as to which hemisphere is the richer in stars, it appears that there is no decided difference shown by the two surveys under consideration. Reducing Argelander's numbers so as to make them comparable with Schönfeld's, and taking stars down to mag. 9 inclusive, we have for the former the total 34,324, and for the latter 34,119, a difference which may reasonably be attributed to accidental circumstances.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 OCTOBER 31—NOVEMBER 6

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on October 31

Sun rises, 6h. 54m. ; souths, 11h. 43m. 44·1s. ; sets, 16h. 34m. ; decl. on meridian, 14° 11′ S. : Sidereal Time at Sunset, 19h. 13m.

Moon (at First Quarter on November 3) rises, 11h. 9m. ; souths, 15h. 35m. ; sets, 20h. 0m. ; decl. on meridian, 18° 56′ S.

Planet	Rises	Souths	Sets	Decl. on meridian
	h. m.	h. m.	h. m.	° ′
Mercury	8 56	13 0	17 4	21 42 S.
Venus	6 3	11 14	16 25	10 17 S.
Mars	10 44	14 32	18 20	24 5 S.
Jupiter	5 14	10 41	16 8	7 11 S.
Saturn	20 57*	4 59	13 1	21 18 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich)

Nov.	Star	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image
			h. m.	h. m.	° °
1	*d* Sagittarii	5	17 15	near approach	195 —
3	B.A.C. 7263	6	15 16	16 32	56 288

Nov.	h.	
3	23	Saturn stationary.

Variable Stars

Star	R.A.	Decl.			h. m.	
	h. m.	° ′				
U Cephei	0 52·2	81 16 N.	Nov.	3,	4 10	*m*
λ Tauri	3 54·4	12 10 N.	,,	2,	20 15	*m*
			,,	6,	19 7	*m*
S Orionis	5 23·4	4 47 S.	,,	1,		*M*
U Monocerotis	7 25·4	9 32 S.	,,	6,		*M*
S Geminorum	7 36·2	23 43 N.	,,	3,		*M*
U Ophiuchi	17 10·7	1 20 N.	,,	3,	2 51	*m*
		and at intervals of			20 8	
U Sagittarii	18 25·2	19 12 S.	Nov.	1,	0 0	*m*
			,,	4,	0 0	*M*
β Lyræ	18 45·9	33 14 N.	,,	3,	2 30	*M*
η Aquilæ	19 46·7	0 43 N.	Oct.	31,	0 0	*m*
S Cephei	21 36·6	78 7 N.	Nov.	4,		*M*
δ Cephei	22 24·9	57 50 N.	,,	3,	5 0	*m*

M signifies maximum ; *m* minimum.

Meteor Showers

The *Taurids*, radiant R.A. 60°, Decl. 18° N., continue through the present week. Slow bright meteors from a radiant near δ Arietis, R.A. 45°, Decl. 22° N. ; and meteors from a radiant in Cygnus, R.A. 348°, Decl. 52° N., are also seen at this season Fireball dates : November 1 and 5.

Stars with Remarkable Spectra

Star	R.A. 1886·0	Decl. 1886·0	Type of spectrum
	h. m. s.	° ′	
D.M. + 65° 369	3 39 3	65 10·2 N.	III.
54 Eridani	4 35 27	19 53·5 S.	III.
41 Schjellerup	4 39 24	67 57·9 N.	IV.
o′ Orionis	4 46 4	14 3·6 N.	III.
D.M. + 6° 810	4 55 39	6 29·0 N.	III.
51 Schjellerup	4 59 52	1 1·8 N.	IV.

GEOGRAPHICAL NOTES

The October number of *Petermann's Mitteilungen* begins with an article, with numerous maps, on the canal between the German Ocean and the Baltic, by Herr Beseke. It describes in detail the numerous early projects—sixteen in all—for the construction of a canal across the isthmus, and then the origin and progress of the project of a canal called *par excellence* the Imperial Canal. The last section of the paper deals with the influence of the canal on navigation, and its military and commercial advantages. But the most important paper in the number, and one of the most interesting that we have read for some time, is one on the changes wrought by man in the flora of Chili, by Dr. R. A. Philippi. Only the first part is published in the present number. These changes are enormous. The traveller in Chili, says the writer, as he passes by the fields and gardens, can scarcely believe that he is in America, not in Europe, so greatly has the native vegetation disappeared. The trees and plants of Europe, and only these (with the exception of the native potato), are found everywhere near habitations. The original native flora has to be sought with care and system, and is to be found only miles away from cultivation and the abodes of men. In fact, the native vegetation is destroyed by that of Europe as effectually as the native himself disappears before the white man. Dr. Philippi treats first of timber-trees introduced from Europe or North America, then of fruit-trees, then cereals, roots which serve for fodder, industrial plants such as the hop, sugar-cane, &c., vegetables, and finally weeds. The last class owes its origin to ordinary cultivated plants running wild, and to seeds which have found their way to Chili in other seed. Herr Wichmann concludes with a short sketch of the Galla States to the south of Abyssinia, with an elaborate map showing the routes of Cecchi and Chiarini in Southern Abyssinia between 1876 and 1881.

To the *Bollettino* of the Italian Geographical Society for September, Count L. dal Verme sends an account of an excursion to the new crater, which made its appearance during the recent eruption of Mount Etna, and which has been named Monte Gemmellaro, in honour of the distinguished geologist of Catania. It lies 300 metres below Monte Nero, at an altitude of 1500 metres above sea level, and may now be approached without much risk from the side of Nicolosi, the route passing by the Convent of Monte S. Nicola (940 metres), and thence by Monte Gervasi and to the west of Albano Pinitello and other older but now quiescent cones. There is an alternative, but much more difficult and dangerous, route further east, running also from S. Nicola straight to the foot of Monte Albano, where the explorer must dismount, whereas by the longer road he may ascend beyond Ca' dei Cervi, close to the new crater, by a bridle-path easily accessible to mules. The cone appears to rise 140 metres above the old level, and has a diameter of about 200 with a depth of perhaps 40 metres, showing at the bottom two openings 3 or 4 metres wide, ejecting a little vapour at short intervals, accompanied by a slight rumbling noise. There was a third aperture with a diameter of some 10 metres, which emitted such a continuous stream of vapour that it was impossible to form any idea of its depth. During the eruption, Gemmellaro appears to have ejected about 66 000,000 cubic metres of eruptive matter, covering a space of 5½ square kilometres on the flank of the mountain, and approaching close to the village of Nicolosi (700 metres), near the upper limit of the vine. During the eruption, which lasted twelve days, the vineyards of this district were wasted to the extent of some 20,000*l.* ; but scarcely any other loss was suffered by the inhabitants, not one of whom perished from the all-devouring stream of molten lava.

The same number of the *Bollettino* has an instructive paper by S. Raineri, on the rise and development of submarine telegraphy from the first tentative experiments of Watson (1747) and Schilling (1812) to the last cable laid on the floor of the Atlantic between Valentia and New York. The historical labours and discoveries of West, Wheatstone, Newall, Brett, and other pioneers are briefly described, and a full account given of all the oceanic cables at present girdling the globe in all directions. The paper is accompanied by two comprehensive charts and tables containing the names, dates, and lengths of all the lines completed down to the year 1885.

From the Report of the Council of the Straits Branch of the Royal Asiatic Society for the past year, we learn that an entirely new map of the Malay Peninsula is now in course of preparation, and will shortly be sent to England for publication. Recent explorations in Pahang, and the work of surveyors in the service of the native States have added greatly to geographical knowledge during the last few years, so that it has been found possible to make great improvements in the map of the Peninsula published for the Society in 1879 by Mr. Stanford. The Council further suggest as a subject worthy of the attention of the Colonial Government and of the Society, the preparation of an authorised Statistical Gazetteer, to which residents, students, travellers, and men of science may turn for authentic information regarding the Straits Settlements and the native States of the Malay Peninsula. Such a work, the Council thinks, should embody a full account of these regions, their inhabitants and productions, in the departments of geography, geology, ethnology, religion, manners and customs, history, arts, manufactures, agriculture, commerce, zoology, ornithology, ichthyology, &c., and should give a concise account of every town and village of importance within these

limits. It would carry on in the Far East the work already performed in British India and Burmah.

A SPANISH Expedition under Capt. Cervera has been exploring Adrar in the Western Sahara. Capt. Cervera describes Port Rio de Oro, where he landed from the Spanish cruiser *Ligera*, as rather difficult of entry, but, once entered, as secure from all winds, with good anchoring ground, and from 10 to 30 metres' depth of water. "Rio de Oro" is a misnomer, as there is only one well of fresh water, and that very dirty. There are, however, good wells in the interior, and at four days' journey there is a running spring. The Expedition proceeded, between latitude 22° and 23°, south-eastwards 425 kilometres through an arid country of gneiss and granite, and struck the boundary of Adrar. The population is composed of four tribes—the Uled Delim, speaking and nearly all capable of writing pure Arabic, mixed with a few words of Berber origin. These tribes are nomadic, moving their tents from well to well for the pasture of their dromedaries, goats, and sheep. The capital of Adrar is Aatar, not Wadan, as hitherto believed. Wadan lies more to the south.

"HYBRID" WHEAT

IT is probably not generally known that the cereal from which we obtain our bread corn is almost invariably self-fertilised in nature, and that only a skilful expert can perform the delicate operation involved in the cross-breeding of wheat. The anthers, when near maturity, must be removed from a number of wheat-flowers, and on the following day the pollen of the male parent must be placed on the stigma. The opening of the glumes, however, is dependent on the swelling of the "lodicules," which only occurs when the temperature of the atmosphere is not less than about 75°. Below that minimum the florets will not open so as to expose the reproductive parts to the operator. The angle of opening of the glumes corresponds to this swelling, and when fertilisation has been performed the lodicules shrivel up and the glumes again close over the pistil. It had long been obvious that half a dozen different varieties of wheat, blossoming at the same time, may be grown in adjacent fields or in contiguous rows without the occurrence of interbreeding, in spite of the clouds of pollen which sunshine and warmth develop at the time of blossoming; and considering the remarkable results from the cross-fertilisation of numerous plants in gardens, it seems surprising that the same process should not have been applied to wheat. Many years ago a well-known selector and "improver" of cereals, the late Mr. Patrick Sheriff, tried some experiments in this direction. His usual method of improvement consisted in the selection and careful cultivation of "sports," and he was approaching the end of his career when his earliest attempts at cross-breeding were made. The increased vigour of wheat, the moulding of the ear, the production of a larger and fuller ear, with superior grain, earlier maturity, and the modification of the straw so as to render it stronger, or shorter, and less liable to become laid as in the present season, are all improvements which may certainly be accomplished in regard to this cereal, just as analogous modifications have beeen effected in animals and some other plants by the recognised methods of breeders.

The wheat-crop of the United States reaches at present 50,000,000 quarters, or four times that of England, and this may in some measure account for the numerous experiments in cross-breeding by scientific American farmers, and especially by some of the professors of agriculture in the colleges of that country. The same remark applies to France, where the cultivation of wheat is relatively far more important than in England, and where the noted seed-firm of Vilmorin are now in the midst of the work of cross-breeding. But even in England, disheartened as farmers may be as regards wheat-culture, their prospects might certainly be improved if the average production of this cereal could be increased, its quality improved, and its liability to disease and injury from indifferent weather diminished. Both growers and consumers, therefore, have an interest in the undertaking of Messrs. Carter and Co., the seedsmen, who for several years past have been engaged in the cross-breeding of wheat at their trial-grounds and gardens at Forest Hill. The collection of different sorts of wheat at this establishment includes varieties from every country which exports this grain to England. Some of them are not hardy, and the wretched appearance of the growing specimens of Persian and Indian varieties was probably due to their depreciation in our climate. Some of the colonial and other sorts were excellent, but none could compare to the so-called hybrids.

The operations commenced in 1882 by the sowing of a number of the best English and American varieties, and in the following summer twenty crosses were effected by experts who are usually employed by the firm in delicate manipulations of a similar kind in connection with garden vegetables and flowers. In the following autumn the hybrids, as they are usually called for convenience, were sown between the rows of the male and female parents for the sake of comparison, and in the succeeding year the mixture of the breeds became apparent. In one plot, for example, the female parent was a short-strawed velvet-chaffed variety, and the male a very large, bearded, and tall American wheat, and the offspring attained a stature exceeding that of the former by a foot, with smooth chaff, and stout thick-set ears bearing minute awns at the apex of the chaff of each grain. This last-named peculiarity, the occurrence of defensive points in serrated order from top to bottom of the ear, may be referred to as one of the many advantageous peculiarities which have been developed in the course of the experiments, and it has gained for the new variety the appropriate name of "Bird-proof."

Another of the cross-breeds, having the earliest of English varieties, Talavera, for one of its parents, was almost ready for cutting this year on July 21, when we inspected the new sorts, a very early date in the case of a late backward harvest. Another has the grains very firmly set, and therefore not liable to shell out even when the crop is dead ripe, as it usually is before the time of cutting in New Zealand, where this wheat will probably prove popular.

Another of the crosses proved to be a wheat with shorter straw than any other variety in cultivation, and this too will prove a valuable modification, since neither soil nor season, however productive of straw they might be in certain years, could throw the crop down. Now does it surprise the experts that the offspring of two parents which are both of average height, should prove to be a dwarf in regard to the length of its straw, since they have had occasion to observe the same thing in the breeding of peas—two sorts of peas, each 4 feet high, and requiring the support of sticks, having produced a very useful seedling of 2½ feet, which requires no such artificial assistance.

We cannot attempt a detailed description of the numerous other peculiarities—some of them promising to be highly advantageous—which have been developed in the course of these wholesale experiments. But we may here observe that the most tiresome part of the business has proved to be the fixing of the types after the crossing had been accomplished. The work, however, has proved sufficiently successful to encourage the experimenters to undertake the cross-breeding of barley as well as wheat, and to lead them to anticipate a large demand for their new varieties, not only in this country, but in the colonies.

H. E.

DR. AUGUST WEISMANN ON THE IMPORTANCE OF SEXUAL REPRODUCTION FOR THE THEORY OF SELECTION [1]

IN NATURE, vol. xxxiii. p. 154, was given an article on Prof. Weismann's most interesting and important memoir on "The Continuity of the Germ-Plasma considered as the Basis of a Theory of Heredity." The present memoir also abounds with interest, and may be regarded as following naturally from the former one as a continuation and further elaboration of some of the questions raised in it. The main aim of the memoir is to establish the position that the process of sexual reproduction is the prime agent by which all the varied differentiations of the complicated phyla of the Metazoa has been brought into existence. A strong part of the argument is devoted to the establishment of the position that peculiarities acquired by the parent are not transmitted to the offspring, and to showing that the hypothesis that such acquired peculiarities are transmitted is not necessary for the explanation of the known phenomena of heredity and the mode of origin of the series of organic forms. It will be remembered that the assumption of this position forms an important and necessary factor in the theory of the

[1] "Die Bedeutung der sexuellen Fortpflanzung für die Selektions Theorie." (Jena: G. Fischer, 1886.)

continuity of the germ-plasma, but it was one which in the former memoir was only lightly touched upon.

A large part of the contents of the present memoir was delivered as a lecture at a meeting of the German Naturalists' Association at Strasburg in September 1885; but numerous alterations and additions have been made, and six appendixes on special points have been added.

At the outset, special attention is drawn to the essential difference between "those special new characters which are correctly to be included under the term 'acquired,'" and the much broader class of new characters generally. Only those new characters can be termed "acquired" the origin of which is due to external influences, and not those which depend on the mysterious collaboration of the different tendencies of heredity which meet one another in the impregnated germ. These latter are not acquired, but "inherited." If, as the author holds, acquired characters are incapable of being transmitted to the progeny by the parent, then a much wider field of action must be ascribed to the processes of selection in the transformation of species than hitherto, since the modifying influences of external conditions being confined to the individual, can, in the vast majority of instances, have no effect on the transformation of species.

After discussing Nägeli's postulation of the existence in organisms of a special internal modifying force to account for the phenomena of adaptation, and showing that such a force has no existence at all, and that there are no reasons or justification for assuming it, the author dwells on the adequacy of the theory of selection to account for the facts. He insists specially on the necessity that the changes occurring during the transformation of a species, both in the organism itself and in the conditions of existence, shall take place by the smallest possible stages with the utmost slowness, so that at no moment of the entire process of transformation shall the species remain inadequately adapted to its conditions.

The possibility of the transmission of acquired characters being excluded, it is assumed that, in the case of all animals and plants which are reproduced by real germs, only those characters can be transmitted to a succeeding generation which were already present in the germ at the time of its formation.

On the theory of the continuity of the germ-plasma, a certain minimum of the active substance of the germ, the germ-plasma, always remains unchanged when the germ develops into the organism, and this remains of the germ-plasma becomes the basis from which the germ-cells of the new organism are formed. Thus there is a continuity of germ-plasma from one generation to another, and the impossibility of the transmission of acquired characters follows directly from this position, since the molecular structure of the germ-plasma is already determined within the embryo.

There are no facts which really prove that acquired characters can be inherited, although many attempts have been made to render such a supposition plausible. On the contrary, the evidence against it is abundant. The children of highly-civilised races of mankind, when brought up in isolation, show no trace of a language, and the results of experiments made on plants tell markedly on the same side.

Twenty-two pages of appendixes are devoted to demonstrating this most important position—the influence of which on future speculation with regard to organic evolution can hardly be over-estimated—that acquired peculiarities are not hereditarily transmissible. Only some of the statements with regard to the familiar instance of Brown-Séquard's hereditarily epileptic guinea-pigs can here be given.

Brown Séquard, as is well known, produced artificially epilepsy in guinea-pigs by means of section of certain parts of the central or even peripheral nervous system in the living healthy animals. The progeny of these guinea-pigs inherited the disease of the parents. The experiments were repeated by Obersteiner, and there is no doubt of the fact. Still, urges our author, this is not to be taken as a proof that acquired peculiarities can be inherited. Epilepsy is no morphological peculiarity, but a disease.

If there were found in the epileptic offspring a distinct and evident morphological alteration in the nervous structures caused by and corresponding with that produced by injury in the parent, and which was at the same time the cause of the epilepsy in the offspring, then the question of the actual transmission of an acquired morphological peculiarity might be justly raised.

That such is the case is, however, not only not proved, but very improbable. What is certain is that many of the young of such artificially epileptic parents are small and feeble, and often die early; others show deformities and sores on various parts of their bodies. In rare instances certain of these young exhibit epileptic symptoms. Two only out of thirty-two young of epileptic parents showed the symptoms, and these soon died, having but very little vitality.

The facts may be fairly expressed by stating that the guinea-pigs artificially rendered epileptic transmit to a part of their offspring the disposition to various diseases of the nerves, to diseases of the motor nerves, and in a less degree to those of sensory nerves, and most markedly to those of trophic nerves. In rare instances, those in which paralysis is most developed in the offspring, the epilepsy is also transmitted with it.

If the pathological change in the nervous structures which follows injury and produces the epilepsy be due, as is possible, to some as yet unknown microbe growing within its substance, it is far easier to understand the transmission of such a microbe from the parent to the offspring in the adult sperm-cell or ovi-cell than to conceive of the disease being communicated in the form of a molecular change in the germ-cell. That such a transference of a microbe in the ovi-cell or sperm-cell occurs in the case of syphilis and tuberculosis is probable, and it is certain that such does occur in the case of the muscardine silkworm disease. Such an explanation would account, in the case of the guinea-pigs, for the fact that the various offspring exhibit various forms of nerve diseases, which remains unexplained if it be assumed that this is a case in which there is an hereditary transmission of a morphological character, namely, a pathological change of structure of a nervous centre. The way in which the artificial epilepsy develops itself in the guinea-pigs after the operation, creeping gradually over the body, and ensuing in the same way after injury to the most varied parts of the nervous organs, is a proof of its infectious nature. The change produced by section of the nerves is obviously not the direct cause of the epilepsy, but only serves to originate a process of disease which is propagated centripetally, with the final result of the appearance of epileptic symptoms.

The germ-plasma is immensely complex in its finest structure, but it has a remarkable power of persistence, since it absorbs nourishment and grows enormously without in the least changing its complicated molecular structure in so doing. This follows from the fact that many species (*e.g.* the Egyptian ibis or the crocodile) have reproduced themselves for thousands of years without change. Further, since the quantity of germ-plasma contained in the single germ-cell must be regarded as extremely small, and as only a minute fraction of this remains unchanged when the germ-cell becomes developed into the new individual, the growth of this fraction in the individual must be a most enormous one, as usually thousands of germ-cells are produced by it.

Since the germ-plasma can remain unchanged in molecular structure in spite of such vast increase in bulk, it is obvious that it is not easily to be modified, and it is probable that the direct influence of modifications in surrounding conditions on the germ-plasma has no effect of any importance on the production of hereditary individual variations. These must have another origin, and this, according to Weismann, is to be sought in the mode of reproduction occurring amongst most organisms at present existing—sexual, or as Haeckel terms it, amphigonic reproduction. This consists, as is well known, in the fusion of two antithetic germ-cells, or possibly nuclei only, which contain the germ-plasma, which in virtue of its peculiar molecular structure, is the bearer of the heritable tendencies of the organism from which the germ-cell originates. Thus in amphigonic reproduction two different sets of hereditable tendencies are to a certain extent mingled with one another. It is this process of mingling which is the cause of the occurrence of hereditarily transmissible individual peculiarities, and it is the production of these peculiarities which it is the office of amphigonic reproduction to effect. Amphigonic reproduction supplies the material for the development of individual variation out of which selection produces new species.

This is a most startling conclusion, and directly opposed to almost all former views on the action of sexual reproduction. The amphigonic process has been regarded as having the effect of rapidly obliterating any deviations from the specific type which may arise in the members of a species. With regard to specific characteristics this may still hold true, since the deviations from them are of so rare occurrence that they are unable to hold their own against the great mass of normally-formed

individuals. But it is otherwise in the case of the minute differences which are characteristic of the individual, because every individual possesses them, and each in a different manner. Here a compensation of the differences could occur only if the entire species were composed of but few individuals, but the number of individuals which constitute a species is in most cases practically unlimited, and thus an intercrossing of all of these with all the others with the result of the compensation of the individual differences is impossible.

The process of amphigonic reproduction is able to bring about what is absolutely essential to the working of selection as an all-powerful agent in development, namely, the summing up of the small individual differences it has to deal with in the direction of the result aimed at, and the production by this means of new characters. Such summing up of minute characters is impossible in the case of species with non-sexual reproduction. In the course of successive sexual generations the differences between the individuals of a species must continually increase, not as regards the greater differences, but as regards the constantly new combinations of individual peculiarities which are formed. Imagine a number of individuals of a species distinguished each from the others by a few hereditable peculiarities; in the next generation no single individual could be like the others, they must all be different. Further, not one of the progeny could be identical with one of their parents, since each has combined in it the hereditable tendencies of two parents, its organism being as it were a compromise between the developmental tendencies of both. In the third generation the hereditary tendencies of two individuals of the second generation are combined. But since the germ-plasma is no longer simple, but is already compounded of two individual kinds of germ-plasma, an individual of the third generation will represent a compromise between four different hereditable tendencies. In the fourth generation eight such tendencies must be combined; in the fifth, sixteen; in the sixth, thirty-two. Each of these tendencies may show its effect more or less marked in this or that part of the organism as it is developed, and thus in the sixth generation a number of the most different combinations of the individual peculiarities of the ancestors may be exhibited; combinations such as never before existed in the history of the species, and such as never can occur again.

The prepotency of the various kinds of idioplasma which compose the germ-plasma of the germ-cells of each individual must vary in intensity at various periods of its life. This must be assumed in order to account for the fact that the several children of the same parents are never exactly alike. The presence of the small individual peculiarities postulated as displayed in the hypothetical series of individuals of the first generation is accounted for as having arisen, not amongst the higher organisms, the Metazoa, but amongst their unicellular Protozoan ancestors. Amongst these there is no distinction between body-cells and germ-cells; their reproduction is by division, and in this case every modification of the body of the organism, every individual peculiarity, however produced during the course of life, must be directly transmitted to the offspring. Here parent and child are in a certain sense still one and the same being. The child is a portion of the parent, and commonly half the parent. The conditions are entirely different from those occurring in the sexual reproduction of the Metazoa, by which acquired peculiarities are not transmitted.

Hereditarily transmissible variation having arisen in the Protozoa by the direct action of external causes, was retained by the Metazoa when they became developed; and as amphigonic sexual reproduction arose at the same time, the variation became thereby enhanced and increased in complication, and preserved in ever-changing combinations.

In the theory of sexual reproduction put forward by Prof. W. K. Brooks, of Baltimore, in 1883,[1] there is a resemblance to the views here maintained, in that sexual reproduction in it also is regarded as the means which is employed by Nature to produce variations, but the mode in which the influence is supposed to act is entirely different in the two theories, Brooks attributing the main effects in variation to the inheritance of acquired peculiarities, and to what may be termed the "circulation of the germ-plasma." His theory is a modification of Darwin's "pangenesis." He assumes, like Darwin, that each cell of the body of higher organisms throws off minute gemmules, not always and under all conditions, but only when they encounter unusual circumstances of existence. Whenever the normal functions of any component cell of the body are disturbed, "it throws off small particles which are the germs or gemmules of this particular cell." These gemmules are, according to Brooks, capable of passing to all parts of the body, and may pass into an ovarian ovum or a bud, but the male germ-cell has a special attractive force for them, gathering them within itself and storing them up. It is one of the peculiar merits of the theory of the continuity of the protoplasm that it dispenses with all necessity for any hypothesis of a circulation of gemmules or germ-plasma, so complicated and difficult to understand as these hypotheses are. As soon as the inheritance of acquired peculiarities is denied, such hypotheses are not required. Brooks further differs widely from Weismann in ascribing to the two germ-cells concerned in impregnation a different *rôle* in the process, the egg-cell or conservative principle being supposed to be charged with comparatively few gemmules, the sperm-cell, or radical principle, with many. This view, that the male germ-cell has a different part to play during the construction of the embryo from the female germ-cell is considered by Weismann to be untenable, because it is in contradiction with the simple matter of observation that human children on the whole are able to inherit just as many characteristics from the mother as from the father.

Apparently, the only function of amphigonic reproduction is the production of a supply of hereditable individual peculiarities on which selection may work, and as the development of the whole higher organic world depends on these processes, the part which amphigony has to play in Nature is one of the most stupendous conceivable, and hence its wide, almost universal, distribution in the animal and vegetable kingdoms. Nevertheless, it is not in any way contended that amphigonic reproduction originally came into existence in order to render the production of species possible. The process must have been already present before it evoked the hereditable variability, and its first appearance must therefore have had another cause. This cannot at present be explained with any certainty, but the key to the problem lies in the conjugation of the Protozoa, the predecessor of amphigonic generation. The fusion of two unicellular individuals into one, the simplest form of conjugation, must have a direct and immediate action which is of advantage to the existence of the species concerned. The view taken by Hensen and E. Van Beneden and others, that conjugation, as well as sexual reproduction generally, effects a rejuvenescence of the organism, is discussed and rejected as unsatisfactory. "The entire conception of rejuvenescence has something indefinite and nebulous about it. The assumption of the necessity of a rejuvenescence of life, brilliant as it is, is scarcely to be reconciled with our other conceptions of the nature of life based on purely physical and mechanical forces. How can it be conceived that an infusorian which has at last by repeated division exhausted its power of reproduction can recover this power by fusion with another similarly effete infusorian? Twice nothing does not make one, and if it be assumed that each infusorian retains half its reproductive force, the two combined would result only in one whole, but this could hardly be termed rejuvenescence. It amounts only to an addition such as, under other conditions, would be produced by simple growth."

It is best to assume in the present state of knowledge that living matter is endowed with a power of unlimited assimilation and consequently unlimited capability of reproduction, and that the form of reproduction, whether sexual or asexual, of itself exercises no influence on the duration of the process. It has not been proved that reproduction by division can never take place indefinitely. The phenomenon of parthenogenesis is strongly against the hypothesis of rejuvenescence, for, if impregnation represents a rejuvenescence, and essentially consists in a combination of energies and materials which in virtue of their differences give rise to the development of reproductive force, it is difficult to understand how occasionally the same reproductive force can be produced by one of the two materials, only the egg-cell. The common assumption that in the case of parthenogenesis a single impregnation suffices for a whole series of generations has no grounds of support, and is at variance with the fact that the same egg which may develop parthenogenetically (in the queen bee) is also capable of fertilisation.

The primitive action and meaning of conjugation may at present be best provisionally defined as a strengthening of the forces of the organism in relation with reproduction, which occurs when, on account of external causes, such as atmosphere, temperature, and want of food, the growth of the single animal to the size necessary

[1] W. K. Brooks, "The Law of Heredity: a Study of the Cause of Variation and the Origin of Living Organisms" (Baltimore, 1883).

for reproduction is not possible. This cannot be regarded as equivalent to rejuvenescence, since rejuvenescence is a process necessary for the maintenance of reproduction, and ought to occur periodically entirely independently of external conditions, whilst according to the above view conjugation originally only presented itself under unfavourable conditions of life and helped the species to surmount them.

Amongst the higher Protozoa the original import of conjugation seems already to have dropped into the background, as shown in the change in the nature of the process itself. The higher Infusoria are only temporarily fused with one another in conjugation, and it appears possible, and even probable, that the process has here already attained the full significance of sexual reproduction, and is to be regarded as functional as a source of variability only.

Amphigonic reproduction, having existed through countless generations of Protozoa in the form of conjugation, passed over to the Metazoa, and, though its original physiological effect lost importance or retired altogether into the background, was preserved from extinction and firmly retained because of the immeasurable advantages which are conferred by it in endowing the species with the power of adapting itself to new conditions of existence. The formation of new species which was possible amongst the lower Protozoa even without amphigony, amongst the Metazoa and Metaphyta was to be attained only by that process.

Amphigony has been lost in certain cases, either partially, as in the case of some lower Crustacea amongst which parthenogenetic generations alternate with sexual, or entirely, as in the case of certain gall-insects and plant-lice amongst which parthenogenesis has become the only form of reproduction. Such restriction to parthenogenetic reproduction may act so as to secure the existence of a species for a time, but according to the views as to the origin of hereditary variability here maintained, such a species dependent on parthenogenesis alone for reproduction must be near its period of dying out, as unable to adapt itself to any new conditions of existence which may arise, since in the loss of amphigonic reproduction it has lost the capability of mingling and increasing the individual hereditable peculiarities which occur amongst its members.

This conclusion is supported by the fact that no whole groups or genera occur the species of which are entirely parthenogenetic in their reproduction.

The persistence of functionless organs in species which are reproduced parthenogenetically is a further corroboration of the general view as to the import of amphigonic reproduction here maintained. Since acquired peculiarities are not inherited, organs which fall out of use cannot become vestiges in a direct way, as has been hitherto assumed to be the case. The functionless organ becomes indeed weaker and less fully developed in the individual which does not use it, but this reduction in the organ is not transmitted to the offspring.

The explanation of the undoubted fact that such organs do become vestiges must be sought elsewhere. In order that any particular part of the body in any species may be maintained at the height of its functions, all individuals which possess this part in less completely perfect development must be excluded from participation in the act of reproduction by perishing in the struggle for existence. As soon, however, as an organ ceases to be useful, this uninterrupted selection of the individuals with the best organs for the purpose of reproduction ceases also, and a condition arises which the author terms "panmixia." Now not only the individuals with the best organs participate in reproduction, but also those with inferior ones. A mixture of all kinds of gradations in goodness and badness in the organ must be the inevitable result, and thus in the course of time a universal deterioration in the organ must be produced. The remarkable fact that the gradual disappearance of functionless organs is extremely slow appears much more in keeping with the above views as to the nature of the process than with those hitherto adopted. The effect of disease of an organ in the course of a single life is a very well marked one, and if it were transmissible even to a reduced extent direct to the offspring, the organ must become reduced to a minimum even in a hundred generations. Yet how many million generations must have elapsed since the whalebone whale abandoned the use of its teeth?

If this new view of the cause of the reduction of disused organs is assumed as correct, it follows that vestiges of organs can occur only amongst species with amphigonic reproduction, not amongst those with parthenogenetic reproduction only, and this appears to be the case. Superfluous organs do not become rudimentary in species parthenogenetically reproduced. As far as the author's investigations extend, the receptaculum seminis does not become aborted in such species, although it is entirely without function. Thus in Chermes, which is without males, the receptaculum seminis is present in the females unchanged; whilst, on the other hand, in Aphis, another plant-louse in which amphigonic reproduction is not extinct, but alternates regularly with parthenogenesis, the receptaculum seminis has become lost in the summer female.

These evidences in favour of the general views here expressed form, of course, no absolute proof of their correctness, but only give evidence in favour of their probability. Further evidence cannot be offered at present, the phenomena dealt with being extremely complicated and their explanation being such as can only be approached gradually.

The author, however, considers that he has plainly shown that the selection theory is by no means incompatible with the conception of the "continuity of the germ-plasma," and further, that as soon as this conception is accepted as correct sexual reproduction appears in an entirely new light, displays a reason for its existence, and becomes to a certain extent comprehensible.

H. N. MOSELEY

N.B. A criticism of Prof. Weismann's above theories by Prof. von Kolliker appears in the *Zeitschrift für Wissenschaftliche Zoologie*, in the October part just issued.

THE FUNCTION OF A UNIVERSITY[1]

I GRATEFULLY accept the honour with which I have been invested thus publicly, and with such kindly feeling. In the future, as in the past, I shall give you my best endeavours.

By a fortunate accident it has happened that I am not called upon to speak to you on behalf of the University of California before I know somewhat of it. The six months which have elapsed since I entered upon the duties of the high office which I now hold, I have utilised in studying with minute care the University in its main features and in some of its details. This is not the place nor the time to speak of minor matters. It is, however, the very place and the very time for me to say to this audience that no friend of the University has any cause to fear, so long as all of us, Regents, Professors, and students alike, remain united in the future, as we are at this present moment, in a single effort toward the same high ends.

For nearly a hundred years the American Republic lived, and grew, and prospered, and the community of nations hardly knew her, and barely gave her place. So, on a smaller scale, it has been in California. The University here has lived, and grown, and prospered, and the communities outside of our own small circle hardly know us, and grant us our place reluctantly. It is a perfectly safe prediction that within the next twenty years, possibly within the next ten, the State of California will find suddenly that here in her midst she has a force on which she never reckoned—a reserve on which she never counted.

It is easy to see what advantages would come if this conviction were now wide-spread and firmly held. It is easy to seek feverishly to make ourselves quickly known, in order that we may be more widely useful. But, I am more and more convinced that if we are always ready, like a strong man, our opportunity will be here almost before we realise it.

Vital Points and Fundamental Principles.—What, then, are the aims upon which our eyes must be ever fixed, and toward which our energies must be ever directed? I will not name them all, nor count them over one by one. But I think that I can point out certain vital points that must be guarded; certain principles that must be fundamental. Let us consider the demands which the community makes of the University, and again, the standards which the University should set for its individual members.

In the first place we must carefully examine what it is that we, as citizens, demand from the University. We must see to it that what we demand is consonant with what we ought to demand. If we find that we disagree with what seems to us to be the position of the University, ought we not in fairness to calmly inquire which of the two is right? Is there not at least a certain presumption that the efforts of a body of intelligent scholars cannot be all misdirected? The usual and careless way of meeting this question, even on the part of those who count

[1] Inaugural Address by President Holden at the University of California.

themselves firm friends of the University, is to pass over these differences of opinion lightly, and to lay them to the errors of the intelligent scholars themselves. "What does a college professor know about life?" we say; "he knows his speciality, his mathematics, his political economy, his physics. Let him keep to that and we are satisfied. But let us, who are engaged in the practical business of life, judge of life and its needs."

We, the faculties of the University, might admit this provisionally and for the sake of peace, and inside our web here at Berkeley go on spinning our theories and trusting to their truth for their commanding influence in the future of the State. We will do that most certainly, and if the theories are right they will prevail. If they are wrong we shall be brought to confusion.

The University Useful and Practical.—But we claim more than this. We claim that the University is one of the most useful, and in a high sense practical parts of the machinery of the State. It has a function as important, or more important than any other. It shares this function with the Church, and the voices of both are to be your guide. The chief and highest function of the University is to assert and perpetually prove to you that general principles—laws—govern man, society, nature, life, and to make unending war on the reign of temporary expedients.

Think how fundamental is this use of the University. Think in how many ways we accomplish it. In the lecture-room, in the laboratories, in the machine-shop, we bring the student face to face with history, with nature, with fixed qualities. In history, in philosophy, in politics, in physics, we see that definite causes produce their definite and inevitable results. In the laboratories we find that nature, candidly interrogated, gives unambiguous answers; that mathematical prediction,—the modern prophecy—is inevitably fulfilled in experiment. In the machine-shop we learn that the hard results in brass and iron will not lie, but that they point relentlessly to careless, shiftless errors, if they exist, or testify to faithful, honest, laborious work if it has been done.

General Principles against Temporary Expedients.—There is no day and there is no hour of the student's life that he is not brought face to face with results, and taught to see that these flow from principles of universal application. Just in so far as a teacher can bring forth this great truth is he a successful teacher. Just in so far as a graduate has learned it, is the work of the University priceless to him. Just in so far as our professors and our students alike go forth into society and proclaim and prove the unending reign of general principles and the utter folly of putting temporary expedients in their place, is the University of prime value and use to the State. There was never a period or a country in which the reign of fundamental law needed more constant assertion and more perpetual proof than in our own period and our own country. All our modern inventions which give quick locomotion and quick transmission of thought tend to exalt the temporary expedient and to debase the general principle. The merchant of old time sent his ships to the Indies with their orders for two years or more; the diplomatist in a foreign country was separated by weeks or months from instructions by the Foreign Office. Now it requires but an hour to reach the uttermost parts of the earth. We have cable despatches which recount the doings of the King of Dahomey. The merchant changes his orders in Bombay as he reads the morning paper; the Secretary of State arranges the affairs of Tuesday on the afternoon of Monday.

The immediate and harmless effect of all this is to paralyse continuous effort based on sound belief, and to substitute a wavering policy of daily temporising. But the living danger is that society may come to permanently distrust the reign of laws. Recollect that we have to train our young men to appreciate this vital truth in the midst of a society where there are apparently many glaring exceptions to the rule; in a society where wealth has come, it seems by accident, and where power seems not to have been gained by work. And when you remember this, remember also how deep and profound your gratitude should be to any institution which is by its traditions and its very nature devoted to the incessant announcement and to the perpetual proof of the fundamental truth of all life here and hereafter, namely, that it is governed by unchanging principles which cannot be evaded nor shirked, and that a national or a personal life built on the expedients of the day, like a house built on the sand, will inevitably come to ruin. When this truth is grasped and firmly builded into the character, then it is that the steam-engine and the telegraph and all the myriad inventions of the day first become truly useful. The man who can command them aright has his powers doubled and trebled. It is the highest use of the University to train such men.

President Gilman's Test of a University.—You must not for one moment forget that the power of a university lies in its *men*. In its governors, its professors, and in its students. If you come here to our beautiful grounds and see them fair as they are today and always, if you see fine buildings and many of them, if you find our laboratories stocked with costly apparatus and our libraries with splendid books, you must not for that reason suppose you have a university fitted to the needs of the State. You are to inquire about far other things. And it is of prime importance that every citizen should know exactly what questions to ask. Nowhere have these questions been more eloquently or more pregnantly put than in a splendid address recently delivered by our former President, Dr. Gilman, at the noble University over which he now presides: "Remembering that a university is the best organisation for the liberal education of individuals and the best organisation for the advancement of science, apply the double test—what is done for personal instruction, and what is done for the promotion of knowledge?—and you will be able to judge any institution which assumes this name.

"Ask, first, is it a place of sound education? Are the youth who are trained within its walls honest lovers of the truth? Are they learned, are they ready, are they trustworthy? When they leave the academic classes do they soon find a demand for their services? Do they rise in professional life? Are they sought for as teachers? Do they show aptitude for mercantile, administrative, or editorial life? Do they acquit themselves with credit in the public service? Do the books they write find publishers? Do they win repute among those who have added to the sum of human knowledge? Have they the power of enjoying literature, music, art? Can they apply the lessons of history to the problems of our day? Are they always eager to enlarge their knowledge? Do they become *conservative* members of society, seeking for progress by steady improvements rather than by the powers of destruction and death? Are they useful, courteous, co-operative citizens in all the relations of life? Do the charities, the churches, the schools, the public affairs of the community receive their constant consideration? Are there frequent manifestations among them of unusual ability in science, in literature, in oratory, in administration? As the roll of the alumni increases, and the graduates are counted by hundreds and not by scores, does it appear that a large proportion are men of honourable, faithful, learned, and public-spirited character? These are the questions by which, as the years go on, a university is to be tested; or, to sum up all questions in one, Is it proved to be a place for the development of manliness?"

It is to be noticed that the stress is laid upon one chief thing—manliness—and that two main questions are to be asked. What does the University do for personal instruction? and what does it do for the promotion of knowledge?

The answers to these questions will depend in every case upon the men whom the University has chosen as its teachers. It will depend not only on their intellectual attainments, but upon their personal characters. It is a most fortunate thing that the following out of a life of true devotion to learning brings, in so vast a majority of cases, the excellences and beauties of character which we desire and look for. We can all point to eminent examples of this in our midst—and it is so everywhere. We should see in a true University the true spirit of research kept alive and eagerly active. How can you teach a young and ardent mind by means of examples culled from books alone? The vast panorama of nature lies before us, glorious by day, resplendent by night, and it is only from the actual pursuit of knowledge at first hand from nature itself that true teaching power is to be derived. Mere information can be gained from books and libraries. True knowledge must be attained by studies that develop mind and character at once.

The Genuine Issues of Life.—How, then, are we, the faculties of this University, to send forth from our midst men and women who are genuine, true, high, noble, sincere, simple? Men and women whose natures are such, and whose training has developed, harmonised and rounded out their natures? We must be constantly on the watch to put the genuine issues of life before ourselves and before our pupils. We are constantly tempted to put the name for the thing. How hard it is to avoid this even in our personal conduct, and how doubly hard it is in

our public acts to point always to the essence, and not to indicate the accidents. We, all of us, suffer from the complexity of modern life which presents masses of detail, demanding attention, and distracting our imaginations with what seems to be but "a multitude of single instances." We lose the thread of logic and law and only grasp the tangled skein of various issues. We are prone to class quickly a man, an action, a belief, and have done with it and him. It seems to save our time. In reality it dissipates and degrades our life. Let u take a familiar example. We meet a man for the first time. Our friend classifies him for us. He is introduced to us as a man of affairs, a physician, or a lawyer. We accept the crude classification based on what he *does*, and we forget the divine possibilities of what he *is* or may be. We indolently accept a commercial classification and omit the reckoning with all the unknown possibilities within a human being. We do him an injustice, and we go on to dull our own minds and souls by repeated iterations of this stupid act until we become puppets meeting our like and not men meeting with our fellow-men. It is a lazy and a shiftless way, and unworthy of all of us. It inevitably dulls the mind by putting a word before a thought, a phrase before a principle, and this process ceaselessly repeated gradually eliminates all thought, and living men become mere dead automata in each other's eyes. Hardly any man is so dull that there are not possibilities unknown to you within him. To classify him at a glance and by a phrase is to deny the divine spark in him and a perceptive sense in your own heart. It is true that the higher a man's profession is the nearer should his life approach to the type signified by the name of that profession. It is safer to think you know somewhat of a poet when you hear that he is such than to predict the quality of a physician from his degree of doctor.

Complete Human Beings.—The University has no higher ideal than to train its students so that their practice may agree with their professions. But their complete professions are by no means signified by the formal degrees with which we invest them.

We grant the degree of A.B. to successful candidates. But A.B. does not really stand for what we have tried to teach. What we wish to teach our students and ourselves is to be complete human beings. Nothing *less* than this. There *can* be nothing more. We wish them to be H.B.'s first—human beings—and A.B.'s afterwards. Let any one of us try to see what is meant by a deserved title such as this. What *is* a human being complete in every way? Is there a manly virtue, is there a feminine grace, is there a divine aspiration which we can conceive to be lacking to such a personality? How carelessly we use the phrase, and with what debased significance! Is the man who has sacrificed his very nature to the service of money deserving of the title? Is it any better if power be the thing he has sold his birthright for? or vanity? or pleasure? or fame?

The moment we reflect upon the inner senses and upon the connoted meaning of the word, we see how we have debased it. We are used to lift a beggar from the ditch and to say with a pity that is half repulsion—at least he is a human being. But when we reflect we see that we can give no higher praise than this to the men who are the chiefest glories of the race. Think of David, King of Israel. How can we praise him, appreciate him, feel his power over us at this day, better than to recognise that he was a complete being human in every part? That is allied directly with Divinity. St. Peter, Socrates, the great Emperor Marcus Aurelius—these touch us through a chord of complete unison of their human natures with ours. What is it that is common to the great Alfred of England and to the poet who sang the beauties of the daisy overturned by the plough? What but this human nature that embraces our own and harmonises with its every part?

In America, young as we are, we have had our complete human beings. We can point to the oration at Gettysberg and know that the man who wrote it did so out of the fulness of a complete human nature. The soldier whose forces were overcome on that fearful field will live in history by his martial deeds, but he will be cherished in our hearts for the rounded symmetry of his humanity. In fiction we have all given this high degree. Where can we find more perfect examples than Col. Newcome or Henry Esmond? Is it not worth reflection to see *why* it is that these stand for us as types of what a man can become?

I think we can conceive of what our ideal of a human being should be by seeking to find the common quality of men so removed from each other in character and circumstance as King David, Peter the Apostle, Socrates, Alfred, Burns, Lincoln, Lee. The great Marcus has even defined such a man for us in formal words: he is "a man who delays not to be among the best, like a priest and minister of the gods; who uses the deity planted within him, which makes him uncontaminated by pleasure, unharmed by pain, untouched by insult, a fighter in the noblest fight, not overpowered by any passion, deep-dyed with justice, and accepting with his soul all that happens and is his portion."

It must be our aim and end to fix clearly in the mind of every pupil that the whole object of his college course should be one and the same as the whole object of his entire life, namely, to be a real human being. Not to strive for partial knowledge, for partial facts as an end, and finally to be graduated a Bachelor of Arts, but to strive for complete and utter manhood and to add to its magnificent qualities all the learning which our schools afford simply as a help towards carrying out his inmost and his highest aspirations. Each one of us should be ever striving to deserve among our fellows and in our most secret life this chief of all titles. The one that expresses the sum of all achievement possible to us; since when it is attained it fixes us as wholly human, and thus made in the image of divinity. The best title of our Master was the Son of Man, and He descended to this to show the term to which we might attain.

A Word to the Graduates.—And now, members of the graduating class, I wish to say one parting word especially to you, who are soon to see the formal signs of the approval of your professors and of our governors—the Regents.

In the name of the University I welcome you to your new estate. If we have done our duty by you, you are equipped for the beginning of your maturer life. If you do your duty by yourselves and by society there is nothing which you need fear to undertake. Here, on the very borders of the most western sea, in a golden land of promise, let me repeat to you the noble words which were first written down eighteen hundred years ago, in the midst of a savage wilderness, in the presence of hostile barbarians, by the hand of the greatest and most virtuous of the rulers of imperial Rome, sitting alone and silent in his soldier's tent. Let these great sentences be at once our farewell and our God-speed to each one of you:—

"If thou workest at that which is before thee, following right, reason calmly, vigorously, allowing nothing to distract thee, but keeping thy divine part pure, although bound to give it back immediately—if thou holdest to this, expecting nothing and fearing nothing, with heroic truth in every word and sound which thou utterest, then thou wilt live happy—and there is no man able to prevent thee."

ANTARCTIC EXPLORATION[1]

THE author began by referring to the results established by Gauss in 1839. Gauss proved: "(1) That the knowledge of Y (the west component of the horizontal component of terrestrial magnetism, called usually X) over the whole earth, along with the knowledge of H (the north component of H) at all points on a line running from one pole to the other, is sufficient for the foundation of a *complete* theory of the magnetism of the earth. (2) That a finally complete theory was also deducible from the simple knowledge of Z (the component of the earth's magnetism, that is directed to the earth's centre) on the whole earth's surface." There existed, for a large part of the earth's surface, data for large charts of the normal values of the declination of H and of Z, at the epoch 1880, from which X, Y, and Z could readily be deduced. These charts were accurate for the zone lying between 60° N. and 50° S. lat. (except for some parts of North Asia and of Central Africa); they were less accurate for 60° to 70° N. lat., and 50° to 60° S. Beyond these limits in the south, lay regions almost unvisited since Ross's Expedition in 1840-43; so that the charts were correspondingly weak in those latitudes. The charts show that the *Challenger* crossed the Antarctic Circle about the meridian 79° E. These and other somewhat recent observations made between 50° and 60° S. lat., show that considerable changes in the magnetic elements have occurred since Ross's time, and therefore the charts for 1880 cannot be com-

[1] "On the Advantages to the Science of Terrestrial Magnetism to be obtained from an Expedition to the Region within the Antarctic Circle." Abstract of a paper read at the Birmingham meeting of the British Association by Capt. Ettrick W. Creak, R.N., F.R.S.

pleted, especially as our knowledge of the changes is too limited to permit of the use of Ross's observations. Further it is desirable to have actual verification of Gauss's extension by theory of the magnetic elements at places where they are known to places where they are unknown. The position of the south magnetic pole is still undetermined, and magnetic observations are wanted from 40° S. to the geographical pole. For the carrying out of these views, Melbourne Observatory, being furnished with the necessary instruments, would serve admirably as a base station, with subsidiary bases at the Cape, and at Sandy Point, Magellan Strait, for the use of portable absolute instruments. Much of the survey could be done on board ship at sea, observations having now become so trustworthy by the process of "swinging ship." Portable instruments could also be used on *ice*, where their readings would be specially free from sources of error. The great effect of the ship's iron in high latitudes can be got rid of by experience, as proved in the voyage of the *Challenger*, an important matter being a proper position on board far the instruments. This position could be chosen immediately after the selection of the ship. The error in the vertical component varies with the "heel" of the ship; the horizontal error can be eliminated by the process of "swinging."

NOTES FROM THE OTAGO UNIVERSITY MUSEUM

VIII.—*On the Claspers of Callorhynchus*

IN my friend Mr. Patrick Geddes' able article, "Reproduction," published in the recently issued twentieth volume of the "Encyclopædia Britannica" (ninth edition), the following note occurs (p. 410):—

"In the curious Holocephalous fish, *Callorhynchus*, Jeffery Parker has recently adduced arguments for regarding the claspers as the surviving rudiments of a third pair of limbs."

As this passage was written under the mistaken impression that a somewhat wild theory, hither only communicated privately to one or two friends, had been published, I think it will be advisable to state exactly the grounds on which I am disposed, provisionally, to consider the Holocephali as the only existing hexapodous vertebrates.

In the Elasmobranchii (Plagiostomi) the male is well known to possess a single pair of so-called claspers, each having the form of a plate rolled longitudinally upon itself so as to produce an incomplete tube, and supported by a more or less complicated cartilaginous skeleton continuous with the basipterygial cartilages of the pelvic fin.

In *Callorhynchus*, on the other hand—and I believe the same applies to *Chimæra*, of which I have no specimen—the male has two pairs of organs, which may be called respectively the anterior and the posterior claspers. The posterior claspers are evidently homologous with the claspers of the Elasmobranchs: they occur in the same position, have the form of a plate rolled longitudinally into a tube, and are supported by a prolongation of the basipterygium. No doubt, like the corresponding organs in sharks and rays, they have an intromittent function.

The structures I call anterior claspers are situated a short distance cephalad of the vent, inclosed, in the position of repose, in a pouch of skin having a somewhat contracted slit-like aperture, so that the clasper is ordinarily hidden from view. The aperture of the sac is erroneously marked "peritoneal aperture" in Günther's figure of *Chimæra colliei* ("Study of Fishes," p. 184). In connection with the sac is a gland secreting a lubricating fluid, and closely resembling the well-known gland of the Elasmobranch clasper.

In the female, although the clasper itself is absent, a small glandular sac occurs in the corresponding position.

The anterior clasper itself is a somewhat complicated organ, consisting of three chief parts supported by cartilage. The largest of these, which forms the main support of the whole structure, is a strong irregular cartilaginous plate, articulated by an elongated surface with the anterior border of the pubic portion of the pelvic girdle in such a way that when in its ordinary position of retraction, the whole apparatus is folded back in the hollow furnished by the outer surface of the pubic cartilage. To this principal cartilage of the anterior clasper are attached two others: one a thin delicate plate, shaped like the human external ear, the use of which is not obvious; the other a somewhat thicker plate, rolled upon itself to form a tube, in much the same way as the posterior clasper, and evidently serving as a duct for the passage of the above-mentioned secretion. The whole apparatus is covered with soft mucous membrane, except the free portion of the principal cartilage which is studded with minute sharp denticles.

The clasper is exserted by the action of a strong muscle arising from the inner face of the pubic cartilage and passing over its anterior border to be inserted into the principal cartilage of the clasper. The plane of movement of the organ is nearly horizontal.

That a serial homology (homoplasy) exists between the anterior and the posterior claspers is suggested by the following facts:—(*a*) The general similarity of their structure; (*b*) they both articulate, mediately or immediately, with the pelvic cartilage; (*c*) they both lie in the line of Balfour's lateral ridge, *i.e.* of the hypothetical lateral fin; (*d*) the blood from both appears to be poured into a vein which is clearly the representative of the lateral vein of Elasmobranchii, which latter I have adduced reasons for considering as the vein of the proto-vertebrate lateral fin (*Trans. N.Z.Inst.*, vol. xiii. p. 413, and vol. xv. p. 222; *Proc. Roy. Soc.*, June 1886).

I regret that all my efforts to obtain earlier embryos of *Callorhynchus* than those I described three years ago in NATURE (vol. xxix. p. 46) have failed. It must therefore remain for future investigations to decide whether the anterior clasper of Holocephali is developed from a portion of the lateral ridge which usually atrophies, and whether its skeleton is formed by the concrescence of pterygiophores (radial fin-cartilages).

At present, therefore, the hypothesis that the anterior claspers of the Holocephali represent a middle pair of limbs is nothing more than a deduction from an unproved theory. I should not have ventured to publish it without further evidence if my friend had not, quite inadvertently, forced my hand.

Dunedin, N.Z., August 16 T. JEFFERY PARKER

SOCIETIES AND ACADEMIES

SYDNEY

Linnean Society of New South Wales, July 28.—Prof. W. J. Stephens, M.A., F.G.S., President, in the chair.—The following papers were read:—On some new or rare fishes from the Australian region, by E. P. Ramsay, F.R.S.E., and J. Douglas-Ogilby. A few notes are given on the curious Blennoid genus *Xiphasia* of Swainson, and a description is given of a species—*Xiphasia setifer*, Swainson—now for the first time taken in Australian waters. *Arrhamphus sclerolepis* and *Gastrotokeus biaculeatus* are recorded as fishes not previously seen on the New South Wales coast.—Catalogue of the Australian Coleoptera, part 5, by George Masters. The present part contains the large family of the Curculionidæ, numbering over 1200 species, and bringing the total number now catalogued up to 5625. It was stated that probably two more parts would complete the order Coleoptera.—Miscellanea Entomologica, No. 2: the genus *Liparetrus*, by William Macleay, F.L.S. This is a complete monograph of the genus *Liparetrus*. All the old species are redescribed, many new ones added, and the genus is subdivided into several sections and sub-sections. Altogether about 100 species are characterised.—Revision of the Australian Lepidoptera, No. 1, by E. Meyrick, B.A., F.E.S. Five families of the Macro-Lepidoptera, or Lesiadæ, Arctiadæ, Hypsidæ, Syntomididæ, and Zygænidæ, are monographed, numbering about 150 species, about half that number being new to science.—Notes on synonymy of Australian Micro-Lepidoptera, by E. Meyrick, B.A., F.E.S. The synonymy of fifteen species of Micro-Lepidoptera is corrected, from an examination of specimens in the British Museum.

PARIS

Academy of Sciences, October 18.—M. Jurien de la Gravière, President, in the chair.—A theory of the unequal flow of gases, by M. Haton de la Goupillière. Although geometricians have already solved a few questions relating to the unequal movement of fluids, no general theory appears to have yet been applied to the subject so far as regards the gases. The object of the present paper is to make good this want, and to present a complete solution of the problem in connection with the receptacles of compressed air for locomotives or tramways filled from reservoirs maintained by the compressing engines at a constant tension.—Researches on the tension of the dry bicarbonate of ammonia, by MM. Berthelot and André. The authors here discuss the important and complex problem, whether the tension of ammonia in the air, the ground, and natural waters, and its movements between these various mediums, is to be assimilated

to the tension of this free alkali dissolved in pure water and to its diffusion between an inert atmosphere and purely aqueous solutions. Numerous experiments here described show that the function of the three constituents of sal ammoniac is not the same in its dissociation, the carbonic and ammoniac gases, even in great excess, possessing no perceptible influence on the tension of the bicarbonate at the ordinary temperature, while liquid water determines its decomposition independently of the laws of dissociation of the salt.—On the origin of the motor nerves in the palate of the dog, by M. Vulpian. His experiments on the origin of the secreting nerves of the salivary glands and of the chord of the tympanum have led the author to the study of this subject, his conclusions mainly confirming the results already obtained by M. Chauveau, who operated on the horse and the ass.—Experimental researches on the cause of *rigor mortis*, by M. Brown-Séquard. In continuation of his previous paper (see last week's NATURE, p. 612), the author describes certain experiments, which seem to show that this phenomenon mainly depends on a contraction, that is, a muscular vital act beginning or continuing after the general extinction of life. At the same time it is not denied that a coagulation of the albuminous substances may also to some extent contribute to the rigidity which sets in immediately after death.—The mountain plants of the Parisian flora, by M. Chatin. It is shown that associated with the ordinary vegetation of the Paris district are found numerous highland and even Alpine specimens, such as *Swertia perennis*, *Atropa Belladonna*, *Euphrasia lutea*, *Digitalis lutea*, *Veronica montana*, &c., most of which are largely represented in the Alps and Scandinavia, and some few in the south of France. They generally flourish in localities where the conditions of life approach nearest to those presented by the Alps; but whether they are due to migration from those regions or are indigenous is a question reserved for future discussion.—Analysis of some cosmic dust which fell on the Cordilleras near San Fernando, Chili, by M. A. E. Nordenskjöld. This specimen, received last February from M. Stolp, of San Fernando, and weighing about 2 grammes, yielded on analysis: oxide of iron 74·59, oxide of nickel 6·01, silicic acid 7·57, magnesia 3·88, alumina 2·90, and minute quantities of lime, phosphoric and sulphuric acid, with traces of oxide of copper. This analysis shows that it is not a product of the Krakatão eruption, but comes for the most part from the inter-planetary spaces.—On surfaces inclosing cones of the second degree, by M. E. Blutel. The case is considered in which each cone touches its inclosure following a moving circle.—On the determination of the coefficients of expansion by means of the pendulum, by M. Ch. Ed. Guillaume. It is shown that this method, recently proposed by M. Robert Weber, cannot possibly yield the accurate determinations anticipated by him. All the apparatus needed for its application render the process extremely complicated, while under the most favourable conditions its precision will never exceed 1/300.—Theoretical value of the local attraction at Nice, by M. Hatt. Theoretic researches undertaken for the purpose of determining this value have led to the results here communicated, including some data tending to correct the geodetic latitude of Nice.—On some pyridic bases, by M. A. Ladenburg. Having two years ago determined the methods of synthesis for the pyridic and piperidic bases, the author has since succeeded, with the aid of MM. Roth, Lange, and Hesekiel, in preparing a whole series of these bases, which are here described.—Researches on the evolution of the embryo of the fowl, when the eggs are submitted to incubation in the vertical position, by M. M. Dareste. Of sixteen eggs treated in this way, only one was successfully hatched, all the other embryos perishing at various dates and under diverse conditions.—On the relations of the Leptocephalidæ to the conger-eel, by M. Yves Delage. Observations recently made at the Laboratory of Roscoff show that the Leptocephals, contrary to the opinion of Günther, are normal larvæ capable of transformation.—Contribution to the study of the Tertiary flora of the west of France and of Dalmatia, by M. Louis Crié. It results from this comparative study that five identical species and seven or eight closely-related types connect in a common palæophytic epoch the Tertiary districts of Mans and Angers in France with that of Monte Promina in Dalmatia.—On the discovery of a grave dating from the polished Stone Age, recently discovered near Crécy-sur-Morin, by M. A. Thieullen. The excavations carried out at this spot revealed two contiguous chambers built under a rock, and containing the skeletons of about thirty human beings of all ages and sexes, besides stone hatchets, knives, scrapers, and other relics of the Neolithic period, but no traces of pottery or the metals. The human remains are remarkably well preserved, five or six of the skulls being almost intact, and by their form apparently indicating the presence of two distinct prehistoric races.—On a meteorite found in a block of Tertiary lignite from Wolfsegg, by M. Gurlt, with remarks by M. Daubrée. This rare specimen of a meteorite, traced to Tertiary times, forms a mass of cosmic iron combined with some carbon and nickel, weighing altogether 785 grammes.—On the constant presence of micro organisms in the thermal waters of Luchon (64° C.), and on their action on the production of baregine, by MM. A. Certes and Garrigou. The object of this paper is to determine the presence of living organisms in thermal waters of the highest temperatures, to ascertain their nature and the part played by them in the production of the baregine or glairine commonly found in sulphurous waters.—On melanosis, a disease of the vine, by MM. Pierre Viala and L. Ravaz. This is described as a malady of American origin, not very injurious to French vineyards, and due probably to a parasitic fungus identical with the *Septoria ampelina* described by Berkeley and Curtis.

BOOKS AND PAMPHLETS RECEIVED

"The Functions of the Brain," new edition, by Dr. D. Ferrier (Smith, Elder, and Co.)—"Journal of Royal Microscopical Society," October (Williams and Norgate).—"Experimental Chemistry," new edition, by C. W. Heaton (Bell and Sons).—"Origin of Languages," by H. Hale (Cambridge, Mass.).—"Concerning Force, Impulsion, and Energy," by J. O'Toole (Hodges, Dublin).—"On the Temperature and the Rainfall of the Croydon District, 1881-85," by H. S. Eaton.—"The Mechanism of Nature," by A. M. Stapley (Cornish, Manchester).—"Bibliotheca Historico-Naturalis et Mathematica: Lager Catalog" (Friedländer, Berlin).—"Notes from the Leyden Museum," vol. viii., No. 4, October, by Dr. F. A. Jentink (Brill, Leyden).—"Natural History," by Dr. H. A. Nicholson (Chambers).—"Diseases of Tropical Climates," by Dr. W. C. Maclean (Macmillan).—"Proceedings of the Royal Society of Edinburgh," Session 1885-86, No. 121.—"Carte Géologique Générale de la Russie d'Europe," Feuille 139.—"Bulletins du Comité Géologique St. Pétersbourg," Nos. 7 and 8, 1886.

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